NISI DOMINUS FRUSTRA

# ALPHABETICAL LIST

OF THE

# CONTRIBUTORS OF ORIGINAL ARTICLES.

---

ANDERSON, KEITH, M.D., late Resident Physician, Royal Infirmary, Edinburgh.

BALFOUR, GEORGE W., M.D., F.R.C.P.E., Lecturer on the Practice of Medicine, Edinburgh.

BEGBIE, J. WARBURTON, M.D., F.R.C.P.E., Physician to the Royal Hospital for Sick Children, Edinburgh.

BELL, CHARLES, M.D., F.R.C.P.E., Edinburgh.

BONNAR, GEORGE L., M.D., Surgeon to the Fife County Prison, Cupar.

BRYDON, JAMES, M.D., Hawick.

BUCHANAN, GEORGE, M.D., F.F.P.S.G., Surgeon to the Royal Infirmary, Glasgow.

CRAIG, WILLIAM, L.F.P.S.G., Ayr.

CUTHILL, JAMES, M.D., Denny, Stirlingshire.

DOUGLAS, A. HALLIDAY, M.D., F.R.C.P.E., Physician to Chalmers' Hospital, Edinburgh.

DUNCAN, JOHN, M.D., F.R.C.S. Ed., Edinburgh.

DUNLOP, ANDREW, M.D., South Shields.

FAYRER, JOSEPH, M.D., F.R.C.S. Ed., First Surgeon and Professor of Surgery in the Bengal Medical College, Calcutta.

FOX, EDWARD LONG, M.D., M.R.C.P., Physician to the Royal Infirmary, Bristol.

FOX, TILBURY, M.D., London.

GAMGEE, ARTHUR, M.D., Assistant to the Professor of Medical Jurisprudence in the University of Edinburgh.

GILLESPIE, JAMES D., M.D., F.R.C.S. Ed., Surgeon to the Royal Infirmary, Edinburgh.

HANDYSIDE, PETER D., M.D., F.R.S.E., Lecturer on Anatomy at the Royal College of Surgeons, Edinburgh.

HJALTELIN, JOHN, M.D., Inspecting Medical Officer of Iceland.

HOWDEN, THOMAS, M.D., Haddington.

INGLIS, ANDREW, M.D., F.R.C.S. Ed., Edinburgh.

KEITH, THOMAS, M.D., F.R.C.S. Ed., Edinburgh.

KENNEDY, J., Surgeon-Major, Trichinopoly.

LAYCOCK, THOMAS, M.D., F.R.C.P.E., Professor of the Practice of Medicine in the University of Edinburgh.

LINDSAY, W. LAUDER, M.D., F.R.S.E., Physician to the Royal Murray Institution for the Insane, Perth.

MACADAM, STEVENSON, Ph. D., F.R.S.E., Lecturer on Chemistry, Edinburgh.

MacGregor, Donald, L.R.C.P. & S. Ed., House Surgeon, Barnhill Hospital, Glasgow.

M'Intosh, W. C., M.D., Medical Superintendent, Perth District Asylum.

MacLaurin, H. N., M.D., Assistant-Surgeon R.N., Greenwich Hospital.

Main, William, M.D., Lasswade.

Mair, R. S., M.D., F.R.C.S. Ed., Madras.

Millar, John, M.D.

Mitchell, Arthur, M.D., Deputy Commissioner in Lunacy for Scotland.

Moir, John, M.D., F.R.C.P.E., Consulting Physician, Royal Maternity Hospital, Edinburgh.

More, James, M.D., Rothwell, Northamptonshire.

Murchison, Charles, M.D., F.R.C.P., Assistant Physician, Middlesex Hospital, London.

Roberts, John Rhiwen, M.D., Castell, Bangor.

Rorie, James, M.D., Dundee.

Rutherford, Daniel, M.D., the late, Professor of Medicine and Botany in University of Edinburgh.

Rutherford, William, M.D., Assistant to the Professor of Physiology in the University of Edinburgh.

Sanders, William R., M.D, F.R.C.P.E., Physician to the Royal Infirmary, Edinburgh.

Sheriff, Thomas, L.R.C.P. & S.Ed., Ratho.

Skae, Francis, M.D., Edinburgh.

Smith, G. Stevenson, L R.C.S.Ed., Resident Medical Officer, Royal Hospital for Sick Children, Edinburgh.

Smith, John, M.D., F.R.C.S.Ed., Surgeon-Dentist to the Royal Infirmary, Edinburgh.

Spence, James, F.R.C.S.Ed., Professor of Surgery in the University of Edinburgh.

Stephenson, William, M.D., F.R.C.S.Ed., Extra Physician to the Royal Hospital for Sick Children, Edinburgh.

Stewart, T. Grainger, M.D., F.R.C.P.E., Pathologist to the Royal Infirmary, Edinburgh.

Strachan, John M., M.D., Dollar.

Syme, James, F.R.S.E., Professor of Clinical Surgery in the University of Edinburgh.

Thomson, James B., L.R.C.S.E., Resident Surgeon to the General Prison for Scotland, Perth.

Turner, John, M.D., Dumfries.

Turner, William, M.B.Lond., F.R.C.S.Ed., Senior Demonstrator of Anatomy in the University of Edinburgh.

Warden, Thomas, M.D., Assistant-Surgeon, R.N.

Watson, Patrick H., M.D., F.R.C.S.Ed., Surgeon to the Royal Infirmary, Edinburgh.

Welch, Francis H., M.R.C.S.E., Assistant-Surgeon, 22d Regiment.

Williamson, Thomas, M.D., F.R.S.E., Leith.

## ORIGINAL COMMUNICATIONS.

ARTICLE I.—*A Clinical Lecture on Pneumonia, delivered fifty-three years ago.* By the late DANIEL RUTHERFORD, M.D., Professor of Medicine and Botany in the University of Edinburgh.[1]

BETTY M'KENZIE, æt. 19.—There is little ambiguity in the nature of this girl's complaint, as every symptom seemed to point to a state of inflammation of her breast, to a state of pneumonia. She felt an acute pain in her left side about the eighth or ninth rib, while she had a considerable degree of fever ; and at the same time she had a frequent cough with considerable expectoration, which was sometimes mixed with or tinged by blood. These are the common symptoms that occur in such a disease. The pain was increased by any considerable motion of the thorax, as by a full inspiration, by the cough, and it is said when she stooped. It is also remarked that she could not lie on the left side, that which was affected, by reason of increase of pain which this posture occasioned. Such is not commonly observed to be the case ; on the other hand, a person shall lie with greatest ease on the side that is affected, because in that posture the motion of the ribs is in a great measure restrained, so there is no play of the lungs upon the inner surface of the containing parts, the same parts always remaining contiguous, which

---

[1] The following lecture was delivered on the 14th July 1812. It is printed verbatim from the manuscript of the author. Dr Rutherford appears to have been in the practice of bringing under the notice of the class all the cases admitted into his wards, and of writing out in full all his clinical lectures. —ED. ED. MED. JOUR.

must save greatly from dilaceration parts which have begun to con-
crete from the effect of inflammation. But when the person cannot
lie on the affected side without increasing the pain of it, and often
to a great degree, we conclude that this proceeds from the inflam-
mation being more external, or being situated in the intercostal
muscles and superficial parts of the thorax, rather than in the
interior parts, as the pleura and lungs; for in this case the pain
arises purely from compression of the parts that are inflamed.
From this view of the matter it will appear probable that the
inflammation in our patient tended rather to the external than to
the internal parts, though from her account it appears that it had
actually extended to the substance of the lungs, as we may con-
clude from her expectoration being sometimes mixed with blood.
There is not anything besides remarkable in the case. Her pulse
is said to beat 130 in a minute. This is a great degree of celerity;
though I have no doubt that it might have been so, as she seemed
to be very irritable; but its velocity was considerably diminished
next day when we first saw her. Though she was very thirsty, her
tongue was clean, so it often is in such a complaint; her skin was
hot, but without any moisture, as it commonly is in any consider-
able inflammation of the breast. Her complaint had come on
gradually, I suppose with rigors and sickness, as is said, about two
days ago. So a pleurisy often begins; but in general there is at
the same time felt the pain, which afterwards becoming more violent,
gives the chief mark of the disease. We have no notice of the
cause of her complaint, though it is probable that they might begin,
as is mostly the case, from exposure to cold when the body chanced
to be overheated. Indeed, from what is observed in the case, it is
probable that the disease had not begun with the obvious marks of
a pleurisy, as it is observed that she had taken an emetic, which, I
think, would hardly have been administered to a person ill of a
pleurisy before some blood had been taken away. However, she
received no advantage from such a remedy.

From the nature of the disease, there must always be a consider-
able degree of danger; and the more so, indeed, the longer the
disease has continued. Every circumstance that affects the lungs
or impedes their action, must necessarily be attended with disad-
vantage; we may easily then conceive that in this disease, where
they can hardly act from the pain that is thence induced, or from
their capacity being greatly diminished, and also from the con-
comitant degree of fever, there must be immediate danger, and
surely the greater the more the strength of the body has been
reduced by the continuance of the disease. Hence, I was well
aware how great a risk this girl ran, and that the only means of
lessening the risk was by endeavouring to abate the degree of
inflammation as speedily as possible.

The surest means of reducing inflammation is by taking away a
quantity of blood rapidly, so as at once to lessen the force of the action

of the heart and arteries.  It is surprising how great benefit may accrue
from one bleeding when it is properly performed.  I have known when
the person had been seemingly in the most perilous hazard from pain
and difficulty of breathing, that his breathing became quite free, and
all pain was entirely removed, by his losing less than a pound of blood
in this way.   But if bleeding be performed, as is too often the case,
slowly or through a small orifice in the vein, so as perhaps to trickle
down the arm, little benefit will be obtained, the pain and difficulty
of breathing will continue, so that recourse must be had to the same
operation again, and with perhaps no better consequence than
before ; and seeing that the disease will not yield but by detraction
of blood, the bleeding is repeated for an indefinite number of times
till the disease shall fortunately appear to give way, or, which is as
likely to happen, till the strength of the person be completely ex-
hausted.   Whereas, if the blood flows very quickly in a very short
time, we shall be sensible of its good effect by the diminution of pain
and other disagreeable symptoms.   Indeed, it is only by this effect
of the bleeding that we can judge properly what quantity of blood it
may be necessary to take away.   I should always advise that the
blood should flow till we find a sensible alleviation of the symptoms
has taken place.   That may happen when eight or ten ounces have
been taken away, or perhaps not till eighteen or twenty, or even
more, have flowed.   There is little risk of faintishness being pro-
duced in such a disease, and more especially as the person is com-
monly in a reclining posture.   Often, as I have hinted, the disease
is quite subdued by a single bleeding ; but it frequently happens
that after a short interval the pleuritic symptoms all again attack
the person, or the disease was lulled but not removed.   In such
cases we must just have recourse to bleeding a second time in the
same manner as at first, and so indeed should I advise if the
disease should again increase with any degree of violence, though I
have very rarely, almost never, known it resist two bleedings, if
they had been properly administered.   This may seem to some of
you to be a new doctrine, who may have heard of the propriety of
repeatedly bleeding a person in a pleurisy, and of the enormous
quantities of blood they may lose, such as 80 or 100 ounces perhaps.
I have before said why I suppose such a loss of blood seemed to
be necessary to those who ordered it ; and can only declare that
when blood is hastily made to flow, as I have recommended, I
never have seen an instance where it was necessary to take above
the fourth part of such a quantity.   I have known physicians, for
whose opinion I have had a respect, who seemed to mark the
violence of the disease by the quantity of blood they had caused
to be taken away, and really seemed to plume themselves by hav-
ing ordered the loss of 100 or. 120 ounces.   I imagine that had
they taken equal care of the rapidity with which the blood issued,
as of the quantity of it, they might have been satisfied with a
smaller detraction of blood.   Sure I am, I can safely recommend to

you the plan I have proposed.   Draw the blood quickly by a large orifice, till the person shall feel relief, and you will probably find that there will be no farther occasion for bleeding.   Thus, you may effectually remove the disease, which is surely all that can be required; while, at the same time, you will greatly save the patient's strength.

But when we have ordered blood to be drawn, still it may be prudent to use every precaution against the disease being again renewed, or against the pain of the side being again excited. Nothing has so great effect for this purpose as topical blistering; and, therefore, it will always be prudent in a severe case to order a blister to be applied over the place where the pain was fixed.   By this I have often remarked a complete cessation of pain.   The expectoration seemed to be promoted, and the disease, indeed, was completely removed.   These are the chief remedies in the case of pneumonia, and which are indeed more or less required in every case, and which must be more or less had recourse to, according to the severity or permanence of the disease.

But there are other things which must also claim our attention, both with regard to medicines and also with regard to the regimen of the sick.   It may be necessary to endeavour to promote diaphoresis by the skin.   I formerly took notice that in this disease the skin is apt to be parched.   This is an unnatural state, and happens only in consequence of the operation of the disease.   It had better, therefore, be corrected.   At the same time I must remark that in this disease any medicines which act with a considerable degree of violence are absolutely improper.   Such would tend rather to augment the disease than to be of any particular advantage.   Therefore we are under the necessity of using only those which rather tend to cool the body and at the same time to moderate the circulation.   Such is the quality of saline ammoniated mixture or of saline medicines in general.   It is likewise of the greatest consequence that there be regular evacuation from the bowels; this can be obtained either by injections, which are preferable in a strong disease, or by laxatives taken by the mouth.

With respect to the regimen, it is highly necessary that the patient be placed in a situation where he can enjoy free air for respiration. This ought to be particularly attended to, for we often find that patients are not only enclosed in very confined rooms, but are even precluded from free air, by drawing close the curtains of their beds and every means that shall prevent a free circulation of air.   Nothing can be more prejudicial than such mistaken attention.   The freer the air is around them so much the better, and so much more comfortable will feel the patient.   Where we have the choice of situation, the person should be laid in a large airy room, and never have the curtains of the bed drawn close.   As to diet, this must be very spare; indeed, generally, the person has no great appetite for food. But drinks should be plentifully administered, but of the thinnest or most innocent kinds, such as toast-water, barley-water, and such like.

Or to induce a person to drink, and at the same time to tend to cool the heat of the body, it may be proper to add some of the acid fruits.

Such are the circumstances that are chiefly to be held in view in treating pneumonia.  Many others may occur in particular cases, which will require appropriate remedies ; but of these I cannot speak, unless we had instances of them occurring in our particular patients.

With respect to our patient who has given occasion to the preceding remarks : as soon as I heard of her particular situation, I immediately advised that she should have about ℥xiv. of blood from the arm, and that she should at the same time get saline mixture also.  I ordered some mucilaginous mixture, with an addition of syrup of lemons, to ease her cough.  The bleeding was performed with great advantage, as the pain of the side greatly remitted, and even though, as she said, she passed the night very badly.  You might remark the appearance of the blood that was taken away.  The coagulum was, as usual in this complaint, covered with a thick crust of fibrine, owing to its having coagulated very slowly, by which the red particles had the opportunity of sinking towards the bottom before the mass concreted ; but the coagulum was by no means much contracted,—not nearly so much as it often shall appear to be in inflammation of the breast.  There, sometimes, it shall be drawn together into a very small bulk, with a hollow in it just like the hollow of a cup.  It exhibits such appearance chiefly, I believe, when the pleura and surface of the lungs are the peculiar seat of the disease.  However, it is always to be carefully remarked, that the blood has a very different appearance according to the velocity with which it has issued from the vein,—for what I have said relates only to the blood that has flowed with rapidity, whereas if it issues very gently or trickles down the arm, it will show little or no crust at all.  This we have often occasion to remark in what has been taken at the same bleeding : in one cup the blood shall have a considerable crust, in the next cup the blood that had flowed immediately after the former shall show no such appearance ; while, perhaps, there shall be again a conspicuous crust on that of the third cup.  This diversity of appearance depends upon the freedom with which the blood issued from the vein, or indeed, in some cases, from the person's having grown faintish during the time of bleeding.  As to our patient, she complained next day so much of pain in her side,—though I firmly believe that she did complain much more than there was any good cause to do, for her breathing was perfectly easy, and her pulse was not at all hard,—that I ordered her to be again blooded, though not to the same quantity as before, only to ℥viij.  Indeed I hesitated whether to bleed her or not ; surely, I should not have ordered any bleeding from the ostensible symptoms.  These were all very favourable.  I did so only from her having expressed that she felt great pain in her side just as she had done before she was bled originally.  It was the

first time I had seen her, so I could not pretend to judge of her particular manner. Had I known her as well as I did afterwards, I dare say I should have trusted to what I saw myself rather than to any report she might make. However, there was no harm resulted from the bleeding farther than perhaps reducing her strength a little. The blood had nearly the same appearance as that of the first bleeding, and so it might have had had she been bled even a third or a fourth time, only the coagulum would have been more tender, or, as I have expressed, it would not be contracted.

But, for the relief of the pain she described, I thought that a blister upon the part might probably be found the most effectual remedy. This I should have ordered at any rate, whether she was blooded or not; and surely such an application must seem peculiarly proper, as I have represented the inflammation to have occupied principally the more external parts. I had no doubt that by the operation of the blister she might be relieved; so we had good reason to believe had actually been the case, for next day we found that her pulse was both considerably softer than it had been, and that it was not quicker than natural. The cough was very inconsiderable, and she spit up with it : her breathing was neither frequent nor laborious, although it is much more under command than the other circumstances I have taken notice of, yet she complained of great pain in her breast, and pretended she could hardly speak. Indeed, I am persuaded it was really a pretence, or that the girl was disconcerted by the pain which the blister had occasioned. From her behaviour this day, I was satisfied that she had given a very false representation of her feelings formerly ; and, indeed, the more I saw of the girl, the more was I convinced that there was no dependence to be had on the accounts she might give. Her disease I considered as nearly gone, notwithstanding her accounts of pain ; and I was glad that I had discovered a sure means of satisfying her that she could not complain unreasonably without subjecting herself to certain pain ; for just the day after, when all the symptoms were either gone or very much on the decline, she still pretended that she yet felt a great degree of pain in her breast. I immediately ordered another blister to be laid upon her breast, when we heard nothing more of the kind. She said, indeed, that she had passed a sleepless night, and felt pain through her body and in her limbs. All this might be true, as I dare say she might fret much in consequence of the pain caused by the blister ; but her pulse and tongue both indicated that she was free from disease, and this appeared more strongly by the heat of the body being moderate or natural. She was, indeed, somewhat costive ; however, this could be corrected by an injection. Yet, as she had passed the night before very badly, I ordered an antimonial anodyne draught to procure sleep. It had the desired effect. Nothing required any attention but the state of the bowels, which were set to right by

laxative pills, and then by laxative electuary. She complained of nothing but weakness; such might have been expected to occur from such a disease at any time, and no doubt it was more remarkable in her by reason of the repeated drains she had been exposed to. By degrees, however, her strength was repaired. She could not, however, desist from making frivolous complaints. These I generally disregarded, or might perhaps order some simple medicine against them. When, however, she began to complain again of pain in her side, and which she said had distressed her for several days,—though there was nothing whatever that indicated any such affection,—I immediately ordered that a blister should be applied over the part which she said was pained; and this, as I expected, soon banished the pain. But two days after, she pretended to be distressed with pain at the point of the sternum. What should have induced the girl to feign complaints of this kind I know not; but that she did feign I was certain. To punish her, I immediately ordered another blister to be applied to the point of the sternum. This blister, it may be concluded, was efficacious in relieving from all pain; indeed, she made no farther complaint. Having let her stay in the house a few days till the blistered part was healed up, I took the opportunity of dismissing her on the 11th.

Notwithstanding the seeming irregularities from the girl's misrepresentation of her feelings, this case shows well the nature of the complaint. It was, indeed, at first violent, though by no means to so great a degree as often we have occasion to remark. We likewise may deduce from it the great benefit that accrues from rapid venesection; for by the first bleeding there is reason to think that the force of the disease was greatly subdued; indeed, so much so, that I believe it might have been unnecessary to take away any more blood. However, the application of the blisters over the seat of pain effectually removed it altogether. And we thence also may learn the great advantages of topical blistering in such complaints.

---

ARTICLE II. — *On some Congenital Deformities of the Human Cranium.* By Wm. Turner, M.B. (Lond.), F.R.S.E., Senior Demonstrator of Anatomy in the University of Edinburgh.

In this communication it is my intention to record some unusual and interesting examples of congenital deformities of the human cranium, and to discuss the causes which have led to their production.

1st, *Scaphocephalus.* — Anatomists have for some years back directed much attention to the influence which a premature union of the cranial bones along one or more of their sutural margins may exercise on the production of certain unusual forms of skull. So

long as the membranous or cartilaginous material in which the cranial bones are developed remains unossified, an increase in the dimensions of the skull may take place. But when, in a given locality, the ossification of this material has been completely effected, and the cranial bones united along their sutural lines, then the growth of the skull in that locality is impeded, so that, as was first clearly enunciated by Professor Virchow,[1] when premature union takes place along the whole or a part of a line of sutures, then the growth of the skull will be retarded in the direction perpendicular to the line of synostosis, and diminished length, or breadth, or height, as the case may be, will be occasioned.

At the meetings of the British Association, held at Newcastle and Bath, in 1863 and 1864, I brought before the Physiological subsection communications in which I discussed the above proposition, and illustrated it by various examples.[2] At the Newcastle meeting I especially selected in illustration of the effects of premature synostosis of the sagittal suture, a remarkably elongated and laterally compressed cranium, to which attention in this country was first directed by Dr Minchin[3] of Dublin, and for which the eminent anatomist Von Baer[4] has suggested the name of Scaphocephalus. Several specimens of this form of skull, contained in the museums in this city, were described, and a case was alluded to of its existence in the head of a gentleman personally known to myself. In all the specimens the parietal eminences were absent, the sagittal suture was entirely obliterated, and the parietal bones were continuous with each other across the middle line. In discussing the cause which had led to the production of this form of skull, I brought forward various facts and arguments which supported the opinion entertained by Professors Virchow and Welcker,[5] that the sagittal suture had been obliterated very early in life, that the parietal bones had become continuous with each other, and that the growth of the skull in the transverse direction had consequently been impeded, and that in order to accommodate the growing brain, a remarkable antero-posterior elongation of the cranium had taken place.

Since the publication of my memoir, two elaborate essays on the same subject have appeared, one by Professor Gustaf von Düben of Stockholm,[6] the other by the accomplished English archaeologist Dr John Thurnam,[7] both of whom support the view that a prema-

[1] Gesammelte Abhandlungen, p. 891.

[2] On Cranial Deformities — Scaphocephalus. Natural History Review, January 1864. On Cranial Deformities—Trigonocephalus. Natural History Review, January 1865.

[3] Contributions to Craniology. Dublin Quarterly Medical Journal, vol. 22, p. 350. 1856.

[4] Die Makrokephalen im Boden der Krym und Österreichs. St Petersburgh, 1860.

[5] Untersuchungen über Wachstum und Bau des Menschlichen Schädels. Leipsic, 1862.

[6] Medicinskt Archiv. Stockholm, vol. 2, part 1, p. 1. 1864.

[7] Natural History Review, April 1865.

ture synostosis in the sagittal region is the cause of the abnormality. In both these essays various additional examples are adduced. In the Baron Von Düben's paper, four crania and the heads of three living persons possessing the features characteristic of the scapho-cephalic skull are described and figured. These specimens are apparently from natives of Sweden. Dr Thurnam, in addition to furnishing a tabular view of the crania, principally European, described by Minchin, Von Baer, Welcker, and myself, records other examples mostly occurring in the crania of the Negro, the Australian, and the Esquimaux.

In this communication I am desirous of putting on record two additional examples of scaphocephalus: one occurring in a living person, the other in a skull in the Museum of Natural History in the University of Edinburgh, which I have recently examined. The first of these I met with, a short time ago, in a young gentle-man, a native of Scotland, and is a very well-marked specimen of its class. The head measured 9 inches in length. The sagittal ridge was strongly pronounced, and the flattening of the skull on each side of the ridge was considerable. The great length of the skull was principally due to a bulging backwards in the occipital region, and there was no marked projection of the forehead. From the account of himself which this gentleman has furnished me with, there can be no question that the characteristic shape of his head was congenital, for it was remarked from his earliest infancy, and his birth was attended with considerable difficulty. This case affords an additional illustration of what I contended for in my former paper, viz., that those who possess scaphocephalic crania, and in whom, consequently, the brain has undergone a marked modification in its form, do not necessarily exhibit intellectual deficiences, for the gentleman to whom I am now referring is of studious habits and very intelligent. For it must be remembered that although in these cases the expansion of the brain in one direction, viz., the transverse, is impeded, yet compensation for this obstruction is provided for by increased growth in the antero-posterior direction. Hence the cubic capacity of these crania is not below that of the mean of the race or races in which they have been found. The capacity of the scaphocephalic skull, 117a,[1] in the Anatomical Museum of the University (the only one the internal dimensions of which I have been able to obtain) is as high as 108 cubic inches.

The cranium from the Natural History Museum of the Univer-sity (for permission to describe which my thanks are due to Pro-fessor Allman) has formed for many years a part of the collection, and is interesting not merely from its shape, but from the locality whence it was derived. It is the skull of an Egyptian mummy, and was described and figured as such by the late Mr Andrew

---

[1] Figured in Natural History Review, p. 95. January 1864.

Fyfe in his illustrations of human anatomy.[1]   He states that " it is remarkable not only for its length and narrowness, but for the strong impression made by the temporal muscle, and for the sharpness of the arches of the forehead and occiput ;" but he says nothing of the condition of the sagittal suture, and apparently regards the skull as normal, and as a characteristic specimen of the ancient Egyptian cranium.   A glance at the skull, however, will at once show that the remarkable length and narrowness are conjoined with an absence of parietal eminences ; with a complete obliteration of the sagittal suture ; with the existence of a ridge or keel along the sagittal line, and with such a perfect blending of the two parietal bones, that but a single biparietal bone exists, characters which at once pronounce it to be a scaphocephalic cranium.

FIG. 1.[2]
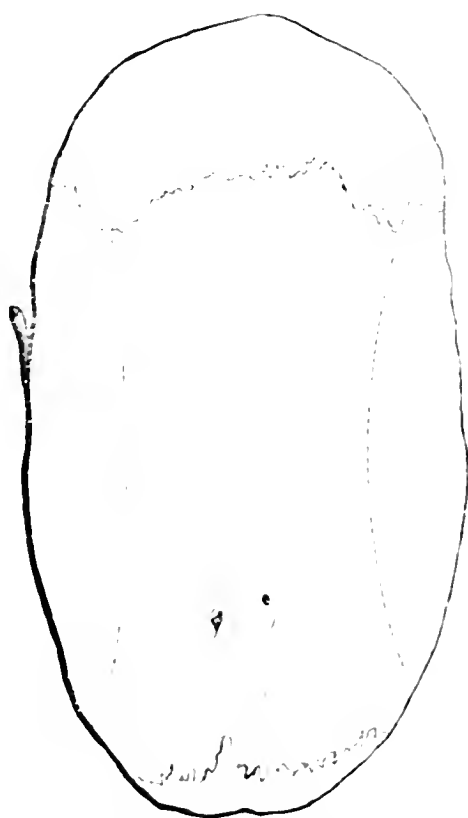


Hence, this skull cannot be regarded as indicative of the normal form of head of the ancient Egyptian.   Its length and narrowness are merely individual peculiarities, due to premature closure of the sagittal suture, and possess no ethnological value.   For there can be no doubt from the number of cases, either of skulls or living

---

[1] Table 7, A, fig. 5, and B, fig. 1, p. 18 and 18a : Edinburgh, 1814.   P. 8, Table viii. fig. 1 : Edinburgh, 1830.   The skull-cap, fig. 6, Table 7, A, is also scaphocephalic.   It is now in the Anatomical Museum of the University, and was described and figured by me in the Natural History Review, p. 98, fig. 3, January 1864.

[2] Fig. 1.   Norma verticalis of the scaphocephalic Egyptian cranium.

persons, upwards of 40, which have now been recorded, and from the various races to which they have belonged, that the scapho-cephalic skull (which most probably owes its origin to causes operating during intra-uterine life), may occur in individuals, both males and females of any race, and of any clime, as well in the ancient Egyptian as in the Scotchman of the present day.[1]　Now, that the attention of anatomists in this and other countries has been directed to the subject, I have little doubt that additional cases will be recorded, and that the abnormality will turn out to be not so unusual a deformity as might at first sight be supposed.

In addition to the more general and diagnostic characters possessed by this Egyptian cranium, which I have just described, there are others to which it may be as well to refer.　The skull is apparently that of a person past the middle period of life, but whether male or female it is not very easy to decide, though probably the latter.　The bones of the face, and in part also the basis cranii, including the cribriform plate of the ethmoid bone, have been broken away, and the interior of the skull has been filled with a hard, black, bituminous material, to the presence of which the bones probably owe their dark brown appearance.　The lateral longitudinal and vertical transverse lines of sutures are all marked externally, but most probably obliterated internally.　The forehead is rounded and projecting in the region of the frontal eminences ; but from these tubera backwards to the coronal suture the frontal bone has much the same ridge or roof-like aspect which the biparietal bone presents in its sagittal region.　The coronal suture is very undulating, but its lateral part is almost vertical, and a line drawn vertically downwards from the mid vertex point of that suture falls 1·2 inch in front of the centre of the external auditory meatus.　Hence, a large share of the elongation of the cranium is due to the increased antero-posterior growth of the biparietal bone, causing a projection forward of the entire frontal bone.　The biparietal bone has no beak jutting forward into the frontal bone, and differs therefore from the crania, 117 and 117a, described in my former paper.　On each side of the sagittal ridge, about the usual position of the parietal foramina, are some small orifices which probably represent those apertures.　The biparietal bone presents at its vertex traces of a radiated appearance similar to that described by Dr Minchin in two of his specimens, but which none of the crania formerly examined by me exhibited.　The supra-spinous occipital squama bulges posteriorly, giving a rounded form to the

---

[1] These cases have been recorded by the following anatomists :—Sandifort, Blumenbach, and Von Baer, each one ; Virchow and Lucæ, each two ; Minchin, three ; Welcker, four ; Baron Von Düben, seven ; Thurnam, nine ; and myself (including the two cases described in the text), eleven ; and they have been found in English, Scotch, Irish, French, German, Danish, Swedish, Croatian, Illyrian, Tartar, Gentoo, Esquimaux, ancient Egyptian, Negro, and Australian heads.　I have given a brief account of the two cases described above, in the Proceedings of the Royal Society of Edinburgh, May 1, 1865.

occiput, and instead of terminating superiorly in an acute angle, as is usual in well-formed crania, the two halves of the lambdoidal margin form a line, approaching the semicircular, and send from close to their place of junction a narrow beak forward into the biparietal bone, which evidently corresponds in position to the posterior fontanelle.   From the comparative narrowness of the parietal and squamoso-temporal regions of the cranium, its greatest transverse diameter is at the mastoid processes, which structures slope downwards and outwards, in a very decided manner.

The following are some of the principal measurements of this skull, expressed in inches and tenths :—

> Extreme length, 8·1 ; breadth (mastoid). 4·8 ; height, 5·3.
> Greatest frontal breadth, 3·9 ; parietal, 4·3 ; occipital, 3·6.
> Fronto-nasal radius, 3·9 ; frontal, 4·8 ; parietal, 4·7 ; occipital, 4·3.
> Longitudinal arc, 16·5 ; frontal, 5·4 ; parietal, 6·2 ; occipital, 4·9.
> Frontal transverse arc, 12·6 ; parietal, 12·0 ; occipital, 10·6.
> Circumference, 21·3.

In my remarks on the mode of production of this scaphocephalic skull (and if admitted for this specimen, it must be granted for others of a similar form), I have assumed that the fusion of the two parietal bones into one had occurred during intra-uterine life, and probably, indeed, had taken place at an early period in the growth of the embryo.   I am led to this conclusion by the total absence of any indications of parietal eminences either in this or in the other well-pronounced scaphocephalic crania which have come under my observation, and, as is well known, the parietal tubera form strongly-marked eminences in the human head at the time of and even for sometime before birth.   But one occasionally meets with crania in which the sagittal suture is more or less completely obliterated, and in which the parietal eminences are present, though not perhaps in so strongly marked a degree as in a normal skull.   I do not include amongst these crania, those in which, from the marks of age they may exhibit, the obliteration is evidently senile ; but I refer to skulls belonging to young persons of both sexes.   In my former paper on scaphocephalus (note, p. 104), were recorded some crania of this description, and I have since then met with others, which seem to me to give additional confirmation to the theory for which I then contended, that the biparietal bone had originally possessed two distinct centres of ossification.   One of these specimens consists of the skull-cap of a male, æt. 22, and is in the museum of my friend Mr Thomas Annandale.   The sagittal suture is completely obliterated, and the biparietal bone presents along the middle line a well-defined carina.   On each side is a fairly-marked parietal eminence. There is no beak projecting into the frontal bone.   Excepting the upper part of the lambdoidal suture, which is obliterated, the rest of the sutures are unossified.   The other specimen is in the Anatomical Museum of the University of Edinburgh, No. 85.   It is the skull of a girl, æt. 18.   The sagittal suture is completely oblite-

rated, except for a little more than an inch at its anterior end.  The parietal eminences and the left parietal foramen are distinct; the other sutures are unossified.  The lambdoidal suture is almost semicircular in form.[1]

These crania may, then, be regarded as forms transitional between the normally-developed skull and the true scaphocephalus, for, whilst they exhibit increased elongation, yet the lateral compression is not so great as to give them the characteristic boat-shaped form.  This description of skull has also been recognised by Professor Von Düben (page 34, and plate 3; fig. 46 and 49), and numerous examples have been described and tabulated under the appropriate name of subscaphocephali, by Dr John Thurnam in his paper already referred to.  In these crania the blending of the two parietal bones had undoubtedly not taken place until a much later period than in the true scaphocephalus.  The development of each bone independently from its own centre had gone on for a considerable period, and as a consequence the growth of the head in the transverse direction had not been so markedly interfered with.  It may be that the obliteration of the suture had not occurred until after birth, or until a late period of intra-uterine life,—a supposition which is rendered not improbable by the size of the parietal eminences.

(*To be continued.*)

---

ARTICLE III.—*On the Definition of Insanity considered Medico-Legally.*  By JAMES RORIE, M.D., etc., Dundee.

(*Read before the Dundee Medical Society, 1st March 1865.*)

HAVING agreed to furnish a paper for this meeting of our Society, I have now to offer the following remarks " on the definition of insanity considered medico-legally."  In my choice of this subject I have endeavoured to consult your interest as well as my own; and as there are few amongst us who will not be called on sooner or later to give an opinion as to the sanity of an individual, I trust the subject will not be altogether devoid of interest.  I need not add, that in this paper I do not expect to give you any additional information on the subject, but merely to bring under your notice a question which deserves at the present time every consideration.  No one who has paid the slightest attention to the conflicting evidence afforded by the medical witnesses in any criminal trial where the verdict has hinged on the question as to the mental condition of the prisoner, will deny that in such evidence there is generally expressed so great a difference of opinion that it has now become quite questionable whether in such cases medical

---

[1] As I furnished Dr Thurnam with measurements of these crania, and as they have been incorporated in his table (9, 11, 12, 15), it is unnecessary for me to reproduce them here.

evidence is of greater value than that afforded by an ordinary lay witness. Moreover, when we consider the comparative want of confidence with which medical evidence in such cases is frequently received by the legal profession, it is surely of the utmost importance that we should inquire whether or not our medical knowledge and experience in this question are really of such importance as to enable us to protect those suffering from insanity from the consequences of their insane acts, and to release them from the penalties of the law.

At the same time it will be equally important if we can by any means arrive at the cause of the different opinions entertained by the legal and medical professions on the subject of insanity ; for in possession of this knowledge we shall be the better able to supply what is required of the medical profession by our legal brethren.

In the investigation of this subject, therefore, it will evidently be necessary, *first*, not only that we have a thorough knowledge of the manner in which insanity as a medical question is viewed, but also, *secondly*, that to a greater or less extent we should be acquainted with the ideas on the subject which our legal brethren entertain.

But let us first consider the definition of insanity viewed as a mere abstract question. Numerous definitions of insanity viewed abstractly have been given ; but none, so far as I know, that is universally applicable or satisfactory. As an example of one out of many may be adduced that given by Locke, "that it is a disposition to associate ideas incorrectly, and to mistake them for truth ; hence," he says, "madmen err as men do who argue right from wrong principles." Now, however applicable such a definition may be to a case of pure monomania or delusional insanity, it is clearly quite inapplicable to an ordinary case of mania or dementia, for in the former case the mental faculties may be so convulsed as to be incapable of associating ideas, while in the latter, in extreme examples, it is questionable whether ideas are formed at all. The truth is, from this point of view, insanity is quite indefinable, it being equally easy to define abstractedly bodily disease.

But when we come to the legal definition of the term we find the case altogether different.

Legally, a person is regarded sane until he is proved insane ; and when his sanity is called in question, it occurs chiefly with regard to his civil capacity, his civil responsibility, and his criminal responsibility. It is from the last of these that we will endeavour to arrive at the legal apprehension of the term.

According to the laws of England, a person *non compos mentis* is either *an idiot*, a *lunatic*, or *a person of unsound mind*, and these terms have been thus defined :—

" An *idiot* is a person whose mind, from his birth, by a perpetual infirmity, is so deficient as to be incapable of directing him in any matter which requires thought or judgment.

" A *lunatic* is a person ' qui gaudet lucidis intervallis,' and who

sometimes is of good and sound memory, and sometimes *non compos mentis.*   And

" A *person of unsound mind* is one who, by reason of a morbid condition of intellect, is as incapable of managing himself and his affairs as an idiot or a lunatic, but not being an idiot or a lunatic, or a person merely of weak mind."[1]

It is also to be borne in mind that, according to the laws of England, " in legal phraseology, a person whose moral feelings are perverted is not by reason of such perversion a person of unsound mind."   Further, " If the mind is unsound in one subject it is not sound on any subject, the mind being indivisible."—(*Phillips*).

These definitions have been given in full, partly because in the Act of Parliament passed 29th July 1862 (25 and 26 Vict., cap. 54), intituled " An Act to make further Provision respecting Lunacy in Scotland," a change has been introduced in the definition of the term " lunatic," which by no means lessens the responsibility to be incurred by medical practitioners in granting certificates of insanity. In the Act (20 and 21 Vict., cap. 71) immediately preceding that above mentioned, passed 25th August 1857, a " lunatic" is declared to " mean and include any mad or furious or fatuous person, or person so diseased or affected in mind as to render him unfit, in the opinion of competent medical persons, to be at large, either as regards his own personal safety and conduct, or the safety of the persons and property of others or of the public."   Whereas in the Act first referred to (25 and 26 Vict., cap. 54), a lunatic " shall mean and include every person certified by two medical persons to be a lunatic, an insane person, an idiot, or a person of unsound mind,"—adopting, to a great extent, it will be observed, the terms used in English law, and leaving the practitioner to discover their legal meaning, with this somewhat important difficulty, that the definitions established by the laws of England cannot, I think, be employed to interpret similar terms in Scotch law.   But although it is held by the English law that if the mind is unsound on one subject it is unsound on all, there are certain limits marked out by both the laws of Scotland and England, which require that the unsoundness must be of such a character and extent as to destroy the person's responsibility before it can act as a bar to punishment ; and this amount of insanity, so to speak, is tested by the person's ability to distinguish right from wrong.   How far this is a proper test may be afterwards considered.   Thus, Blackstone, in his Commentaries on the Laws of England, in referring to the cases which excuse from guilt (after mentioning infancy) states that, " The second case of a deficiency in will, which excuses from the guilt of crimes, arises also from a defective or vitiated understanding, viz., in an idiot or a lunatic.   For the rule of law as to the latter, which may readily be adapted also to the former, is, that ' furiosus furore solum punitur.'   In criminal cases, therefore, idiots and lunatics are not

[1] Phillips' Law of Lunatics, Idiots, and Persons of Unsound Mind.   1858.

chargeable for their own acts if committed under these incapacities; no, not even for treason itself."[1]

But he adds elsewhere, that " if there be any doubt whether the party be *compos* or not, this shall be tried by a jury; and if he be so found, a total idiocy or absolute insanity excuses from the guilt, and of course from the punishment of any criminal action committed under such deprivation of the senses. But if a lunatic hath lucid intervals of understanding he shall answer for what he does in those intervals as if he had no deficiency."[2]

But although this may appear strict enough; nevertheless, that there is considerable leniency extended to lunatics by the English law, is shown by the fact that the protection afforded by it does not merely refer to the mental state of the individual at the time of committing the crime, but also provides for any change which may occur between the time of committing the crime and the award of punishment.

Thus the author already quoted states, that " if a man in his sound memory commits a capital offence, and before arraignment for it becomes mad, he ought not to be arraigned for it because he is not able to plead to it with that advice and caution that he ought; and if after he has pleaded the prisoner becomes mad he shall not be tried; for how can he make his defence? If after he be tried and found guilty he loses his senses before judgment, judgment shall not be pronounced; and if after judgment he becomes of non-sane memory, execution shall be stayed; for peradventure, says the humanity of the English laws, had the prisoner been of sound memory he might have alleged something in stay of judgment or execution."

" The only exception," continues Blackstone, " which appears ever to have existed in the English law to this rule was in the reign of Henry VIII., when a statute was made (33 Hen. VIII., c. 20) which, enacted that if a person, being *compos mentis*, should commit high treason, and after fall into madness, he might be tried in his absence, and should suffer death, as if he were of perfect memory. But this savage and inhuman law was repealed by the statute (1 and 2 Ph. and M., c. 10). For, as is observed by Sir Edward Coke (3 Inst. 6), the execution of an offender is for example, ut pœna ad paucos metus ad omnes perveniat; but so it is not when a madman is executed, but should be a miserable spectacle, both against law, and of extreme inhumanity and cruelty, and can be no example to others."[3]

But another important question in connexion with this matter has arisen, about which there might well be uncertainty were such a thing admissible in legal affairs. It is, in fact, one of the questions proposed to the judges by the House of Lords in M'Naughten's case, and may be expressed thus, Whether a person under partial

---

[1] Commentaries on the Laws of England, by Sir W. Blackstone.
[2] Ibid.                    [3] Ibid.

delusion, committing a crime under the influence of this delusion, but knowing he was doing wrong, is liable to punishment; or to give the question as it was put, " What is the law respecting alleged crimes committed by persons afflicted with insane delusions in respect of one or more particular subjects or persons, as, for instance, when at the time of the commission of the alleged crime the accused knew he was acting contrary to law, but did the act complained of with a view, under the influence of insane delusion, of redressing or avenging some supposed grievance or injury, or of producing some supposed public benefit?"[1]   You will no doubt be surprised to hear that this question was unanimously answered in the affirmative, namely, that the person under these circumstances was to be considered punishable.   The answer given in full was, that assuming the question to be " confined to those persons who labour under such partial delusions only, and are not in any other respects insane," we are of opinion that, notwithstanding the party accused did the act complained of with a view, under the influence of insane delusion, of redressing or revenging some supposed grievance or injury, or of producing some public benefit, he is nevertheless punishable according to the nature of the crime committed, if he knew at the time of committing such crime that he was acting contrary to law."[2]

But we find that a difference exists when the injury is inflicted in supposed self-defence, and when it is inflicted in revenge for a supposed attack or injury.

Thus, in reply to another question put as above, " Whether a person under an insane delusion as to existing facts, committing an offence in consequence thereof, is thereby excused?" the judges stated that " the answer must of course depend on the nature of the delusion; but making the same assumption as we did before, namely, that he labours under such partial delusion only, and is not in other respects insane, we think that he must be considered in the same situation as to responsibility, as if the facts with respect to which the delusion exists were real.   For example, if under the influence of his delusion he supposes another man to be in the act of attempting to take away his life, and he kills that man as he supposes in self-defence, he would be exempt from punishment.   If his delusion was that the deceased had inflicted a serious injury to his character and fortune, and he killed him in revenge for such supposed injury, he would be liable to punishment."[3]   The following is added by one of the judges, who gave a separate answer, that " there is no law that I am aware of that makes persons in the state described in the question not responsible for their criminal acts.   To render a person irresponsible for crime on account of unsoundness of mind, the unsoundness should, according to the law as it has long been understood and held, be such as render him incapable of knowing right from wrong.   The terms used in the questions," he adds, " cannot be said (with reference only to the usage of language) to be

[1] Phillips' Law of Lunatics, etc.          [2] Ibid.          [3] Ibid.

equivalent to a description of this kind and degree of unsoundness of mind. If the state described in the question be one which involves, or is necessarily connected with such an unsoundness, this is not a matter of law but of physiology, and not of that obvious and familiar kind as to be inferred without proof." [1]

If we turn now to the laws of Scotland we shall find that by them criminal irresponsibility is to a great extent based on similar principles; but in the Scotch law there do not appear to exist those minute definitions which we have seen exist in the laws of England. Thus Alison, in his Principles of the Criminal Law of Scotland, states that "if insanity be of that complete and perfect kind which entirely overpowers the reason and takes away from the pannel the power of distinguishing right from wrong, or knowing what he is doing, it forms a complete bar to any criminal prosecution." But he adds, "several nice and delicate questions arise as to the degree of insanity which in law have this effect."

"To amount to a complete bar to punishment the insanity either at the time of committing the crime or of the trial must have been of such a kind as entirely deprived him of the use of reason, as applied to the act in question, and the knowledge that he was doing wrong in committing it;" and he adds, "If the pannel, though somewhat deranged, is yet able to distinguish right from wrong in his own case, and to know he was doing wrong in the act which he committed, he is liable to the full punishment of his criminal acts."

But it is also stated that, "if it appear from the evidence that the pannel, though partially deranged, was not so much so as to relieve him entirely from punishment, the proper course is to find him guilty; but on account of the period of infirmity of mind which he could not control, to recommend him to the royal mercy." [2]

In the case, however, of the trial of Alexander Milne, in Edinburgh, for the murder of James Paterson, in February 1863, where the plea of insanity was found not proven, and which has always appeared to me a singular miscarriage of justice, evidence is afforded that the strict view given above has, to a certain, though small, extent, been relaxed, for the Lord Justice-Clerk, in charging the jury, stated that "the doctrine of criminal responsibility is exceedingly simple. If a person knows what he is doing—that is, if he knows the act he is committing—if he knows the true nature and quality of the act, and if he apprehends also its effects, he is responsible; but if he is in such a state that he does not know the act or its true nature and quality, or the effects which may follow from it, he is not responsible, provided he is in that condition through mental disease. If," continued his Lordship, "you are quite satisfied that the prisoner was under insane delusion at the time of the act, you need not inquire whether he knew right and wrong. If the delusion be once established, the law will presume

---

[1] Phillips' Law of Lunatics, etc.
[2] Alison's Principles of the Criminal Law of Scotland, 1832.

from that, that he did not know right from wrong.  But you must be quite satisfied that he was under an insane delusion.  It is not sufficient to say that a man is in an anomalous state, from whatever cause,—in a state that makes the bad part of his nature get the better of him,—a state, so to speak, of moral depravity.  Weakness of mind, combined with moral depravity, does not make a man insane.  There is no greater mistake.  But if the mind is diseased, the man is insane ; and if the mind is diseased, it is no matter what has caused it, so be that the insanity is actually produced and present at the time." [1]

The particulars of the case here referred to were as follows :— On the morning of the 7th January 1863, the deceased James Paterson sent one of his lads to the prisoner's to see about some work which had been intrusted to him by Milne the day before. This was about ten o'clock.  The prisoner asked, Why didn't Paterson come himself? and told the lad to send Paterson.  This the lad did, and Paterson left his lodgings about eleven to go to the prisoner's. . . . .  At ten o'clock the same morning the prisoner purchased at a shop in West Register Street a dirk, for which he paid 3s. 6d., and which he examined, saying "this would do."  Between eleven and twelve the deceased rushed through a room below the prisoner's shop, saying he was stabbed, and out by a door into the front area. . . .  He then climbed over the railing, staggered up five or six steps into the baker's shop next the prisoner's, crying "I am stabbed."  There he endeavoured to seat himself on the counter, but slid to the ground, and expired. The prisoner gave several accounts of the occurrence, in one of which he stated that he had given his working jeweller a "prog" with the dirk, and in another attributed the death of deceased to an accident.  He (the prisoner) was apprehended in his own shop in about ten minutes, having made no attempt to escape, and he had then the dirk in his possession, with bloody streaks upon it. The principal question involved in the case was in regard to the prisoner's sanity.

Now, the evidence adduced, and cleared of much extraneous and irrelevant matter, showed that the mental condition of the prisoner was as follows :—At a party the prisoner gave on the previous Christmas, he had been drinking to a certain extent, but it struck one of the witnesses that there was something else wrong with him, "there was a wild roll in his eyes ;" he was excited, he arranged his male guests in line, took a sword and marched them to supper to the tune of the Merry Masons, and otherwise conducted himself outrageously.  That on the 5th January he declared that he felt himself unwell, and that he suspected poison had been put in the water ; that on the same day he had expressed a strong fear of robbers, and declared they were trying to poison his wife, his family, and himself.  On the 6th it was shown that he

suspected Paterson of having an intention to break into his shop;
that he was unnaturally afraid of robbers, and that "the spirit had
told him so." That in consequence he removed some jewellery to
a neighbour's safe, expressed his fear of being killed; and on the
7th, although perfectly sober, he seemed very strange. There was
something very piercing in his look; and he declared his wife and
a man had been attempting to poison him.

I have purposely avoided referring to the evidence given by wit-
nesses who had conversed with him after his apprehension, because
such evidence must always be received with great caution. But
by far too great importance appears to have been attached to the
various explanations given by Milne after the deed, as affording
proof of the absence of insanity; for although the medical witnesses
could not on the spur of the moment recall any similar case occur-
ring in a lunatic, it is well known to be by no means an uncommon
occurrence for the insane, after committing an outrage, to attempt to
excuse themselves in this manner, as if they were conscious of right
and wrong, although their conduct in other respects clearly established
the opposite. In short, the evidence afforded as to the prisoner's
mental state at the time of the act, showed that he was suffering
from a very ordinary form of insanity, viz., monomania of suspicion,
and that under the influence of the delusion that he was to be
robbed and poisoned, he killed Paterson.

From evidence admitted in a still more recent case, it would
appear that further relaxation of the strictness of the Scotch law
may be hoped for, and in a direction of the utmost consequence to
a class now almost unquestionably recognised by the medical pro-
fession as insane, but for whom no legal protection at present
exists, namely, impulsive and moral lunatics. I here refer to the
admission, with the concurrence of the Court, of evidence regarding
the mental state of the prisoner's maternal relatives as bearing on
the question of the prisoner's sanity, in the case of J. S. Love,
tried before Lord Ardmillan on 22d September 1864. Although
in this case there was an extraordinary difference of medical
opinion, which led to the medical evidence being virtually thrown
aside altogether, there was no want of direct evidence to show
that at the time of the deed the prisoner was so insane as to render
him irresponsible; but the indirect, though strong, collateral evi-
dence was also admitted, that his mother had been insane and in an
asylum, and that her brother had four idiot children. Now, the
universal admission of such evidence would be of the greatest
assistance in proving that moral and impulsive lunatics should be
regarded legally as irresponsible, as the very essence of moral
insanity appears to me to consist in the fact, that their inability to
act in accordance with the laws of their country is, in reality, a
mental defect acquired by hereditary transmission.

Leaving the legal definition, let us now look for a little to the
medical definition of insanity. In doing so, we find at once that

we enter upon a totally new field of inquiry. The individual's responsibility is now entirely overlooked, and his ability to distinguish right from wrong disregarded. The only question here raised is, Does the person suffer from one or other of the following forms of mental disease, mania, dementia, idiocy, monomania, melancholia, and impulsive and moral insanity. As examples of the various definitions, given by medical writers, of insanity as a disease, may be quoted that by Esquirol, viz., " That insanity is a cerebral affection ordinarily chronic and without fever, characterized by disorders of sensibility, understanding, intelligence, and will;" or that by Cullen, " That insanity is in a person awake a false judgment arising from perceptions of the imagination, or from false recollections commonly producing disproportionate emotions;" or, to take a more lengthy one by Pritchard, insanity is " a chronic disease manifested by deviations from the healthy and natural state of the mind, such deviations consisting either in a *moral perversion*, or a disorder of the feelings, affections, and habits of the individual; or in *intellectual derangement*, which last is sometimes partial, namely, in *monomania*, affecting the understanding only in particular modes of thought, or general and accompanied with excitement, namely, in *mania* or *raving madness;* or, lastly, confounding or destroying the connexions or associations of ideas, and producing a state of *incoherence.*"

As it is customary for every person who treats of this subject to give a new definition, I would suggest the following,—That insanity, the disease so called, is a perverted condition of the mental and moral faculties, consequent on derangement, or diseased condition, or maldevelopment of the nervous system, and which derangement or disease may be either primary, arising from a functional or organic derangement of the nervous system, or secondary, arising from functional or organic derangement of other organs, as of the heart, liver, intestines, uterus, etc.

With regard to the medical definitions of the different forms of insanity, those given by Traill[1] and Taylor[2] may be regarded as sufficiently established. In *mania* " there is a general derangement of the mental faculties accompanied by greater or less excitement, sometimes amounting to violent fury"—(Taylor), producing the furiosity which renders the individual irresponsible according to the law of Scotland.

Monomania and melancholia are forms of insanity " in which the mental alienation is partial"—(Taylor), and characterized by the dominance of one morbid idea, or of a confined class of ideas—(Traill), producing the unsoundness of mind of the laws both of Scotland and England.

*Dementia* " is a total absence of all reasoning power"—(Taylor), producing the fatuous condition referred to in the law of Scotland.

[1] Medical Jurisprudence, by Professor Traill, 1858.
[2] Taylor's Medical Jurisprudence, 1861.

*Idiocy* " is characterized by the want of mental power being congenital "—(Taylor), a term used in the laws of both countries.

With regard to these forms of insanity there can be no difference of opinion between the members of the legal and medical professions, for, if established, we have seen that they render the individual irresponsible.

The great point at issue has reference to those forms of mental alienation known as moral and impulsive insanity.

For this class of cases the law makes no provision, and the reason is obvious. Moral and impulsive lunatics are cases where we have neither fury, delusions, nor inability to distinguish right from wrong. When a woman commits impulsive infanticide, she may be found before and after the act to all appearances quite sane, and sufficiently horrified at the deed she has committed, although in some cases there is sufficient evidence afforded that the desire to commit the act may have existed for days, and gradually increased in strength till it overcome all power of resistance. The same remarks apply to impulsive homicide.

Now, if we examine into these cases, one character will be found to pervade them all, and one to which too little importance has been attached by legal writers, namely, the absence of all motive or inducement. It is true that it may be extremely difficult in some cases to prove the existence of sufficient motive, but when it appears to be absent, and especially when there is also strong presumption of hereditary insanity in the individual's family, we have a condition which it is earnestly to be desired should be legally established as proof of irresponsibility.

To remedy this palpable defect in the law, it has been urged that the legal definition should, by Act of Parliament, be made to include such cases, on the grounds of the person's inability to control his actions ; but leaving out of consideration whether or not this could be effected so easily as imagined, the expediency of such a proceeding might well be called in question. It is true that an impulsive lunatic acts in consequence of this reason, but a thief must be equally excused, because the temptation to steal overcomes his power of resisting the temptation, and so he might equally be declared unable to control his actions.

Before closing this paper there is one more subject to which I would beg to call your attention ; and that is in connexion with attempts at suicide. Is every attempt at suicide to be regarded as a proof of insanity? The almost invariable finding of the English coroners that persons who have committed suicide have done so under a fit of temporary insanity, might well lead us to suppose that an attempt at suicide should be regarded as a proof of temporary insanity ; but before coming to this conclusion, should we not also consider whether if the public were to realize the fact more than they appear to do, that an attempt at suicide is still legally a crime punishable by law, and that its commission might stamp their

family with all the odium that a crime regarded as a felony and little inferior to murder had been committed by one of its members, might not this knowledge serve to check to some extent the prevalence of attempts at self-destruction.   There can, I think, be no doubt that there are many who attempt suicide who would shudder at the idea of being thought capable of committing murder; and yet what is the law on the subject?   The great exponent of the English law argues the question thus: " The pretended heroism, but real cowardice of the Stoic philosophers, who destroyed themselves to avoid those ills which they had not fortitude to endure, though the committing it seems to be countenanced by the civil law, yet was punished with cutting off the hand which committed the desperate deed.  And also, the law of England wisely and religiously considers that no man hath a power to destroy life but by commission from God the author of it, and as the suicide is guilty of a double offence, one spiritual in invading the prerogative of the Almighty, and rushing into his immediate presence uncalled for, the other temporal, against the king, who hath an interest in the preservation of all his subjects; the law has, therefore, ranked this among the highest crimes, making it a peculiar species of felony,— a felony committed on one's self; a *felo-de-se*, therefore, is he that deliberately puts an end to his own existence or commits an unlawful malicious act, the consequence of which is his own death: as if attempting to kill another, he runs upon his antagonist's sword, or shooting at another, the gun bursts and kills himself.  The party must be of years of discretion and in his senses, else it is no crime. But this excuse ought not to be strained to that length to which our coroner's juries are apt to carry it, viz., that the very act of suicide is an evidence of insanity; as if every man who acts contrary to reason had no reason at all, for the same argument would prove every other criminal *non compos*, as well as the self-murderer. The law very rationally judges that every melancholy or hypochondriac fit does not deprive a man of the capacity of discerning right from wrong, which is necessary to form a legal excuse; and therefore, if a real lunatic kills himself in a lucid interval, he is a *felo-de-se* as much as another man." [1]

From the foregoing observations we have seen that a very important difference exists between insanity regarded as a medical, and lunacy regarded as a legal question.   While the former applies to a disease which may or may not destroy responsibility, the latter demands that irresponsibility be proved, before it can be admitted as a plea; and the great puzzle of the present day with regard to civil incapacity and irresponsibility is, how to bring insanity under medical treatment before it has rendered the individual legally irresponsible.   Into this subject, I cannot at present enter, and will now close with the practical inference that it is the duty of the medical practitioner, in giving evidence as to the mental condition

[1] Commentaries on the Laws of England, by Sir W. Blackstone.

of a person on trial for outraging the laws of his country, to satisfy himself that the insanity destroys responsibility, before certifying him to be a lunatic. To certify a person in such circumstances to be insane because he appears unable to control his actions, is to offer a medical theory where a medical fact is demanded, and is to become an advocate of that pseudo-philanthropy which would insist on moral perversion being legally recognised as proof of irresponsibility,—a state of matters which would be fraught with the greatest danger to all our social interests. The depravity, immorality, and low cunning of a convict may resemble in some respects the character of the true moral lunatic; but for such cases seclusion in Portland or Pentonville is the best of moral remedies, and in dealing with such, the medical practitioner should never forget that laws " cannot be framed on principles of compassion to guilt."

ARTICLE IV.—*On the Facilitation of the First Stage of Labour.* By ANDREW INGLIS, M.D., F.R.C.S.E.

*(Read before the Edinburgh Obstetrical Society, 8th March 1865.)*

AT present many different means are in use for favouring the progress of the first stage of labour. Simple dilatation with the finger has from time to time been alternately praised and decried. Just now the voice of the majority is in its favour; and I am certain that it does good in some cases, but only where the resistance to dilatation is not excessive. Within the last few years, india-rubber dilaters have been tried pretty extensively, and they also have been found of service. However, not only is considerable difficulty experienced in their use, but that difficulty is usually great just in proportion to the urgency of the case. Sponge tents also have been used in such cases, but I have never seen them produce any great result. While these our direct surgical means are so scanty, there is no lack of therapeutic agents which have been tried. Injections have been used extensively, both per vaginam and per anum, consisting of warm water, as well as containing opium, belladonna, and other drugs. Medicinal applications to the same parts, in a more concentrated form, have also been used.

Remedies intended to act through the general system are frequently given. Chloroform, which is the most valuable of these; opium, which also certainly sometimes does good; tartrate of antimony, bleeding, emetics, purgatives, and ergot,—have all been in turn pressed into the service, but I think little need be said about their general inefficiency. My experience supports what I believe to be the general opinion,—that it is precisely where dilatation without interference is most tedious, that the foregoing means are most nearly inert.

To show that there is good reason for believing that another means—namely, separation of the membranes for some distance round the os—will promote dilatation much more efficiently than any hitherto proposed, is the object of this paper; and I believe that where attempts at direct dilatation have been successful, the success has been in great part owing to unintentional separation of the membranes. In pursuance of my object, I shall call attention to the effect on the character of labour of such a separation, either as occurring spontaneously or effected artificially for its induction or facilitation.

In the first stage of labour coming on and proceeding without interference, there are two opposite conditions of the passages,—one in which there is a copious discharge of viscid mucus, and which is often called a "wet labour;" and another, in which there is hardly any, and labour is called "dry." At the full time, the first seems to occur normally in the cow, mare, bitch, etc., and, I am inclined to believe, is natural also in the human female. I consider the following as being the natural process in women :— The ovum having become ripe, the membranes separate from the cervical portion of the uterus, if not from the whole surface. They then by their weight press more heavily against the cervix, even when the patient is lying down; and as the pressure is soft, equable, and continuous, the cervix gradually yields to it and becomes quite slack, and this takes place without the occurrence of pain. Next, when relaxation has become complete, the mucous discharge commences, proceeding from the uterus. Finally, a pain comes on and terminates the first stage. That the discharge comes from the uterus is shown by its protruding from the os previous to its appearance in the vagina. Besides, it is only found when the membranes are already separated, and is very often tinged with blood before pains have been felt. A process resembling this form of the first stage may be seen where the other muscular canals are concerned, and perhaps most prominently in the case of the rectum. If the finger or bougie is gently and cautiously inserted past the sphincter ani, and kept there for some time, complete relaxation gradually ensues, a profuse discharge from within the sphincter comes on, and, if the bougie is allowed to remain long enough, the muscular fibres above begin expulsive action.

The foregoing explanation seems equally applicable to "wet" cases of abortion or miscarriage in the human subject, except that in such cases healthy ripening of the ovum cannot be said to be the cause of the separation of the membranes.

Since I have begun to pay attention to the subject, I have met with many examples where the first stage has been sudden and short, on account of previous separation of the membranes. The following are details of three of these :—

In November last, I was sent for to attend a patient who had felt a subsidence of the uterine tumour, and a slight bearing

down, affecting the bladder and rectum, but without pain.   Examination showed the vagina to be in a normal condition, but the os was as large as a shilling and quite thin.   The finger could be pushed a good way into the cervix, and the membranes were found separated as far as could be reached.   There was no discharge.   This condition of the parts continued for about sixty hours after I saw her first.   When I called on her on the fourth morning, no pains had yet appeared; but there was a quantity of thick mucus around the os, and, on pressing the abdomen, the membranes distinctly yielded and came down a little.   Soon after my visit, two or three pains in close succession came on, and the child was born before any one could leave the house for assistance.

At the last meeting of the Society, I mentioned a case where, very shortly after labour had begun, and while the patient had as yet had very few pains, I found the os dilated and the membranes protruding beyond the external parts.   Though she had not sent for any one, she had felt the pressing down for days before; and as the membranes were displaced to the extent I have mentioned, a minute or two after the first pain had been felt they must have been previously separated.   It is worthy of notice that in this case there was great deformity of the pelvis.

About the beginning of October, I was called to a case where labour had not been expected by the patient for some weeks, but where the membranes had ruptured in consequence of violent bodily exertion.   I found the liquor amnii had only partially escaped from a tear in the membranes, and was still coming away every time she moved.   There was a quantity of thick mucus around the os, which was slightly open, and moreover showed by its character that the ninth month was nearly if not quite complete.   The pains came on in about twelve hours, and labour was over in less than a quarter of an hour.   In such cases, where the liquor amnii comes away before labour, I believe our fears as to the character of the labour should be at once allayed if we discover the thick mucous secretion, holding that its presence is always proof of previous separation of the membranes accompanied by a soft and dilatable state of the os.

The other form of the first stage, which I have designated "dry," is very different.   Preliminary to labour there is no separation of the membranes, and consequently no slackening of the cervix, and there results a great increase in the amount of uterine exertion required, even where nothing worse is entailed; for, though separation and dilatation generally at last take place, they do so only at the expense of multiplied and more distressing pains. At best, the pains may stop for a time after causing some separation, and labour on their return may thereby then come to take on the "wet" form; but very often no such intermission occurs, and dilatation costs a long series of wearing-out pains.   Moreover, the membranes may rupture, making dilatation have to be effected

by the fœtus, and therefore even slower and more painful. Again, sometimes the imperfectly dilated cervix is forced into the pelvis along with the head, and becomes jammed between the two, increasing greatly the pain and delay. Lastly, the resistance to dilatation may cause permanent cessation of the pains, or the cervix, or body of the uterus, or both, may rupture.

For some time when first in practice, I was in the habit of inducing premature labour in cases requiring such interference, either by introducing a catheter between the membranes and the uterus and leaving it there, or by using sponge tents. In these cases, labour often came on very soon, though a few resisted for a long time the action of both means combined, and the membranes had to be ruptured before pains could be made to come on regularly; but in all cases, whether labour followed at once or came on only after great expenditure of time and trouble, the first stage was slow and painful, and in some the pains continued for days before dilatation was complete.

About eighteen months ago, I attended a case where Dr James Sidey brought on premature labour by the late Dr Hamilton's method. By means of Dr Hamilton's uterine bolt, he separated the membranes for some inches round the cervix, and the instrument was passed in and turned round every twelve hours. About forty-eight hours after the first separation of the membranes, the os was soft and dilatable, and a little discharge was apparent at the top of the vagina. Six hours later, I found the discharge copious; a few minutes after I had ascertained this by examination, labour came on suddenly; and in three or four pains the first stage was completed, and the head well down in the pelvis.

Soon after this I induced premature labour in the same manner in another case. In about forty-eight hours the discharge was copious, and the os soft and dilatable, but no pains had appeared; but immediately after my examination one pain came on and completed both stages.

Since the first of these cases I have never employed any other means for inducing premature labour, being convinced that it is the easiest for the patient, though not always the speediest. The first stage of labour induced by it seems identical in all respects with that of the " wet" form of labour occurring spontaneously.

The circumstance of the first stage of labour so induced, presenting so favourable a character, has led me to use that separation also for the facilitation of labour already begun in cases rendered tedious by so-called rigidity of the os. The number of cases in which I have had recourse to this mode of procedure is perhaps not yet sufficiently considerable to entitle me to lay much stress on these as bearing on my argument; but I may state that all of them which can be considered as affecting the question seem to support the practice I am advocating. The following are particulars of a few :—

On the evening of 5th November, I saw a patient in the fifth month of pregnancy, who had had strong pains all day; she thought

the child was dead, and I could discover no fœtal sound. The os was firm, and barely admitted the finger, and there was no discharge. I ordered some opium, and called the next morning. The os was then nearly in the same state, but the pains were more severe. At night she was much worse, the opium having failed to stop the pains. The os was still firm and the discharge absent. I therefore separated the membranes from the cervix as far as I could reach, after which the pains ceased; and, after waiting for about two hours, I left her till morning. About ten hours afterwards I was sent for. She had not had any more pains, but felt something pressing down at the vulva. I examined and found that the entire ovum had slipped down into the vagina. The child had evidently been dead for some days.

On the 2d of February, I was sent for to attend a woman at the sixth month. There had been some strong pains; the abdomen was hard and contracted, and very tender to touch; and the os would not admit the finger. The pains continued for two days and two nights without producing any local change. As the fœtal sounds were normal I continued to give sedatives regularly during that time, but without any apparent effect on the pains. Vomiting then came on; the pulse rose to 130; and the tongue became brown and coated, like that of a patient with fever. I then tried to introduce my finger to separate the membranes; but found the os too firm to admit it, and therefore had recourse to a sound. The placenta being at the fundus, I separated the membranes all round to about the middle of the body of the uterus. The intervals between the pains then became longer, and a large bag of membranes gradually protruded, and in less than two hours the patient was delivered. The child was alive until immediately before birth, but never breathed.

On the 7th February, one of the New Town Dispensary pupils went to a patient in labour who was in the seventh month of pregnancy; and after she had been about fifty hours ill he sent for me. The os was only about the size of a shilling; and as there was no chance of labour going back, and, moreover, the child seemed to be dead, I separated the membranes with the finger as far in as I could, with due regard to the position of the placenta, which was near the os, and had therefore to be avoided. After separation, the pains, though stronger, began to come on at longer intervals, and I believe would, in the absence of further interference, have died away; but, as I feared to risk longer delay, they were kept up by mechanical irritation of the uterus, and the child was extracted an hour and a half after my arrival. It was much decomposed.

On the 17th of May, I was called to see a patient in the eighth month. She had severe purging and vomiting. She suspected the child to be dead, and, as she was very dangerously ill, I considered it advisable to encourage the labour, which she thought threatening. I therefore separated the membranes with the finger as far as I

could reach, and about ten hours after, a putrid fœtus was unexpectedly expelled by a single pain.

In one of the cases I reported to the Society last month, I examined and found labour coming on at the full time while the membranes were still adherent. I separated them, and gave a large opiate. The pains ceased, but returned in about twelve hours, and though the pelvis was badly contracted, the first stage was got over with great ease.

To recapitulate—

1st, The easiest form of the first stage of labour is characterized by protrusion of the membranes and a copious discharge.

2d, These are always direct consequences of separation of the membranes.

3d, The result of artificial separation seems precisely similar to that of spontaneous.

The following is the practice I would recommend in regard to separation of the membranes :—
1st, That it should always be the initial measure in the induction of premature labour; and that until complete relaxation of the os has resulted from it, there should be no further interference of any other kind; 2d, That when labour has begun without previous separation.of the membranes, and these are still adherent, they should always be at once separated, as the best means of overcoming the rigidity of the os, and the painful and prolonged first stage, which almost invariably accompany such a state of matters.

In the latter of these two cases, if the pains are severe and ineffective, I should be inclined to recommend sedatives to be given at the same time, in the hope of gaining time for painless relaxation to take place.

With regard to the means of separating the membranes, I have in most cases been able to do it with the finger, though in one or two an instrument was required. In one case, which I have not reported, I could not at first get the finger far enough in to effect any extensive separation ; but the small portion round the os, which was thus denuded, was so relaxed an hour after that I was able to insert the whole finger and separate to the desired extent.

Where I have used an instrument it has been Dr Hamilton's uterine bolt, which I have already mentioned. As its form is not well known I give the accompanying drawing. From its rounded
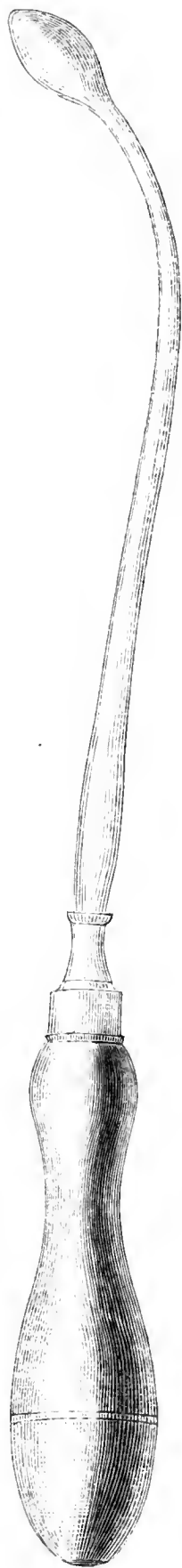
form at the end there is but little fear of rupturing the membrane with it.

Since reading the foregoing I have added the following as manifestly supporting my views :—

Dr Trask, in his prize essay on Placenta Prævia, has shown, on the one hand, that where the placenta is not much separated the os is almost always rigid, and that the force used to procure dilatation is the cause of death in a large proportion of the fatal cases of placenta prævia; and, on the other hand, that those cases in which separation has taken place are specially characterized by speedy delivery by the natural efforts. He mentions also that the effect of artificial separation in such cases, as regards speedy expulsion, is nearly, though not quite, as good as that of natural separation.

Moreover, in all the cases I have met with myself I have found delivery take place naturally and easily where separation of the placenta had *previously taken place* to a great extent; while, after slight separation, extraction of the child had always to be effected by artificial means.

The usual history of primiparous cases seems also to be confirmatory of the views I have now advocated. The duration of the pregnancy being generally shorter, labour commences before the separation of the membranes has occurred, and the first stage is in consequence much slower than in subsequent labours. This is commonly supposed to be the result of the absence of that mucous discharge already referred to, but I think, in reality, chiefly arises from the want of previous separation of the membranes, of which the mucous discharge is only a symptom.

———

ARTICLE V.—*On the Medical Organization of the Army of the Potomac.*

IN a former communication it was briefly stated that the Federal authorities had organized their field hospitals either on the divisional or corps system. Both are at present in operation; but the former (divisional) for the time being has the preference, and is the plan pursued in the so-called "Army of the Potomac;" whereas, in the "Army of the James," the corps system is under trial, and appears to be advantageous.

Regimental hospitals at the commencement of the war were tried, proved a failure, and have consequently been abandoned. These hospitals were sufficiently suited for the purpose while the United States maintained only a small standing army, and the various regiments comprising that army were kept at their uniform strength; but they were found valueless, and unfit to adapt themselves to the requirements of a large field force. The organization of the different state regiments may have had something to do with the impracticability of this system. The authorities of the various states, in sending a certain number of regiments to the

field from their individual states, fail to fill up the numerous vacancies caused either through disease or battle, but, in place thereof, find it more convenient, either from political or some other causes, to raise new regiments, such being more popular at the time. The old regiments are sometimes reduced to a mere handful from the above causes, and the military authorities are thereby compelled to amalgamate a certain number of regiments so decimated, so as to form one respectable battalion.

If the war lasts sufficiently long, it is more than probable that corps field hospitals will supersede the divisional throughout the entire army.

The corps hospital is organized on the same principle as the divisional. The funds for carrying on the establishment are procured, the medical officers and operators are allotted and selected in the same manner, only that it is worked as one whole and not as three separate establishments. This in no way interferes with the hospital at the transportation base. It will therefore be perceived that an hospital of this nature assimilates itself more to the working of a general field hospital; and there are not wanting officers in the British medical service, whose experience in field arrangements has not been limited, who advocate the superiority of the general hospital system over the regimental, as adapting itself better to an army in the field. An English army on field service has both systems in operation, and the head of the Medical Department is necessarily compelled to work both.

Various advantages are gained by the corps system being established. The medical officers can be more economized, and they, as well as the medical appliances, can be made available at any part of the hospital, which cannot be so well done when the corps has three divisional hospitals, and frequently separated, as they are, considerable distances from each other. Should it become necessary, from some military operation, to separate a division from its corps, and send it to some distant part, an hospital equipment for such a force could easily be allotted from the corps establishment. Otherwise the corps system gives more harmony and unity to the whole,—the business of the corps director is simplified, and the working of the ambulance corps would be facilitated, by rendering mistakes less liable in bringing the wounded from the field during any excitement that might occur, there being only one hospital for the whole corps; besides, the various documents connected with the hospital—such as, returns, reports, and other records —while being more easily procurable, would be more reliable.

There is not that division of labour in their large hospitals that one would expect to find, after the experience of four years that the Federals have had. Under their present organization, it is not unusual to find the medical officer, in numerous instances, quartermaster, commissary, as well as commanding-officer; and this is particularly observed in some of their large hospitals in the cities of the Union. Having established a most perfect

ambulance system, it is surprising that the Federal authorities have never attempted to organize an hospital corps. This is as needful and as essential to complete the working of a military hospital as any part of the equipment thereof. It is true that a battalion, under the name of "Veteran Reserve," has been formed to remedy this deficiency and obviate some of the requirements ; but, as far as observation goes, it is very defective, and by no means tends to forward the end in view. Selections of men for this important duty have not been well made ; it seems more an asylum for the lame and the incapables who have got sufficient political interest to obtain transfers to this corps ; and in by far the greater number of instances where their services have been required, they have not given that satisfaction which was expected from them. In fact, complaints are but too rife as to their total incapacity as hospital servants, as tending, in numerous instances, rather to obstruct than facilitate the working of the establishment.

A word in passing regarding those large hospitals in the various cities of the Union. Where permanent buildings have not been converted to hospital purposes, the American authorities have erected temporary accommodation after the form of Crimean huts. These structures are similar in their general features in nearly all instances, consisting of a common corridor, with detached pavilions projecting therefrom. This corridor is not always of the same form it being sometimes circular, at others oval, diagonal, or straight. The various forms it may assume are inexhaustible, and as frequently depend on the whim of the authorities as the design of the architect. The pavilions project from the common corridor at certain and uniform distances from each other, and the number of them depends on the length of it, and on the number of sick to be accommodated.

These pavilions are sometimes built on piles, thereby raising the floor three or four feet from the ground, and so allowing a free current of air underneath, the average length of each being from 150 to 175 feet, 20 feet in breadth, and from 17 to 19 feet to the ridge. They are lined with smooth boards on the inside, and lathed and plastered on the outside, and on the average each patient has an allowance of 900 or 1000 feet of cubic space.

In addition to the ward in each of the pavilions, are a bath and ablution room, scullery, water-closet, and ward-master's room,—all of which take up on the average 20 feet in length of the structure.

The ventilation is remarkably good, and, in addition to the usual modes, ridge ventilation has been added, running the whole length of the building; and also small holes are cut in the sides of the walls under the beds, allowing the free entrance of external air, and thereby causing an upward current. By these means the whole building can be kept cool and free from offensive odours. The holes cut in the walls can be closed at will by sliding doors.

The kitchen arrangements are most excellent. A separate building is allotted for this purpose within the hospital enclosure, and the cooking is principally performed by steam. The funds for carrying on

the interior economy of those large establishments are procured after the manner described in the former paper regarding field hospitals.

These large establishments have in connexion with them, and often in the hospital enclosure, laundry, bakery, printing-press, engine-room, butcher's shop, etc., as well as other necessary arrangements; so that, with the exception of procuring the rough supplies, they are complete within themselves, and independent of the outside world. These temporary structures are generally on a very large scale, much larger than anything of the kind in Europe, and sometimes accommodating as many as from 1200 to 3000 patients in one establishment.

It is difficult to speculate on the advantages gained by the erection of such vast establishments; except it be the economizing of medical officers, as well as the difficulty of procuring gentlemen in sufficient numbers, with the requisite amount of administrative ability, capable of holding such responsible charges, and conducting the intricate business of such a machine. In situations of this description, other indispensable qualities are necessary besides professional knowledge: the acquirement of the habit of command, knowledge of the habits and customs of soldiers, as well as sufficient firmness to carry out those measures which are so often necessary in the management of such numbers of sick.

It is impossible to describe the difficulties that the officer in charge has to contend with in the management of these hospitals, and the amount of work he is called upon to perform is incredible, under the imperfect system they are obliged to follow out; taking all things into consideration, the highest praise is due to the medical officers holding those onerous positions.

I will now briefly call attention to one of the many things which have been introduced into field practice during the war; and, firstly, to " Smith's Anterior Splint."

This is contrived as a swinging support for compound or simple fracture either of the leg or thigh during transportation, after the limb has been properly put up in ordinary splints and bandages. This splint has cheapness, durability, and simpleness of construction, as well as easiness of application, to recommend it as a slinging apparatus and as an adjuvant to the other medical appliances. It consists of two simple parallel bars of iron, little thicker than an ordinary goose-quill, forty-two inches in length, three inches apart, and united by being continued transversely at the extremities; the horizontal bars being kept in their place and maintained parallel by three transverse bars, one near the centre, and the others at six and seven inches from either extremity; the same six and seven inches of either ends of the splint are continued sloping upwards at about an angle of 35°, to adapt itself over the foot and groin respectively; so that the straight part of the splint will only measure about thirty inches. To this iron frame, when properly adjusted

to the anterior part of the limb by bandages, is attached a cord by
means of two hooks, which respectively pass over the horizontal
bars of the splint; near the junction of the straight and sloping
portions, to this cord is attached a second by means of a loop,
which again passes through a pulley fastened to the roof of the
ambulance-waggon, thence on into the hands of the patient;
thereby suspending the wounded limb, and hence enabling the
patient to regulate the amount of its elevation, either raising or
lowering it as his feelings may direct.   When the limb has been
placed in an easy and comfortable position, either the patient him-
self or the medical attendant can fasten the cord to a " belaying
pin " fixed to the side of the waggon for that purpose.

This, as a transportation splint and support for fractured limbs,
is admirably adapted for the end in view, as well from its lightness
as from its easy manner of application.   Considerable difficulty has
always been experienced in the transportation of such fractures;
but by this method remote contingencies are averted, and from the
double fixture of the cord at the two extremities of the splint, it
has the advantage of giving a more full and equable support to the
whole limb than when it is supported only from one fixed point.

This splint has now been in use a considerable time, and has so
far met the requirements of the service in preventing the augmen-
tation of the patient's suffering during transportation.   By so
slinging the limb it is also protected from the numerous casualties
that are so apt to occur, and to which an injury of this kind is so
liable to be exposed over bad and imperfectly-formed roads, thereby
giving greater security, facilitating the chances of recovery, and
preventing as far as possible motion of the displaced fragments.

Before closing this paper, I would wish to make a few remarks
on the peculiar method of treating penetrating gunshot wounds of
the chest by " hermetically sealing," recently brought into notice
and practised by Dr Howard, of the United States army, having had
the good fortune to see several cases under this method of treatment.

The plan adopted is based on the exclusion of atmospheric air,
and the removal of collecting fluids.   The profession at large will
be much indebted to Dr Howard for bringing forward this peculiar
method of treatment, if he can show from statistics, as well as from
detailed accounts of cases, the success of the operation; and he will
also have conferred a boon on suffering humanity, considering that
the proportion of fatal cases in this region of the body has always
been very large.

The directions laid down by Dr Howard for the performance of
the operation are as follow :—

" All accessible foreign bodies having been removed, the point
of a sharp bistoury is introduced perpendicular to the surface just
beyond the contused portion, and with a sawing motion the circum-
ference of the wound is pared, converting it into a simple incised

wound of an elliptical form. Dissect away all the injured parts down to the ribs, then bring the edges of the wound together with wire sutures, deeply inserted; secure them by twisting the ends, which are cut off short, and turned down out of the way. Carefully dry the surface, and with a camel's-hair pencil apply a free coating of collodion over the wound; let it dry, and repeat at discretion. For greater security, shreds of lint or charpie are arranged crosswise over the wound; saturate it with collodion, and when dry, repeat till the wound is securely cemented over; and, as a still greater protection, a dossil of lint may be placed over the part and retained with adhesive straps. If there be a tendency to undue heat in the part, it may be kept down with cold affusion. Should any loosening of the dressing occur, an additional coating of collodion may be applied. The sutures must not be removed until healing by the first intention is complete.

"Should suppuration occur so as to occasion distressing dyspnœa, proceed to treat it in all respects as a case of empyema, introducing the trocar at the most dependent point, taking special care to avoid the admission of air."

Considering the small number of cases which I have been fortunate enough to see under this method of treatment, it is difficult to predict its future, even as a surgical operation.

I do not suppose for one moment that, when Dr Howard matures his views, he will advocate the suitability of the above operation in all cases, either of penetrating or perforating gunshot wounds of the chest. By so closing the orifices of the wound, union by the first intention will in most instances ensue, and dyspnœa for the time being will be alleviated; but how long will this latter good result continue? Just so long, I consider, as inflammatory action can be controlled, and effusion kept in abeyance. And this will depend in great measure on the amount of contusion the lung has suffered, the amount of dead tissue to be cast off, and the presence of foreign bodies, if any, in the track of the wound.

Penetrating gunshot wounds of the chest are notoriously prone to be followed by serous and purulent effusions, no matter what means may be taken for their prevention; and if any of those irritating contingencies above mentioned exist, we conceive that, in nearly all cases, the above operation can scarcely be expected to prevent their formation. Besides, if inflammatory action becomes fully established, and pent-up matter be retained in the cavity of the chest, it follows, as we have seen under this method of procedure, that such foreign matter, seeking escape, will press on the recently united tissue, and that this being weak and unresisting, will naturally give way, and the old track of the wound be established.

It is well known that foreign bodies, such as bullets, etc., sometimes become encysted when lodged in the lungs; but, in the majority of instances of this nature, they are but too liable to give

rise to extensive disorganization of the pulmonary tissue. And even in those favourable instances of the bullet becoming encysted, great suffering is the result at various times during the lifetime of the individual, rendering existence a burden.

I have been informed by a surgeon-in-chief of division, that he has performed the above operation with complete success in cases where the pleural cavity was perforated, and the surface of the lung grazed by gunshot; and I can easily conceive that the operation in question is admirably adapted for cases of this description.

If, by further evidence, the operation should eventually become established, great praise will be due to Dr Howard for the great pains and assiduity he has bestowed in endeavouring to prove its advantages under the numerous difficulties that must attend cases of this description on active service.

### Additional Notes on "Hermetically Sealing."

No hospital as yet has been organized under Dr Howard for the special treatment of "hermetically sealing" gunshot wounds of the chest. He has been treating cases in the hospitals at the base of field operations. Till within a very recent date such cases were sent away, along with other sick and wounded, so that all traces of them were lost.

Dr Howard has not met with that success which his enthusiasm deserves, has made but few converts to his theory, less to his practice, the operation itself being looked upon with marked suspicion by far the greater number of medical officers of the United States army.

The operation has novelty to recommend it, looks well as a theory, but when put into practice it assumes a different aspect.

In detailing the following cases, it must be borne in mind that I took no notes at the time I saw the cases,—the details are written from memory a month after. The description of the cases may be relied on, though I cannot vouch for the accuracy of the dates.

Nine cases were treated in the 18th Corps Hospital at the base of field operations by Dr Howard, according to his plan, with modifications, as laid down in the American Medical Journal.

In two of the cases he performed his elliptical-shaped incision,— first paring away all the lacerated tissue, removing the broken debris of bone, and other foreign substances, if any, around the wound, etc., and bringing the raw edges together, and securing them with silver-wire sutures, drying the surface and applying collodion over it; and, for better protection, arranging charpie crosswise over the wound, over which is smeared a thick layer of collodion. A dossil of lint, retained by adhesive plaster, is placed over all for safe security.

The first of the above cases was that of a man belonging to one of the Massachussets regiments, who had been wounded, about

the end of June or beginning of July, by a minie ball passing obliquely through his chest, entering (if I properly recollect) just over the first rib of right side, about half or three-fourths of an inch from sternum, and making its exit near the centre of infra-spinous fossa of left scapula. The elliptical operation had been performed by Dr Howard in this case shortly after the receival of the injury. On the evening of the fifth or sixth day after the operation (I think 5th or 6th July), I saw this man with Dr Howard ; he seemed to be doing well. No examination was made at this time. The following day the man was again visited, when the successive layers of dressing from the posterior orifice were for the first time removed since the operation.

It was then seen that union of the edges of the wound had taken place. The wire sutures, three in number, were at this time removed, oozings of serum taking place as they were withdrawn, and also after their removal, through the small crevices. The healed or united surface was again covered with collodion, and dressed in the same manner as before described, after the raw edges had been brought together. The outer dressing of the anterior orifice was only removed at this time, as it was found when the charpie was half removed that the wound had not entirely cicatrized, and that oozing took place from it. It was re-applied (the removed half of the dressing), and over the whole collodion was coated as formerly, with fresh lint and adhesive plaster to retain it properly. At this time the man complained of no pain, felt comfortable ; when he spoke did so in short broken sentences. There seemed to be slight dyspnœa, accelerated pulse, and increased respiration, mostly diaphragmatic.

The physical signs showed the lungs engorged with blood, crepitant and mucous rales being perceptible over the chest, and percussion yielding a dull sound.

Dr Howard thought that the pleural cavity contained fluid. He proceeded to thrust a moderate-sized trocar and canula into the chest, between the fifth and sixth ribs. Withdrawing the trocar, he affixed a nice-fitting brass tube to the canula; to which brass tube is adherent a caoutchouc bag, for the reception of any fluid that may be within the pleural cavity. Allowing this apparatus so adjusted to remain for a considerable time, he withdrew it, placing his finger over the canula, to prevent the admission of air. This elastic receptacle when squeezed was found empty, no fluid having passed into it from the pleural cavity. The canula was now withdrawn, when a thick coating of collodion was spread over this wound, and a piece of adhesive plaster placed over all.

The following day the condition of this patient was not so favourable, all the previous symptoms being on the increase. The posterior orifice, from which the wire sutures had been withdrawn, was again uncovered, and, on removal of the dressing, it was seen that what had so recently united by first intention was now an

open wound.   The pent-up serum and other discharges accumulat-
ing, had burst through this weak and unresisting barrier, converting
it into an open sac.   The anterior orifice, which had only previously
been partially undressed, was now completely uncovered; serum
trickled from the wound, and it was only imperfectly united; and
at every inspiration and expiration this thin layer of tissue was
elevated and depressed.   Around this opening there was a consider-
able amount of emphysema.   The wound was dried, and over each
orifice charpie was arranged crosswise, and covered with a thick
layer of collodion; and over all a dossil of lint placed, retained in
its place by adhesive plaster.   Over this anterior dressing, water
dressing was ordered to be applied, on account of the emphysema.

The physical signs, as well as the other symptoms, were also on
the increase, the vesicular murmur being inaudible and overpowered
by the crepitating rales.   Dr Howard still considered that the
pleural cavity contained fluid.   He once more introduced the trocar
and canula, as above described, into that cavity, but on this
occasion between the sixth and seventh ribs.   As formerly, no
fluid of any description was drawn off, and the wound so made
was again closed in the usual manner, as previously mentioned.
It so happened that the man from this time gradually became
weaker and weaker, and died on the afternoon of the following
day.

The other one of the two cases in which the "elliptical incision"
was performed, and all the steps of the operation gone through, is
that of a man, also in the 18th Corps Hospital, wounded by a minie
ball about the same time as that mentioned in the previous case,—
the end of June or beginning of July.   In this case, the projectile
penetrated the chest from behind, and lodged therein, entering
close to the lower margin of left scapula (I am not certain of
having described the exact spot of entrance).   The operation in
this case, as in the other, was also performed shortly after the
receipt of the injury, and possibly with the idea of arresting
hæmorrhage, and that a cyst might form round the ball.

The patient, I was informed, did well for the first three or four
days after the operation; but I can only remember the man as fast
sinking, the wound partially reopened, and of his having expired
shortly before the preceding case.

In this case it is but just to state, that it is more than probable
that this patient would have succumbed under any kind of treatment.

The remaining seven cases were treated by Dr Howard on what
may be called a modified principle of "hermetically sealing."
The exit and entrance orifices were closed from twelve to twenty-
four hours after the receipt of the injury, by coating the edges of
wound, previously dried, by a thick layer of collodion, then arrang-
ing charpie crosswise over the wound, coating that with collodion,
and placing lint, retained by adhesive plaster, over all; in fact,
going through all the steps of the operation except the coating

part, and the bringing the edges of the wound together by wire sutures; but excluding the atmosphere and its attendant pressure as thoroughly as when all the steps in operation are gone through.

The seven cases referred to were perforating wounds, occurring in various parts of the chest (not having taken notes at the time I am unable to give their situation); but nevertheless, with all the care taken in drawing off the accumulated fluid in the pleural cavity, a like result to that of the two foregoing cases annexed,— namely, a fatal result in all.

*February* 1865.

<hr>

ARTICLE VI.—*Cases illustrative of the Employment of Acupressure as a Hæmostatic Agent.* By PATRICK HERON WATSON, M. D., F. R. C. S. E.; Lecturer on Surgery; Surgeon to the Royal Infirmary and Chalmers' Hospital, Edinburgh.

CASE I.—C. G., æt. 21, a sailor, was admitted into the Royal Infirmary on account of a strangulated inguinal hernia. Symptoms of incarceration had existed for six hours, and of strangulation for two hours, previous to admission. Repeated efforts at reduction had been made, and the parts were very tense and tender. The testicle occupied the back part of the swelling, which extended somewhat below its level. The sac was greatly constricted at the external ring, but the bulge along the line of the inguinal canal was well marked. The cord could be distinctly traced crossing the front of the protrusion, and lying at the internal abdominal ring, on the outer side of the neck of the sac. After one fair effort, when the patient was under chloroform, to reduce the hernia by the taxis, the operation was performed in the usual way, by opening the sac. There were three distinct constrictions found in the course of the operation: one at the external abdominal ring, another at the internal abdominal ring in the neck of the sac, the third in the structure of the omentum, through which the coil of intestine, a foot long, had protruded. The omentum was attached by old and firm adhesion to the neck of the sac. There was great congestion of all the textures involved, which presented a perfectly dry appearance; there was, however, no "nipping" of the bowel, or lymphy deposit on its serous surface. There was no fluid collected in the lower part of the sac, and the testis was felt lying at its lower and posterior part. The bleeding was very copious, but was readily stanched by three needles, wire sutures introduced, and pads of lint secured by a spica bandage employed. These dressings were removed and the needles withdrawn at the end of thirty-eight hours after the operation, when a mere serous fluid escaped from the outer angle of the incision. The bowels moved spontaneously in the course of the second day, and all seemed progressing favourably,

when peritonitis, accompanied with diarrhœa, set in, and proved fatal on the fourth day after the operation. The discharge at this time certainly contained pus.

CASE II.—B. R., æt. 49, a washerwoman, admitted into Chalmers' Hospital on account of a large scirrhous tumour of the right breast, complicated with enlargement of a single gland in the floor of the axilla. On the 28th January, the mamma was excised, along with the cellular and glandular tissues in the axilla, as high as the level of the pectoralis minor. Six needles were employed to arrest the bleeding, which was very copious, and the wires attached to the needles were brought out through the most dependent part of the lower flap by means of a common surgical sewing needle. Wire sutures introduced, and pads of lint, and a sponge in the axilla, were employed as dressings. Two days after, these dressings were removed and the needles withdrawn, when the whole extent of the flaps was found adherent to the parts beneath, except one small portion of the margin of the lower flap in the centre of the pectoral incision, and another in the axillary wound, where originally accurate approximation could not be effected. These, however, healed speedily. The glands, however, in the upper axillary and subclavian regions, inflamed, and the latter suppurated, requiring an incision in the subclavian triangle to evacuate the matter which had formed. This patient returned to hospital a month after dismissal to report herself well. The whole pectoral and axillary regions were sound, and not a trace of glandular swelling in the upper axillary or subclavian region could be detected.

CASE III.[1]—J. N., a letterpress-printer, admitted into the Royal Infirmary January 18, 1865, on account of diffuse phlegmonous suppuration of the hand, wrist-joint, and lower third of the forearm, consequent upon a poisoned wound of the middle finger. An attempt was made in the first instance to save the hand by making several counter openings, affording support by means of lint applied in the form of a many-tailed bandage, while the limb was supported upon a splint. As the condition of matters locally did not improve, while the health and strength were becoming rapidly exhausted from the copious discharge, the patient assented to the removal of the limb by amputation. This was effected under chloroform on the 9th February, the operation being performed in the middle-third of the forearm by a double flap. Five needles were employed to arrest the bleeding, and the forearm and flaps having been supported by means of cork splints, several deep sutures, penetrating both flaps and splints, were introduced and fastened off in pairs upon the outside of the corks. Only a few sutures were inserted along the margins of the flaps, as it was deemed expedient that all oozing should cease before the edges of the wound were accurately closed. At evening visit, as all oozing had stopped, my house-surgeon Mr Moir introduced the necessary sutures along the

[1] *Vide* The Lancet, April 1, 1865, p. 333, "Case 1."

margins of the flaps; and as the patient wished chloroform for this, Mr Syme's resident surgeon kindly administered the anæsthetic while the sutures were introduced. No dressing of any kind was applied, the whole line of incision being left exposed to inspection. The accessible needles, two in number, were removed twenty-four hours after the operation: the wires connected with the others, being completely encrusted with the serum which covered the line of union of the flaps, were left undisturbed. On the fourth day after the operation, a slight serous weeping escaped from the outer angle, which crusted upon the lower splint. In order to determine its nature, a bit of lint was applied at this point to collect the scanty discharge for examination with the microscope. It was found to consist of serosity, with a few pus corpuscles floating in it. This fluid continued in small quantity to be discharged for a fortnight, when it entirely ceased; but at no period could it, from its naked-eye character, have been affirmed to be pus. Twenty-two days after the operation, the cork splints, deep and superficial sutures, which up to this time had been left untouched, were removed. On picking away the crusts which still adhered to the line of union of the flaps at the inner angle, the three wires attached to the needles introduced on the ulnar side were disclosed. On withdrawing them, neither blood, serum, nor pus escaped. The stump was, in fact, perfectly sound and painless: it was wrapped in cotton, and supported with a sling for a few days longer, during which no discharge of any kind occurred.

The patient has since returned to show his stump: there is neither change in the form of the stump nor puckering at the outer angle from which the serosity flowed.

CASE IV.—On Saturday, February 4, 1865, I removed the foot at the ankle in a patient of Dr MacWatt of Dunse. The case was one of necrosis of the os calcis, with extensive inflammation and suppuration of the bones and joints of the tarsus. A large opening with extensively-undermined edges existed over the outer side of the os calcis and astragalus. The operation was performed by means of the large internal plantar flap, as recommended by the late Dr Richard Mackenzie. No vessel spouted, but copious oozing ensued from several parts of the surface of the flap. This was readily checked by means of six needles with wire loops. These were removed at the end of forty-eight hours. A portion of the margin of the flap sloughed, and copious suppuration of the whole surface ensued, which lasted for a fortnight. After this had apparently ceased an abscess formed in the hollow of the stump, from which I am informed that some discharge still continues (April 11).

CASE V.—A. M., æt. 30, admitted into Chalmers' Hospital, 3d February 1865, on account of old standing disease of the right knee-joint. The joint was acutely painful, especially at night,—the limb was shrunken, the knee flexed nearly at a right angle and rigid. Excision of the knee-joint, performed on the 8th of February,

by the long semi-lunar flap incision retaining the patella. Bleeding very free, but readily arrested by means of ten needles. No dressing to the line of wound. The limb adjusted upon a gooch-splint, extending from the buttock to beyond the heel, the lower end of which was bifurcated to avoid pressure on the calcaneum. An iron rod, applied along the front of the limb from the toes to the groin, was, along with the gooch-splint, retained in its position by a gypsu.n bandage, as a means of slinging the whole limb. In the evening, as all oozing had ceased, the requisite wire sutures were introduced. The needles were all withdrawn the following day. For ten days, serum continued to ooze away from the intervals between the stitches at the most dependent parts of the incision. On the eleventh day, for the first time, a few pus corpuscles could be detected by means of the microscope in this scanty discharge, and they continued to be recognised for three days, when all discharge entirely ceased. Two months have now elapsed since the operation. The original apparatus has never been touched since its application till to-day (the sixty-second day), when not only were the soft parts found firmly healed, but the bones united together. Not a morsel of dressing has ever been applied to the wound, and the wire stitches were only removed a fortnight before, as then the crusts had spontaneously separated, leaving a sound and dry surface beneath.

CASE VI.[1]—J. W., æt. 40, a goods guard, N. B. R., was admitted, 23d February, into the Royal Infirmary, on account of a compound comminuted fracture of the bones of the right leg, and a similar injury of the os calcis of the left foot. This accident was produced by a goods train passing over his limbs. When admitted, the bleeding was so copious and general that the resident surgeon applied tourniquets to both limbs above the knee. These, though apparently thoroughly applied, did not stanch the bleeding, which was still going on when I reached the hospital. Amputation was performed below the knee, by means of a posterior flap in the right leg, and at the ankle by means of a large flap derived from the dorsum of the tarsal region in the left limb. The bleeding vessels were secured by needles. The flaps brought into position and maintained there by wire sutures. As slight oozing, apparently venous, came on during the night after the operation, elevation of the stump and application of lint wrung out of cold water was employed. Next day, as the patient was sleeping, starting of the right stump occurred, followed by bleeding, which distended the cavity of the flap. At the hour of visit, on opening up the stump, the bleeding was found to come from vessels which had not bled at the operation, and to which no needles had been applied. Needles were employed to secure their bleeding orifices. The original needles were at the same time removed, and no bleeding followed. On the third day, as slight oozing came from the left or ankle stump (the needles of which were untouched), the

[1] Vide The Lancet, April 1, 1865, p. 333, "Case 4."

sutures were removed, and a small vessel which bled freely was secured by means of a needle. On removing one of the original needles, at the same time as a gentle trickling stream of blood showed itself, certainly not more than might come from a granulating surface when touched, a needle was introduced, and a loop of wire employed to secure its arrest. A small portion of the flap of the leg stump having manifestly sloughed, and as matter was collecting in its cavity, several sutures were removed. In effecting this, a small arterial branch in the sural mass feebly bled, and was at once secured by means of a needle. From this period there was no return of the bleeding and no extension of the sloughing. The patient also continued to take food, to sleep, and in other particulars apparently to go on satisfactorily, till an attack of erysipelas of the head and neck occurred in connexion with a slight scalp wound which he had sustained at the time of the injury. From this he sunk on the ninth day after the accident.

His body was examined after death, but without discovering any pyæmic lesion. On dissecting the larger arteries in both stumps, which had been secured by needles, conical clots of pale rosy fibrine were found extending upwards from the cut surface of each vessel, and terminating in a small point or in a long filiform extremity ; the length of the fibrinous clot varied from an eighth to a quarter of an inch. On the surface of the stumps, the extremity of the artery was softened and flaccid as though deprived of its inner coats, presenting at the same time the ash-grey appearance, characteristic in the dead body, of all wounds which have suppurated.

CASE VII.[1]—W. S., æt. 46, a farm-labourer, was admitted to the Royal Infirmary on 13th March, on account of a large chronic abscess, occupying the lower part of the arm and the whole of the forearm as low as the wrist. From the history of the case, it was manifestly one of gelatinous disease of the synovial membrane of the elbow-joint, ending in suppuration with destruction of the articulation. On opening this large abscess on the day after admission, a quantity of curdy pus was evacuated, and the condition of the joint explored with the finger. As the cartilage was found peeling off the ends of the bones, and the articulating surfaces undergoing acute inflammation, attended with extreme pain and feverish excitement, I deemed it expedient, as the surest mode of relieving the tension, to excise the elbow-joint. This was effected in the usual way by the linear incision, on 17th March, the only point deserving notice being the greatly-thickened state and vascular condition of the soft parts, especially of the periosteum and also of the bone. The bleeding having been satisfactorily stanched by means of eleven needles so far as arteries were concerned, as free general oozing still continued, the limb was laid upon a pillow and left exposed till evening, when the requisite sutures were introduced, leaving, however, a free opening at the lower part for the escape of the

[1] *Vide* The Lancet, April 1, 1865, p. 333, " Case 5."

copious and fetid discharge which was coming from the original abscess sac. On the day after the operation, the patient was very well, being free from pain, and having rested well during the night. His appetite was, however, but poor, as the chloroform had made him very sick. The needles were removed; and as the discharge was both very copious and offensive, water dressing, in the form of a many-tailed bandage, applied from the wrist upwards. All went on satisfactorily till the evening of the third day from the operation, when, after taking a hearty supper, he was seized with a violent epileptic attack. During this he was quite insensible, and threw his right arm about, unconscious of the injury he was inflicting upon it.

As the convulsions gradually subsided, he sank into a state of profound coma, which seemed likely to prove fatal within a few hours. During this period of insensibility his urine was drawn off, and, on being tested, was found to contain no albumen. After lying in a comatose condition for a day, he gradually emerged into a state of furious mania, requiring him to be held down by several attendants. This condition, with intervals of repose, continued till the fifth day, when he gradually fell into a torpid state, from which he could with difficulty be roused. The arm, after the rough usage to which it had been exposed during the epileptic and maniacal paroxysms, was much inflamed, the whole length of the incision gaping, leaving the ends of the bone exposed. The discharge (coming, however, chiefly from the abscess sac) was very fetid and copious. After this the patient, although much exhausted, began to rally under treatment directed to the head affection and the maintenance of his general health, till he was attacked by erysipelas of the right flank and buttock, under which he succumbed on the twenty-fifth day from the operation.

The examination of the body after death revealed a few dark softened patches in the spleen, which were undoubtedly secondary abscesses in process of formation. A clot was found occupying the posterior branches of the internal iliac vein, which extended upwards into the common iliac trunk. The brain was very œdematous, both ventricles containing a quantity of clear serum. The choroid plexuses, almost bloodless, were affected with cystic changes. There was no appearence of embolism in any of the intercranial vessels. In some of the convoluted tubercles of the right kidney there was exudation, and the epithelium was in a state of cloudy swelling.

Case VIII.—On the 13th of March, I operated by excision upon a patient of Dr Lorimer of Haddington, on account of a simple cystic tumour of the right breast. The arterial bleeding was arrested by the use of five needles. These were removed in the course of the next day. The whole line of incision healed by the first intention, except at the point where the needles were withdrawn.

From this, matter continued obstinately to be discharged from a

small abscess which had formed in the line of incision, till a more direct opening was afforded by the knife for its escape, when the aperture healed at once.

CASE IX.—I removed a large adenoid tumour from the sub-maxillary region of Mrs ——, employing five needles to arrest the arterial and venous bleeding. These were withdrawn next day, and as no dressing had been employed, the serous oozing crusted upon the edges of the wound. Two days after, some redness of the surrounding skin having occurred, water dressing was applied. A small quantity of purulent discharge, barely sufficient to stain the dressing, along the line of incision, escaped for two days more. After this, as the blush of redness had disappeared, the surface was again left exposed, and a crust allowed to form. From this period no further discharge occurred, though the patient was kept under observation for a week, until the crust had completely separated.

*Remarks.*—At first sight, looking at these cases, it is apparent that here are nine cases of acupressure, with three deaths; and according to this common method of estimating the success of certain great operative procedures, such a result would be tabulated as a fatality in cases of acupressure of 33 per cent. But as hardly the most vehement opponents of this means of checking hæmorrhage are inclined to go the length of charging upon it such disastrous consequences, it is unnecessary for me to say more on this aspect of the facts, than merely to show that this fatality was in every instance independent of the surgical proceedings resorted to for the relief of urgent disease.

In the first case (that of a strangulated inguinal hernia) the form of the affection,—congenital in the adult,—the protrusion occurring into the tunica vaginalis testis as its sac, is sufficient to indicate the likelihood of a fatal issue, especially when we consider that the protrusion had taken place on this occasion for the first time, that the apertures were very rigid and but slightly dilated; that the hernia had been incarcerated for certainly eight hours, and strangulated for at least two; that at the time of the operation the sac contained no fluid, and that the bowel was tightly constricted in no less than three situations. Furthermore, the fatal peritonitis was in all probability already established before the operation for the relief of the hernia was undertaken, as turbid fluid escaped from the abdominal cavity after the contents of the sac had been returned.

The second fatal result, in the case of the railway guard, occurring after amputation of both limbs, consequent upon a severe railway injury, attended with great constitutional shock, a long transit, with consequently some hours' delay after the accident, and considerable loss of blood, is nothing more than what is usually observed in such circumstances. To the question of the recurrence of hæmorrhage after the amputation, I shall again advert; but meanwhile I mention

it merely to say, that its amount was so insignificant as to be incapable of seriously influencing the ultimate result.

In the last case which proved fatal, that, namely, of the farm-labourer with disease of the elbow-joint, and a large chronic abscess of the forearm and arm,—to all appearance the determining cause of death must be found in the epileptic attack, first followed by deep coma, and then succeeded by a violent state of maniacal fury. It was during this period of deep stupor, followed by violence, that the bed-sore formed over the sacral region which originated the erysipelatous affection. That this was the direct cause of the pyæmic affection from which he died was rendered probable from the fact that, while there was no coagulum contained in any of the larger veins of the right arm, the right internal iliac vein was occupied by a coagulum originating in its posterior branches. When the epileptic convulsions, followed by stupor and then by mania, first appeared, I imagined either that this was due to some embolic cerebral lesion or to uræmic poisoning. No cerebral embolism, however, was found to exist in the post-mortem inspection; and the theory of uræmia, although so far borne out by a trace of albumen being discovered in the urine for a day or two, received no confirmation in the condition of the kidneys.

Passing from the consideration of these fatal cases, because, as I have already said, the unfortunate result is in no respect attributable to the employment of acupressure instead of the ligature, I would wish to consider what bearing these cases have: 1st, Upon the question of acupressure as a safe hæmostatic agent; 2d, Upon the utility and application of acupressure as contrasted with ligature, and other hæmostatics; and, 3d, Upon the prospect it affords of expediting the process of healing by the first intention.

1. Is acupressure then a safe hæmostatic agent? This question, so far as my experience goes in its employment, I must answer in the affirmative. I should speak with submission, as no doubt I have never had any opportunity of employing it in any vessel larger than the posterior and anterior tibials, radial and ulnar arteries. My experience, however, such as it is, would lead me to have no hesitation in resorting to its use in the largest arteries which require to be secured in amputation, such as the axillary or femoral. Nay, such is my complete confidence in the trustworthy nature of this measure, that I should be inclined to employ some modification of it in preference to the ligature in the operations upon the continuity of arteries in the treatment of aneurism. Before leaving the hæmostatic capabilities of acupressure, I must say a word in regard to Cases Nos. 3 and 6, which have been alluded to in Mr Syme's summary[1] as examples of hæmorrhage following the use of the needle.

In regard to Case 3,—the amputation of the forearm,—the bleeding from the flaps was easily stopped by the employment of needles,

[1] *Vide* The Lancet, April 1, 1865, p. 333.

and when the surfaces were brought in contact there was nothing but a scanty oozing, such as must occur in every wound of similar extent. To have brought the edges and surfaces of the flaps in contact, however, while even this was going on, would not have facilitated union by the first intention, and, as to obtain this was my chief aim, I left the edges of the flaps gaping in the intervals, between two and three points of suture, which were inserted only for the purpose of equalizing the margins. From the surface of the flaps there was undoubtedly no further bleeding of any moment; but in passing the deep sutures through the flaps, some arterial blood flowed along the track of one of the wires when it was first introduced, and came, I am inclined to believe, from the radial artery, which had been injured in the passage of the long needle. As the flow ceased as soon as the sutures were tightened, and as the trick-ling stream was arrested, when I saw the patient after his removal to bed, as, furthermore, my house-surgeon informs me all bleeding of any kind had entirely ceased some time before the hour of even-ing visit, and, as certainly no means were deemed necessary to arrest any oozing which did occur, as certainly Mr Syme's house-surgeon only saw the case—not on account of bleeding[1]—but to give the patient chloroform, while the additional sutures were inserted, I am compelled to conclude that there was no "after bleeding" of any consequence, certainly not more than is usually seen in cases of amputation, certainly nothing which needed inter-ference, and assuredly that which did occur was altogether uncon-nected with the employment of acupressure.

In Case 6, there was bleeding repeated again and again from trifling vessels which did not bleed at the time of the operation, which were not previously secured by means of needles. This hæmorrhage, therefore, was altogether independent, and unconnected with the use of acupressure. In the removal of one needle and its wire loop, some oozing did ensue, and in this case I certainly did re-introduce the needle and re-apply the wire, not because this was absolutely required, but because the patient had already manifested such a proneness to bleed from any open surface that I deemed it expedient to secure myself and the patient against even the possi-bility of any further oozing. Had I deemed the needle insecure and unsatisfactory, it is scarcely likely in such circumstances that I should have re-employed it, instead of resorting to the ligature. But even supposing in this example, copious hæmorrhage had ensued when the needles were removed from the larger vessels, which was not the case, it still would not have prevented me in happier circumstances from resorting to the employment of acupres-sure; for the nature of the flaps, and the consequent impossibility of obtaining accurate co-aptation of the cut surfaces, rendered the probability of the occurrence of hæmorrhage when the needles were removed, or while suppuration was going on, much greater than in the

[1] *Vide* The Lancet, April 1, 1865, p. 333, " Case 1."

instance of amputation of the forearm, already commented upon. In fact, I regard the case as a very severe test of the safety of the needles as a hæmostatic agency; and, as it stood this test so well, the dissection of the vessels exhibiting firmly adherent coagula, occupying their extremities, which required very considerable force to displace or to separate them from their attachment, I should be inclined to quote this case, not as an instance of the unsatisfactory nature of acupressure, so far as the arrest of bleeding is concerned, but as an instance of the trustworthy character of this procedure as a hæmostatic agency.

2. As to the applicability and utility of acupressure as contrasted with ligature and other hæmostatics. When acupressure was first proposed, the procedure consisted in introducing a long glass-headed needle from the cutaneous surface of the flap. This, after traversing the textures, compressed the orifice of the vessel, by passing across its track, and again emerging on the cutaneous surface. It, therefore, compressed the artery in a degree proportioned to the thickness and resiliency of the tissues, the directness of the ingress and egress of the needle, together with its own rigidity. Such a method was manifestly ill-suited for general application. Hence, I presume, it was that the statement was very generally accepted, that acupressure was limited in its application to a few situations. But modified as this agency has been in various different ways, I can conceive of no situation in the body where its employment cannot readily be resorted to. Certainly, of all these various methods, that is simplest in which a small-sized glover's needle, threaded with a loop of fine iron wire twisted into a single strand, or a common long hare-lip needle, is employed either to support a compressing loop of wire, or to obstruct the orifice of the vessel by rotation through the arc of a circle. There are situations too, such as the extirpation of tumours, or on plastic operations on the face, where, while the dissection is proceeded with, the bleeding, arterial and venous, is readily checked by passing a common pin, or, better, a steel pin with a glass head, in at the bleeding orifice, till its head compresses the cut surface. I have in several examples employed this method, and have had much reason to be satisfied with it, because it certainly saved loss of blood, while it avoided the necessity for the fingers of assistants embarrassing the operator, by being employed either mediately or immediately in compressing the bleeding vessels. After completing the operation, these pins are removed, and should the vessels again bleed, one or other of the more permanent methods of acupressure may then be adopted. The needles are of course more readily employed in some situations than in others. In open exposed surfaces, rather than narrow deep wounds, in flap operations performed by transfixion, than in those by the circular method, or where the mouths of the vessels are cut even with the muscular tissues in which they are embedded. But even such circumstances offer no insuperable obstacle to their employment. The interosseous arteries in the forearm, the anterior and posterior tibials and peroneal

in the amputation in the leg, certainly gave me no trouble in employ-ing acupressure. There is one situation in which bleeding is some-times troublesome, and where a needle seems to afford a satisfactory means of stanching the arterial flow. I mean when the hæmor-rhage obstinately comes from the nutrient artery of a bone, or from an unusually active branch of the vessel. In such circumstances a needle affords an easy means of stopping the bleeding, and may surely be admitted to be a very preferable hæmostatic to the actual cautery, a compress of lint or a peg of wood attached to a ligature, all of which I have in such circumstances seen employed. The use of the needle, however, has appeared to me to be much more easy even in cases where the ligature can easily be employed. To use a ligature properly, the mouth of the bleeding vessel should be seized by the forceps or tenaculum, and secured by the ligature, including in its noose as little of the extraneous soft parts as possible. No such accuracy is required in the use of the needle in any of the methods recommended for its application; while, in acu-pressure, the degree of compression of the tissues is so slight, and its necessary duration so short, that no ill can arise from it, what-ever be the nature of the textures included. I do not think, there-fore, that difficulty of application is an objection to acupressure likely to arise in the mind of any one who has had an opportunity of employing it.

In the case of the ankle-joint amputation (No. 4), it will be observed, that while the bleeding was very copious, it came from no vessel of such size as to admit of the satisfactory use of a ligature. Possibly it may be said that sponging cold water over the wound, or leaving the flap open for an hour or so, would have sufficed to stop any such bleeding. I cannot say that this has been my experience in similar cases, where the bleeding, though free, came from so many open points as to render the use of ligatures to all of them out of the question. Certain it is, that the needles afforded a remarkably easy means of securing the satisfactory stoppage of this continuous and copious flow from a number of extensive patches of surface. Again, in sloughing wounds or sores, and in cases of secondary hæmorrhage, I think there will be little objection taken to the reasonable anticipation that acupressure will prove more satisfactory than ligature; and, again, in bleeding from the palm of the hand or sole of the foot, either in the form of acu-pressure or of filopressure,[1] by means of a curved needle and wire suture we possess a means of stopping the flow more easily applied than the ligature, and neither so painful nor so likely to cause sup-puration as the graduated compress. As compared with torsion, the necessity for the removal of the needles may be urged as an objection to the use of acupressure; and no doubt it is, to a certain

[1] Acupressure, by J. Y. Simpson, M.D., pp. 320, 334, 348. Hilliard, Medi-cal Times, 7th Feb. and 7th March 1863. Dix, Edinburgh Medical Journal, September 1864, p. 214.

extent, a drawback, because, however gently effected, it must in some degree interfere with the accurate apposition of the surfaces which have been approximated. Should there, however, be any object to be gained by leaving the wound completely undisturbed, this might easily enough be effected, either by leaving the needles in till the wound is healed, or by taking care that in the introduction of the needles they shall lie so completely horizontal with the opposed surfaces of the wound, and so directly in the axis of the opening in the margin through which they are to be withdrawn, as to avoid any undue disturbance in their removal. In this manner, without force, the needles will admit of extraction at any time from a few hours up to days or weeks after the operation as may be thought most expedient.

In the amputation of the forearm (Case 3), some of the needles were taken out early, some were left untouched till the sutures were removed, and in the latter instance they seemed to create no irritation of any kind, while from the side in which they were left undisturbed there was less discharge than from the other. In all the cases a very moderate amount of attention enabled the needles to be inserted in such an axis as to lay them in a position most suitable for easy removal. Without entering upon the question as to the immediate or remote effect of torsion upon the twisted end of the smaller arteries, there can be no doubt, in the case of vessels the size of those of the forearm or leg, that if the process of torsion is to be really efficacious in arresting all further bleeding, a considerable portion of the extremity of the vessel must be so injured as to ensure its dying and separating as a slough, while in those cases of my own, and others where I have had an opportunity of dissecting the parts after the employment of the needles, no slough whatever appeared to have formed.

The effect of the ligature in producing loss of vitality of the small portion of the vessel, and other soft parts beyond the embrace of its noose is, I suppose, undeniable; and although it may be of small moment when the ligature remains in the wound till the separation of the little morsel of dead tissue is complete, and it has floated away in the discharge of matter which continues to find its way along the ligature, in the case of the twisted vessel it is far otherwise. Here the portion of vessel which has been injured by the torsion, more nearly resembles in its condition that of the noose of a ligature which has had both ends cut off short after the deligation of the artery. In such cases, as is well known, should the wound seem to have healed kindly, a small abscess sooner or later forms in connexion with the foreign body, and, by its seeking the surface, breaks up the union which had already taken place. In considering the effects of torsion as contrasted with acupressure in small vessels, as affecting union by the first intention, we are apt, when torsion is applied, to be deceived by the fair union of the external surface of the wound, and to imagine that it is the same throughout its whole

depth, and thus to conclude that the extremities of the twisted vessels have healed kindly along with the rest of the cut surface. That there may be no sloughing of the extremities of these minute vessels after torsion, I am not prepared to deny, but that it is necessarily so because the union seems so perfect externally, I am not prepared to admit.   In those very cases where the union by the first intention is most readily obtained, and is most perfect in appearance upon the surface, it is so just because there is another surface of the wound in which the union is much less perfect, and from which a ready escape is afforded for blood and serum in the first instance, and for more or less suppuration afterwards.   The pus, then finding its way most readily in the direction of the deepest surface, does not interfere with the accuracy of union on the cutaneous surface. If, in such cases, torsion of the bleeding vessels has been employed, and sloughing of their extremities ensues, the small fragment of detached tissue escapes inwards along with the pus, and leaves the surface unaffected.   The union, then, in such cases by the first intention is rather apparent than complete.   The surface aspect of the wound does so heal, the deeper portion suppurates and granulates, I will not say always, but certainly very frequently.

3. The only remaining question to be determined is the influence of acupressure on the union of wounds in the cases described.   In determining this result it is essential to understand the use of terms, for many differences of opinion in the matter of union by the first intention, union by adhesion, or primary union, are undoubtedly due to the varying significations attached to these terms.   In strict application they should be reserved for wounds which heal without the formation of a particle of pus.   From what I have just said under the previous head, it is plain that such union occurs but rarely, that though a part of a wound frequently heals in this manner it is but a portion, not the whole, and that those instances which are most frequently quoted, as affording a pattern example in this way, are exactly the cases where the suppurating part of the process is least easily observed, and where the union by adhesion is most obvious.   I do not quite go the length of some continental authors who deny *in toto* the existence of union by the first intention, except to a most limited extent in most favourable circumstances, but I am inclined to believe that the entire absence of all trace of suppuration has been too readily taken for granted.   In the careless and incorrect use of pathological language, which is perhaps too common with us all, cases of plastic operations upon the lips and cheeks are usually referred to as examples of the most satisfactory union we can obtain when we have adhesion without the formation of a drop of matter.   But while this is undoubtedly perfectly true as regards the cutaneous aspect of such wounds, it is, I am convinced from observation, not true as regards the mucous aspect of the wound from which matter does escape, but mingling with the saliva, and being swallowed, is apt to elude our notice. In the instance of any incised wound, where there is no great

tension, nothing is more easy than to obtain a similar amount of primary union, if we bring the cutaneous edges together with equal care; but here, there being no second surface corresponding to the buccal cavity in wounds of the cheek and lips from which the pus may escape unperceived, the consequence is, that it is pent up within and forms an abscess. When, then, we wish to obtain union by the first intention to as great a degree as possible, in the case of large flesh wounds or amputations, our principal object, it appears to me, should be to regard the cutaneous surface as the secondary one which should be sacrificed to the accurate adhesion of the deeper parts, so that while they heal, from it the blood and serum, and afterwards the pus—should it form—may obtain a free escape. It was with this view that in Cases 2, 3, 5, and 9, I adopted measures calculated to obtain such results. The progress in these cases was certainly to my mind, and to that of all experienced observers who saw them along with me, very different from what I had ever previously obtained; and in my description of the progress of these cases my object has been not to set them in as favourable a light as possible, but to give the facts which might be described as unfavourable as great a prominence as possible. In these cases I have no hesitation in saying that union occurred as thoroughly by the first intention as is ever seen in plastic operations upon the lips and cheeks, and certainly with an entire absence of anything that could be described as characteristic of the local or constitutional symptoms of inflammatory action. As to the other cases, where undoubtedly suppuration occurred, and in some copiously, the result need excite no surprise, as a few words will suffice to demonstrate.

In Case 1, of hernia, the parts operated on and cut through were acutely inflamed: nothing short of suppuration could therefore have been expected.

In Case 4, of amputation at the ankle, the hollow form of flap in this amputation does not admit of accurate apposition in any circumstances, and therefore, for that reason alone, suppuration must have taken place, but especially so in this instance was it likely to occur, where from the extensive formation of matter in connexion with the disease of bone, a part, and that a considerable part, of the interior of the flap had already suppurated. The sloughing of a portion of the flap which ensued was where the soft parts had been undermined by the progress of the pus towards the surface during the advance of the necrosis.

In Case 6, amputation below the knee and at the ankle, suppuration occurred simply from the want of accurate apposition of the surfaces of the flaps with the parts with which they were brought in contact: this apposition being impossible from the site of amputation and the nature of the flaps. The sloughing of a portion of the flap in the amputation below the knee being manifestly due to its being involved in the original injury, although at the time of the amputation there was no symptom of any such extension of the effects of the accident as to render it justifiable to resort to amputa-

tion of the thigh rather than attempt to save the knee by the operating on the leg.

In Case 7, excision of the elbow-joint, that suppuration after the operation should occur where a large abscess sac already existed in connexion with the disease of the articulation, and when the patient had, for some days, tossed the limb about during convulsive and maniacal seizures, is certainly not more than might have been anticipated, and may, I think, in such circumstances, find a more easy explanation than in the fact that needles were employed to check the bleeding.

In Case 8, suppuration certainly took place in a most limited area of the hiatus left by the removal of the cystic tumour of the breast, and was largely due to the want, on our part, of that accurate application of pressure to keep the flaps steadily fixed in their position upon the surface of the pectoral muscles, by which alone any hope of perfect primary union could be realized.

The sum of the whole matter, it seems to me, may be expressed in the following statements :—

1. Acupressure may be employed as a hæmostatic agency in the instance of vessels as large as the posterior tibials without risk, so far as bleeding, immediate, consecutive, or secondary, is concerned.

2. Acupressure is easy of application, while its adaptation to situation and circumstances is as great, or it may even be greater than that of the ligature.

3. Acupressure is more trustworthy and satisfactory than either torsion, compression, or the use of the cautery.

4. Where primary union can take place, acupressure is likely to favour its occurrence; and, in three of the cases described, seemed of material service in this respect.

5. In the cases described, where suppuration or sloughing occurred, this was altogether independent of the use of acupressure, or the absence of the employment of the ligature, and referable to causes inherent in the individual cases.

6. In my opinion, the employment of acupressure in one or other of the methods suggested by Professor Simpson is safe, satisfactory, and well worthy of an extended trial.

---

ARTICLE VII.—*Account of a Roseoloid Exanthem in Malta, during the Autumn of* 1864. By FRANCIS H. WELCH, Assistant-Surgeon, 1st Battalion 22d Foot.

AMONG the minor ailments to which a resident in Malta is liable is one that up to the present time has received but little attention. The only native physician who has brought forward the subject is Dr Gulia, in a series of papers in one of the local weekly press. Its description, as exemplified amongst the natives, coincides very much with my own observations gleaned in the 1st Battalion 22d Foot, varying only in the minor details; consequently the greater

part of what I shall adduce has double and independent testimony to support it. The disease in question has hitherto been known as " spurious measles," and considered contagious; but I think sufficient facts will be brought forward to convince any one that it is in no way related to morbilli, nor communicable from one to another.

Whether the disease existed prior to the autumn of 1862, or whether, as some suppose, it has always occurred annually, only assuming within the above-named period an eruption different from that by which it was formerly manifested, there is no evidence to produce. I can only say positively that I first saw it in 1862 in three or four individual cases; in 1863 more frequently, and during last autumn at least 70 cases have fallen under my notice.

My reason for writing on the subject is chiefly from a desire to draw attention to it for further elucidation, especially with respect to its cause; and also, as an epidemic of morbilli has succeeded it in the 1st Battalion 22d Foot, the opportunities for contrasting the one with the other have been very favourable.

My attention was first drawn to it by seeing a man admitted into hospital with much general febrile disturbance, and excessively acute pains across the loins and temples, the latter causing much lachrymation. The following day, tumefaction of the lips, eyelids, and ankles was superadded, and the pains were augmented; his urine was high coloured and cloudy, but when tested no evidence of kidney disease was obtained. The case was obscure. On the third day the man was covered from head to foot with the characteristic eruption, and expressed himself greatly relieved; but beyond the fact that we had to deal with an exanthem, we were as much at a nonplus as before. However, in two days, the eruption began to subside, and a feeling of intense weakness succeeded; this overcome, the man returned to duty, no sequelae following. Two or three cases of a milder character occurred, but no especial notice was taken of them, and the same course was pursued during the succeeding autumn; but this year, owing to its greater frequency, my attention was forcibly directed to it, receiving a great impetus from experiencing its unpleasantness in my own person.

There is a decided concurrence of opinion as to its absence during the summer, its onset about the month of September, and its fading during the winter. Its course, when an individual is affected, is characterized by the following symptoms:—After two or three days of general indisposition marked by lassitude, febrile disturbance, disinclination to eating or exertion, severe neuralgic pains across the loins, sacrum, and temples, constant watering of the eyes, and occasional oppression of the chest and epigastrium, frequent rigors occur. At the same time the person experiences a tingling sensation of the skin, very similar to the sting of the common nettle; he rubs himself, the heat of the skin increases, and with it this unpleasant feeling. This leads him to look at the irritating part, and to notice the presence of the eruption covering his whole surface.

With the advent of the eruption, the pains, oppression, and general " malaise," are invariably relieved, and often entirely disappear,— the eruption itself, and the accompanying stinging sensation, terminate gradually after having continued about two days.    The convalescence of the patient is retarded by a weakness that is often very great, and that bears no proportion whatever to the duration or severity of the affection; however, this is soon recovered from, and the person returns to his usual employment, no sequelæ following. Five days may be considered as the average duration of the affection, and two days' confinement to the house.

This is the usual course of the disease.    I have noticed only two cases (Dr Gulia mentions several) in which the febrile disturbance was very great, with tumefaction of the skin, and exceedingly acute neuralgic pains.    The majority were of such a mild character that one day's confinement to hospital was only necessary, and occasionally the men continued at their duty and braved it out.

The whole surface of the body is implicated by the eruption at the same time, the inner portion of the wrists, elbows, and knees the most.    It is decidedly erythematous; it consists of small, roundish, invariably rose-coloured spots, about the size of a large pin's head, with slight intervals between them; occasionally these spots coalesce, and form patches of very varying shape and size. It disappears completely on pressure.    In two cases it was accompanied by tumefaction of the skin, and in a few, when much friction was employed, a weal, almost resembling urticaria, was produced.    Dr Gulia describes several, in which he states " that the eruption had somewhat the appearance of urticaria, but yet was distinct from it."    This condition, I believe, entirely depends upon the irritability of the patient, and his rubbing himself to get relief, and is in nowise a primary characteristic of the exanthem.    I have noticed no case in which the mucous membranes were involved,[1] no after result beyond the intense weakness and an occasional increase of epidermic desquamation.    Beyond the urine, which contains an excess of acid, the secretions of the body are not altered.    As may be supposed, persons of delicate skin and of a nervous temperament suffer most from the eruption and its accompanying tingling sensation.

The treatment requisite is of the mildest kind,—warmth, hot drinks, and diaphoretics aid much the appearance of the eruption, and consequently accelerate the progress of the affection and the occurrence of relief to the general symptoms.    Care to avoid exposure to cold, and attention to the excretions are necessary.    In two cases it was deemed desirable to abstract blood for the relief of the lumbo-sacral pains, and to administer narcotics.    When the eruption is out, warm baths or warmth in any form only increases the irrita-

---

[1] The watery condition of the eyes has been brought forward as a proof of involvement of the mucous membrane, and as showing an affinity to measles, but the conjunctivæ are in nowise injected, and the lachrymation departs, *pari passu*, with the neuralgia of the temples.

tion. Good nourishing diet and tonics may be necessary for the subsequent debility.

In a few cases, there is, for some weeks, a great tendency on the part of the skin to take on this tingling on exposure to warmth, or whilst under exertion, or any emotion accompanied by capillary fulness.

This exanthem invariably attacks adults in preference to children, and males more frequently than females. The affection once incurred predisposes to a recurrence. A prior attack of measles does not exempt from it, and previous immunity from that malady does not predispose to it. That the disease is not contagious there is abundant evidence. Numerous cases have fallen under my notice where the most satisfactory conditions for the propagation of it from one to another existed, and yet no transference took place,—in no case have I traced it from the parents to the children, or *vice versa*. Why I believe it has been characterized as contagious is chiefly from a supposed similitude in all its details to the exanthem whose qualified name it bears. With respect to the inapplicability of the term "spurious measles," there can be no doubt. Beyond the fact of both belonging to the exanthematous class, and a certain resemblance in the character of the eruption, they have nothing in common, as will be seen from the subjoined contrast :—

| *The disease in question.* | *Morbilli.* |
|---|---|
| (1.) Invariably attacks adults in preference to children. | (1.) Usually attacks children and the young. |
| (2.) Commences with febrile disturbance, but no appreciable involvement of the mucous membranes. | (2.) Commences with febrile disturbance ; — involvement of the mucous membranes of the eye, nose, mouth, bronchi, and intestinal tract, with derangement of the secretions. |
| (3.) Eruption about the second day. | (3.) Eruption about the fourth day. |
| (4.) Involves whole surface of skin at the same time. | (4.) Commences on the forehead and face, and travels downward, generally taking three days for involvement of whole surface. |
| (5.) Colour, rose red. | (5.) Colour, raspberry red. |
| (6.) Eruption declines about second day after appearance. | (6.) Eruption generally lasts about six days. |
| (7.) Decline of eruption gradual, but from whole surface at same time. | (7.) Decline of eruption gradual, and follows same course as the involvement. |
| (8.) All constitutional symptoms invariably relieved by the appearance of the eruption. | (8.) Some constitutional symptoms are relieved and some exacerbated. |
| (9.) No sequelæ beyond the weakness. | (9.) Sequelæ of common occurrence. |

Besides this, at the subsidence of the eruption in the 1st Battalion 22d Foot, an epidemic of morbilli occurred, attacking the greater part of the children, and including those who had suffered about six weeks previously, and those who had not, in the most indiscriminate manner. And here it was curious to notice, that whereas the disease in question attacked the parents in preference to the children in the proportion of about 10 to 1, the measles were *vice versa*. This is opposed also to the opinion of those who think it a modified measles; for if so, surely those who were not guaranteed by a previous attack of morbilli would have come more under its influence and suffered more from it than those who were. But it was exactly the contrary which was noticed, and this beyond all doubt. It has been seen running side by side with true measles in an asylum, yet there was no difficulty in detecting the one from the other, neither did the one follow any general rule in predisposing to or preventing the onset of the other. One case I saw, in which the man had suffered from measles in his infancy: he came under the influence of the affection at its height, and then again succumbed to a second attack of measles in a severe form.

The disease in question is evidently roseolous, and ought to be classed as such, leaving to further time and inquiry the addition of some qualifying name to distinguish it from the numerous subdivisions into which the genus is divided. It has most in common with the Roseola æstiva of Wilson; and yet, apart from the time of its appearance, there are many reasons for not classing it as such.

With respect to its etiology, that which can be adduced is more negative than positive. All are agreed as to its autumnal onset about the commencement of the early rains, and to its gradual fading towards the month of November, as well as to its entire absence during the summer; so that the cause is evidently to be sought for in the conditions that exist during the period mentioned, and that are absent for the rest of the year,—conditions not limited to the cities of Valletta and Cottonera, but existent throughout the whole island. It cannot be due to the presence of moisture in the air alone, inasmuch as there is no correspondence between the intensity of the one and the other; for the same reason it cannot be placed to the already sufficiently severely slandered sirocco wind, nor to the two combined. Some have considered it due to the emanations of the mud dredged from the bottom of the harbour; but this process is in existence throughout the whole year, and limited to the vicinity of the cities, whereas the disease is seen in all the villages throughout the country, and is existent for only about two months of the year! Its cause is a general one, and widespread. My own impression is that it is due to emanations from decomposing animal or vegetable matter. During the summer, filth of all kind collects in the drains, cesspools, and on the surface; but, owing to the hot sun and the absence of moisture, it remains in a quiescent state. But no sooner do the first few showers of autumn

come, then with them all the conditions necessary for the free
putrefaction of this garbage; the air must of necessity be filled with
the emanations, and the roseola abounds.   But no sooner does the
rain descend in earnest and in large volumes, then all the surface
drains are swept clean, and with this the roseola abates, almost
entirely disappearing about the month of November.   That these
are the conditions that exist in every habitable spot in the island
about the commencement and full setting in of the autumnal rains
I think no one will deny; certainly, with these conditions, and only
with these, does the roseola appear and take the course I have
stated; and if it be not entirely due to the miasmata, at least they
in some way are very intimately connected.

## Part Second.

### REVIEWS.

*Guy's Hospital Reports.*   Edited by SAMUEL WILKS, M.D.   Third
Series.   Vol. X.   London: Churchills: 1864.

GUY'S Hospital Reports have long enjoyed a well-merited reputation,
founded on the excellent practical and scientific papers which
they have contained.   Under the present editor there has been no
falling off, and the volume before us is worthy to take its place with
any of its predecessors.

The first paper is entitled " Fourth Report of the Guy's Hospital
Lying-in Charity," and is drawn up by Dr Braxton Hicks.   It
embraces a period of nine years, during which time 14,871 women
were attended.   It is satisfactory to find that there has been a
great improvement in the death-rate, which, during the period
under consideration, has been 1 in 340; the death-rate of the
former twenty years was 1 in 140.   This improvement is chiefly
due to the great diminution in the number of malignant cases of
puerperal fever.   The report gives some interesting information
regarding cases of induction of premature labour; placenta prævia;
retained placenta; version; instrumental deliveries; eclampsia;
and some other subjects.

The second paper, by Mr Bader, is on "The Treatment of Granular
Conjunctivitis by Inoculation with Pus."   This mode of treatment,
of a very troublesome and often intractable affection, appears to
have been first employed in the Austrian army about 1812; it seems
to have been frequently successful, but the severity of the inflam-
mation set up in some cases, which sometimes led to total loss of
vision, led to its abandonment.   Of late years the proceeding has

been revived, and a good many cases treated successfully by it have
been recorded.     Mr Bader, during the last seven years, has
treated about 157 cases (240 eyes) by inoculation, and the results
seem to have been in suitable cases very satisfactory.

Dr Habershon contributes the next two papers; the first "On the
Medical Preparations of Arsenic;" the second on "Two Cases of
Disease of the Supra-renal Capsules with bronzing of the Skin."
From the former of these, we extract Dr Habershon's conclusions
with regard to employment of arsenic.

"There are three forms of disease in which arsenic is of service :—(1) Mias-
matic poisoning; (2) diseases of the skin; (3) some diseases of the nervous
system; and in the treatment of these some general rules may be laid down for
the administration of arsenic.

"1. The preparations of arsenic are best given in solution; the medicine can
be more accurately measured and its dose better proportioned; in the fluid
state they are more readily absorbed, and the action is more efficient.

"2. The best time to take the remedy is soon after a meal; for in this
way any irritant effect is less likely to occur, and the drug may be increased
in quantity, and its use continued for a longer period than could otherwise be
the case.

"3. If it be desired to give arsenic with quinine, the acid solution in hydro-
chloric acid will be found to be a convenient form of administration.

"4. In states of great febrile excitement, especially when associated with
furred tongue, retained excretions, and congestion of the chylopoietic viscera,
arsenic is not well borne; and it is well to attempt the removal of these symp-
toms before commencing its use.

"5. In states of great irritability of the stomach and bowels, it is better to
postpone its use till those symptoms have been relieved.

"6. In diseases of the nervous system having among their symptoms a con-
tracted state of the pupil, with vertigo, arsenic is not generally of service.
Thus, some forms of neuralgic pain in the head are greatly relieved by arsenic,
but we shall often be disappointed in its efficacy unless the instances be care-
fully selected.

"7. If menorrhagia and dysmenorrhœa be present with maladies in which
arsenic might be of service, the former symptoms will often be aggravated by
its administration.

"8. Although strumous disease does not necessarily preclude arsenical medi-
cines, they are better avoided where much enlargement of the lymphatic glands
exist.

"9. In acute diseases of the skin, preparations of arsenic are often preju-
dicial.

"10. They are of but little service in true syphilitic eruptions.

"11. Disappointment in the efficacy of arsenic has often arisen from the
dose not being properly increased, and from the discontinuance of the remedy
before the disease has been thoroughly cured.

"12. Whilst the experience of the profession more than confirms its value
in chronic skin disease, in cancerous disease it is regarded as comparatively
valueless as an internal remedy, and dangerous as an external one.

"13. Although irritability of the stomach and bowels, as well as the mouth
and conjunctiva, may be induced by arsenic, these symptoms do not necessarily
compel us to discontinue its use, for a diminution of the dose, and admixture
with an opiate, may remove the symptoms.

"14. Where very minute and continued doses induce a general sense of
exhaustion, with compressibility of the pulse and loss of appetite, although
there is no irritation of the mucous membrane of the alimentary tract, the
arsenic must for a time, at least, be discontinued.

"15. The acid solution of arsenic may often be used very advantageously with the preparations of iron, and in some forms of chronic disease of the skin in strumous subjects the solution of the iodide of arsenic will be found an exceedingly advantageous form of administration, as recomended by Beitt, Thomson, Neligan, etc.

"16. The arseniates of soda and of iron have been recommended as milder in their action than the arsenites. There is some doubt whether the arsenic acid becomes changed in the system, for in an instance in which the arseniate of soda was given for several days, Dr Stevenson could detect none of the per-acid in the urine."

The two cases of supra-renal disease are very interesting, and are quite confirmatory of the observations of Dr Addison, Dr Wilks, and others. The first case was a girl nineteen years of age, who had suffered for two years from slight pain in the stomach, sickness, and gradually-increasing prostration. The skin gradually got dark in colour, so that when admitted into Guy's Hospital she presented the appearance of a mulatto. She remained in the hospital for two months, and then left unrelieved. She lived for nearly three years afterwards, and then died somewhat suddenly. On post-mortem examination, all her organs were essentially healthy, with the exception of the supra-renal capsules, which were converted into masses of "a white, low-organized product, surrounded by dense tissue; fatty and semi-cretaceous substance replaced the normal structure. Under the microscope, fibroid tissue, imperfect cell-growth, and highly-refracting granules were observed."

The symptoms and post-mortem appearances in the second case —a lad, aged 18—were very similar.

Dr Habershon regards the disease as influencing the system through the vaso-motor nerve. "The sickness, exhaustion, compressible pulse, and failing power of the vital function, are not peculiar to this disease of the supra-renal capsules; and even discolouration of a very similar kind is found in other maladies. Thus, in the exhaustion from long-continued lactation, there is great weakness, a compressible and irritable pulse, disturbance of the stomach, and very frequently patches of discolouration are seen about the forehead and face, as well as on other parts of the body. Here the vaso-motor nerve and the whole cerebro-spinal system of nerves are affected from exhausted uterine function; but the cause is a removable one, and the disease is therefore remedial; but in extensive deposit in the supra-renal capsule, the sense of irritation and exhaustion to the vaso-motor nerve is persistent, and consequently the malady is progressive."

The paper is illustrated by two plates; the first gives a coloured portrait of the girl whose case is recorded; the second shows the nervous connexions of the supra-renal capsules.

The next paper, by Mr Bryant, is entitled "Clinical Report on Inflammation and Tumours of the Breast," with special reference to their diagnosis. The paper is a valuable one, and is well worthy of the attention of the surgeon.

Mr Towne contributes a paper, in which he supplements former writings on the stereoscope and stereoscopic results. His theory of vision is that of Newton and Müller, assuming "that an object is seen single because its pictures fall on corresponding points of the two retinæ,—that is, on points which are similarly situated with reference to the two centres, both in distance and position, corresponding points of the two pictures falling on corresponding points of the two retinæ." Mr Towne describes some ingenious experiments devised with a view to afford additional evidence in proof of the existence of identical light-receiving points upon the two retinæ, and of the perfect identity of action and of sensation which exists between them.

Mr Cooper Forster records an interesting case of intestinal obstruction depending upon the passage of a small loop of the lower portion of the ileum through a constriction of the peritoneum, situated opposite the right obturator foramen, but within the abdomen. There were all the symptoms of strangulation of the intestine, though, on examination of the outlets, no hernia could be detected. Mr Forster accordingly expresses his regret that he did not make an exploratory incision, with a view to searching for the source of constriction.

Dr Hilton Fagge gives the particulars of the case of a woman admitted into Guy's Hospital, complaining of pain in the back of the right thigh, with hardness and swelling at the inner and lower part of the tuber ischii. Constitutional symptoms were severe, and the patient died about a month after admission. On post-mortem examination, a ruptured aneurism connected with an abnormal vessel was discovered. A large quantity of blood was found beneath the gluteus maximus; blood had extended through the sacro-sciatic notch into the pelvis, and also among the muscles of the thigh towards the front. The vessel with which the aneurism was connected arose from the internal iliac, ran parallel to the sciatic nerve, and was of about the size of the femoral artery; it terminated in the popliteal artery. The femoral artery itself was small, and terminated in a branch, apparently the superficial part of the anastomotica magna, which ran near the internal saphenous nerve. The case is one of great interest, and it is unfortunate that the examination, having been performed, not in the dissecting-room, but in the post-mortem theatre, was incomplete; in particular, the condition of the arteries in the opposite limb was overlooked.

Mr John Birkett is the author of two papers. In the first he describes a remarkable case of disease affecting the shaft of the tibia. The patient, a woman, 43 years of age, sustained a simple fracture of the left tibia about its middle, by slipping upon the street. The leg was put up in the usual manner, but after the expiration of about a month, when the usual amount of consolidation around the fragments had taken place, a persistent swelling attracted attention. It was situated over the front of the tibia, and

the patient now stated that it had existed for six months. The bone united, but the tumour remained. She was discharged, but was re-admitted nine months afterwards, on account of pain at the seat of fracture. The tumour was rather larger than before, and in its outline closely resembled an ordinary node. The pain did not yield to treatment, and at length became so agonizing that an exploratory incision was made into the tumour. It was then found that a soft vascular growth was situated within the shaft of the bone, and extended a considerable distance upwards and downwards. After this examination the sufferings of the patient became aggravated, and the growth increased; amputation of the limb was accordingly performed. The upper flap, however, sloughed, incontrollable vomiting set in, and the patient died on the fifth day after the amputation. Mr Birkett's own opinion is, that the growth was of a simple or fibro-plastic character. For our own part, after looking to the whole progress of the case, and to the character of the growth itself, we cannot avoid coming to the conclusion that the growth was of a malignant nature. This opinion is strengthened by the rapidity with which the tumour grew after it had been interfered with, and by the severity of the symptoms which followed the amputation.

Mr Birkett's second paper is an elaborate one, entitled, "Cases of Inguinal Hernia depending upon Abnormal Conditions of the Vaginal Process of the Peritoneum." The cases, thirty-five in number, are arranged in the following order :—

"I, A.—Those in which the vaginal process of the peritoneum remained open along its whole extent.

"I, B.—Those where a constriction of the vaginal process of the peritoneum constituted an impediment to the reduction of the hernia, and was the cause of its strangulation.

"II.—Cases of hernia associated with malposition of the testicle.

"III.—Those depending upon an open state of the funicular division of the vaginal process of the peritoneum.

"IV—The cases in which the canal of the vaginal process of the peritoneum remains unobliterated, whilst its ventral orifice is closed, and a hernia pushes its sac before it along the open tube."

Dr Alfred S. Taylor contributes three papers. The first is headed, "Cases and Observations in Medical Jurisprudence," and contains reports of cases of poisoning by various preparations of mercury, by aconite, by ammonia, by alcohol, by nitrobenzole, and by aniline. There are also some observations on the process for detecting chloroform in the blood, from which it appears that although chloroform, when added even in small quantity to blood, can be readily detected by the decomposition of its vapour by heat, the process cannot in actual practice be very much depended upon. "Some years since," says Dr Taylor, "in conjunction with the late Dr Snow, I examined by this process the blood of a boy who had died in Guy's Hospital from the effects of chloroform vapour, but

without detecting any trace of it. There was no odour in the blood, and the result was negative. In 1863, I examined on three occasions blood taken from patients while they were under the full operation of chloroform. It was brought from the operating-table of Guy's Hospital, where it had been collected in closely stopped glass bottles. One of the samples, examined within half an hour after removal from the living body, had no odour of chloroform, and gave not the slightest chemical indication of its presence. The two other samples kept in close bottles until tested forty-eight hours after removal, did not contain a trace of chloroform vapour. Either the quantity in a few ounces of blood is too small for detection, or it is rapidly lost by its volatility, or it is converted in the blood into formic acid or some other product, and is thus withdrawn from this method of analysis."

In his second paper Dr Taylor records a case of poisoning by the external application of arsenic. An ointment containing arsenic was applied to the head of a child with a view to destroying lice, and death took place *ten* days afterwards. The symptoms were comparatively mild, and there was no vomiting or gastric pain, although there was found to be inflammation of the stomach and bowels, pointing to the specific action of the poison on the mucous membrane of the alimentary canal. On chemical analysis, minute traces of arsenic were obtained by Reinsch's process, from the liver, stomach, and intestines.

Dr Taylor's third paper is on a case of death from the rupture of the uterus. The rupture took place after one prolonged pain, without any previous symptoms calculated to lead to a suspicion of what was about to happen.

Mr Edward Cock records some interesting cases of aneurism, and Dr Owen Rees contributes a short paper entitled, " Clinical Remarks on Calculous Disease." The principal object of Dr Rees' paper is to prove that cases of renal calculus are frequently met with without the occurrence of hæmorrhage in such form as to attract the attention of the patient, and that the physician may even fail to detect it microscopically. The following remarks on the character of the pain in this affection are worthy of attention :—

" As regards the seat of pain in cases of calculus, I have heard it laid down as a rule that, even if it happens to exist on both sides of the body, it is always felt more on one side than the other. Now, this, though a good *general* rule has very numerous exceptions, and I have frequently met with cases in which calculi have passed from the kidney after the occurrence of the full catalogue of symptoms usually described as significant of their presence, with the exception that no pain had been felt in the lumbar region. The whole discomfort in these cases has been referred to the sacrum, and so far as the patient could determine, not more on one side than on the other.

" There is, again, a peculiarity with regard to the seat of pain when calculus exists in the right kidney which is very apt to deceive, notwithstanding that it has been long ago described by the older writers.

" The pain in these cases is referred to the right hypochondrium. It extends downwards towards the umbilicus, but not to the lumbar region. There is a feeling of great distention over the colon, and the bowels are constipated.

"These are the symptoms so often regarded as significant of the passage of biliary calculus, an error easily committed if blood be not perceived in the urine."

Dr Braxton Hicks has a paper " On the Glandular Nature of Proliferous Disease of the Ovary, with Remarks on Proliferous Cysts." The paper is illustrated by some well-executed drawings.

Dr Pavy contributes a paper " On the so-called Amyloid Degeneration." Having given a short sketch of the history of his subject, Dr Pavy proceeds to inquire into what is the real nature of the degeneration in question. He does not agree with Virchow in considering it to be of an amyiaceous or amyloid character, but comes to the conclusion (which is supported by chemical analysis), " that we have in reality to deal with an albuminous or nitrogenized body." Dr Pavy, therefore, considers that the term " amyloid," as applied to this degeneration should be abandoned, and that either the term lardaceous or waxy should be employed. The following are Dr Pavy's remarks with regard to the effects of iodine on tissues affected with this form of degeneration,—

" From the observations I have conducted it has seemed to me that the coloration of the lardaceous matter effected by iodine has depended upon a simple absorption of the reagent. My impression is that the coloration is not the result of a definite, coloured product, like the product of union between iodine and starch or iodine and textrine, but is due to iodine as such, which is absorbed much more greedily, as it were, and held more firmly by the lardaceous deposit than by ordinary forms of animal matter. Unlike, in other words, the reaction of iodine with starch, textrine, or the amyloid substance of physiology, the colour appears to depend on the iodine alone, its intensity being regulated by the amount absorbed or deposited.

" Undoubtedly, according to my experience, the colour, in arriving at black, passes through shades of *red and brown and not of blue*. This I have observed not only during the heightening of the colour, but likewise during its fading under the influence of exposure to boiling in a test-tube."

We are ourselves quite satisfied of the soundness of Dr Pavy's remarks, and agree with him in thinking that the term " amyloid" should be abandoned as calculated to mislead.

The two remaining papers are by Mr Hinton, on disease of the ear after scarlet fever, and by Mr Durham on certain abnormal conditions of the bones. In conclusion, we can recommend this volume of " Reports," both to the physician and the surgeon, as containing a large amount of interesting matter.

—————

*Gunshot Wounds and other Injuries of Nerves.* By S. WEIR MITCHELL, M.D., GEORGE R. MOREHOUSE, M.D., and WILLIAM W. KEEN, M.D., Acting Assistant-Surgeons, U.S.A., in Charge of U.S.A. Wards for Diseases of the Nervous System. Philadelphia : Lippincott and Co. : 1864. Pp. 164.

MEN do not gather figs of thistles or grapes of thorns; yet one of the nearest approaches to such a reaping of what one has not sown, is to be found in the medical history of a great war.

While at first there seems enough to be done in dressing the wounds, and no time for registering the symptoms of the hecatombs of the hurt; as the war goes on, and specially as it nears its close, hospital arrangements are perfected, and numbers of surgeons are added to the staff; and this is the time when science reaps her harvest, not in any sudden advances in practice or startling novelties in theory, but in the careful registers of numberless clinical cases, and the comparison of hundreds of strange yet illustrative pathological facts.

The enormous magnitude of the war in America, which has now verged into a conquest, has made it one of the most magnificent fields that could well be conceived for the true exercise of our vocation—saving of life and limb.

An administrative arrangement, initiated by the then Surgeon-general, Dr William Hammond, has been the means of making certain hospitals (especially in Philadelphia and its neighbourhood) peculiarly well fitted for advancing the scientific study of surgical disease, as well as for the special treatment of the patients. We refer to the plan of grouping different sets of cases in different hospitals.

On a small scale this arrangement would have financial and administrative disadvantages, but on the enormous one on which things have had to be done in America this is not the case. The hospital for "stumps" and stumps alone, at Philadelphia, contained 600 beds and rarely fewer than 550 patients.

The book, the name of which heads this notice, is the fruit of one of these special wards, " for diseases and injuries of the nervous system," chiefly, of course, the result of gunshot wounds, and consists of a digest of the careful records of 120 cases of this nature, seen by the authors during a period of fifteen months.

It is only one of a series of papers (some of which are frequently referred to in the text) on allied subjects; such as, malingering, reflex paralysis, muscular hyperæsthesia, and spinal affections, all by the same authors.

Besides its value to practical surgeons, from the many details of treatment it gives, this work will be specially interesting to physiologists and neuro-pathologists, from the extreme care with which the cases appear to have been taken, and the exactness and minuteness of the descriptions of the effects of the injuries on motion and sensation.

A point or two of special interest may be noticed.

On the question of local shock, from the passage of a ball near a nerve, e.g., the great sciatic, we read,—

" The most difficult fact to explain in this connexion, is the great frequency with which a gunshot injury of a nerve causes total loss of motion and very little of sensation. It would be natural to suppose, that a ball striking a nerve, or passing near it, would equally damage its motor and sensory fibres. Practically, it is the motor filaments which suffer most severely, most often, and most extensively. Nor is this less true of the case in all stages, for we

find that the lesions of motion are always the least readily relieved and the last to improve."

Under the head of " Spinal Commotion," an interesting class of cases is given. In these, a ball passed over or alongside the spine, in some injuring only a spinous process,—in others with no injury to the bones at all,—yet in all paralysis, complete though not permanent, of one or more extremities, occurred. In one, though no bones were injured by a wound in the neck, paralysis of all four extremities occurred and lasted for months.

In the chapter on Injuries of Special Nerves, one case of wound of the sympathetic in the neck is of extreme physiological interest, from its effects on the pupil, and on the vaso-motor system of the affected side.

The remarks on the particular condition of the skin and appendages, as the results of nerve injuries, are very good. " Two distinct varieties are observable." The first is well known, the result of entire division of the nerves of a limb, seen in total palsies. The second, or " glossy skin," has been noticed already by Mr Paget in the Medical Times and Gazette for 1864 : his remarks are quoted here ; but the following more detailed account is added :—

" The skin affected in these cases was deep red and mottled, or red and pale in patches. The epithelium appeared to have been partially lost, so that the cutis was exposed in places. . . . In the fingers there were often cracks in the altered skin, and the integuments presented the appearance of being tightly drawn over the subjacent tissues. The surface of all the affected part was glossy and shining as though it had been skilfully varnished. Nothing more curious than these red and shining tissues can be conceived of. In most of them the part was devoid of wrinkles and perfectly free from hair."

The dryness of the skin after complete section of the nerve is found to contrast curiously with the occasional very excessive secretion of sweat of an acetous odour, found in limbs or portions of limbs of which the nervous supply has been partially injured.

Lesions of sensation are carefully described, special prominence in the description being given to a form of pain to which other authors give the name of " burning pain," excessive in its amount, peculiar in its character, and connected in many cases with the " glossy skin" form of lesion of nutrition.

With regard to treatment, chief prominence is given to a most unqualified approval and adoption of M. Duchenne's " Faradization" of the muscles involved, while the burning pain is found to yield to the frequent use of blisters.

On the whole, notwithstanding marks of haste, and a perhaps unnecessarily complicated method of arrangement, this little monograph is a valuable addition to medical science.

*A Manual of the Practice of Surgery.* By WILLIAM FAIRLIE
   CLARKE, M.A., F.R.C.S., etc. Henry Renshaw: 1865. 32mo,
   pp. 352.

To the canons of criticism left us by the father of the art, one has
been added only very lately, which bears in a very special manner
the impress of this practical and mechanical age. One of the first
instances we remember of the employment of this, which we may
call the avoirdupois or beam-and-scale criticism, was the review of
a large system of surgery in two volumes, which, with what (car-
rying out the figure) we may call a ponderous levity, was
described as being heavy in both senses—heavy to hold and heavy
to read.

The Manual of Surgery which Mr Renshaw has lately added to
his series, might, with advantage, claim to be tried by the same
test, for its small size is apparently considered its chief recommend-
ation.

We all remember the poor girl, who, applying for the situation
of wet-nurse, excused her antenuptial maternity by the plaintive
plea that the baby was such a little one ; but this smallness, which
is considered an excuse for a peccadillo, or a beauty in a fancy
terrier, is, we think, not to be desired in a manual of surgery.

There must, we own, be a demand for such works, or there would
not be such shoals of " Hints," " Vade-mecums," " Companions,"
and the like, published every day ; and Mr Renshaw's little
manuals are becoming very numerous. We have known dispensary
pupils who would be quite helpless on their rounds without their
" Tanner." Anatomical facts can be selected and compressed.
Ward's Osteology is a very full and accurate description ; but
surgery cannot be compressed in a like fashion ; and we feel that
Mr Clarke has done himself and his subject injustice in making the
attempt. It can be conceived possible that a very clever and
original thinker might, in his old age, condense the principles of
surgery into small space, in a series of axioms or aphorisms, which
might relieve the memory by their brevity, and stimulate thought
by their pregnancy of meaning. The Elzevir edition of the
Aphorisms contains only 230 pages, and is fit for the waistcoat
pocket ; but it is a very different thing to attempt to condense the
description of surgical disease and the method of performance of
surgical operations. Such brevity is too apt to be inaccurate, such
condensation to be unintelligible ; and for what class of readers
are such manuals required ? No diligent student would be satisfied
with such a class-book,—no idle one would venture to trust to it
even in his cramming for a pass. A book-tray full of such booklets
would hardly take the place of the practitioner's shelves, and we
can scarcely fancy an operating surgeon deriving any advantage

from the description of lithotomy or amputation at the hip-joint contained in such a portable vade-mecum.

We are finding fault with the work he has had to do, but not with the manner in which Mr Clarke has done it. It is a dry thankless task to condense the opinions of others; to do it at all implies a good deal of self-denial, and requires a good deal of fairness; but, on the whole, Mr Clarke has done it well. The little pathology there is, is innocent, if neither very recent nor very transcendental. The weakest part of the whole is the section containing the descriptions of operations; for while many important amputations are slurred over and are unintelligible, if not inaccurate, the utterly unimportant ligatures of radial, ulnar, and dorsalis pedis arteries occupy fully a page.

When Mr Clarke again appears as an author, we heartily wish him either a less extensive subject, or more time and space to devote to it.

---

*Transactions of the Pathological Society of London. Volume Fifteenth: Comprising the Report of the Proceedings for the Session* 1863–64. London: 1864.

*A General Index to the first Fifteen Volumes of the Transactions of the Pathological Society of London; with a List of Authors, and a classified List of Subjects.* Compiled by T. HOLMES, M.A. Cantab., Hon. Secretary to the Society. Presented to the Society by PRESCOTT HEWETT, President. London: 1864.

EVERY additional year during which the Pathological Society carries on its operations furnishes a valuable contribution to pathological literature. The unrivalled opportunities for the cultivation of morbid anatomy afforded by the metropolitan hospitals, and the energetic and harmonious manner in which the labours of the members have been carried on, enable the Pathological Society yearly to publish a volume containing descriptions of rare forms of lesions, and a mass of materials invaluable to the statistical inquirer. Of the present volume of transactions we can only say that it is in no respect inferior to its predecessors. The number of specimens described is about 170, and many of these become of additional interest from the general observations appended by their exhibitors. The only quotation we can make consists of some remarks by Dr Samuel Wilks in reference to a case of Addison's disease. We select this subject because we believe there is still some misapprehension in regard to the cases where a morbid condition of the supra-renal capsules leads to morbus Addisonii. Every disease of the capsules, or every deposit in

their substance, does not produce this train of symptoms, and we are indebted to Dr Wilks for showing that a special lesion of the gland is present in all cases of the disease.

"It would appear, as far as observations have hitherto gone, that, in Addison's disease, the change in the capsules has always been of the kind described in the present case. This uniformity is quite in accordance with ordinary pathological observations, for such an affection is closely allied to the primary or idiopathic diseases which take place in other organs, whereas diseases which affect a large part of the body, such as cancer or tubercle, are very rarely seen to attack one organ alone to its complete destruction, especially if that organ should be double, as the kidney or the supra-renal body. Indeed, in studying the diseases of the liver, kidney, or other organs, we should not select cases where cancer, or such like affection, had attacked these viscera, but should rather select those instances where they are affected independently and idiopathically, as in Bright's disease, or cirrhosis; and it is to these chronic affections of a more or less inflammatory kind of the lungs, liver, or kidneys, that one must compare Addison's disease of the supra-renal capsules. Just as a cancer of the lung would fail to produce the symptoms known as those belonging to ordinary pulmonary diseases, so a cancerous deposit in a supra-renal capsule would be unaccompanied by the symptoms which Addison described. Thus all analogy, as well as observation, would point to the condition described as that which constitutes the only true form of the disease. Thus it is that the observations of Professor Mattei, of Siena, have produced only a negative result, for although he has often found adventitious deposits in the supra-renal bodies, he has not met with a true example of Addison's disease, and, consequently, in none of his cases were there any of the symptoms which the discoverer described.

"It may also be observed that, apart from the discoloration, the remarkable want of power, or asthenia, constitutes the most striking feature of the disease. It is not a state of anæmia, for, as in the present case, the blood was in good condition and the lips red. The pulse is not full and soft as in anæmia, but very small and thready as in nervous depression, witnessed especially in those cases where the abdominal sympathetic is involved."

A most valuable addition to the Transactions of the Society has been made by the publication of an Index to the first fifteen volumes. No doubt each volume has been provided with an index, but the value of a general index will be appreciated by all who have occasion to consult these Transactions. For this contribution the Society is indebted to its Secretary, Mr Holmes, and its President, Mr Prescott Hewett. The former gentleman ably performed the arduous task of preparing the index; while the latter defrayed the expenses of publication, and presented it to the Society.

# Part Third.

## PERISCOPE.

### PRACTICE OF MEDICINE.

CASE OF DISEASE OF POSTERIOR COLUMNS OF THE CORD—LOCOMOTOR ATAXY (?).
BY J. LOCKHART CLARKE, AND DR J. HUGHLINGS JACKSON.

*History of the Case, by Dr Jackson.*

LOUIS A——, at the date of the following notes, was fifty-seven years of age. The date of the notes is about three months before his death, which occurred on 20th December 1864.

The man has been for some time a patient at the hospital for epilepsy and paralysis; but has been rarely seen, as he gets from home only after much trouble and at great expense. He was quite blind, and was unable to walk. His sight failed fourteen years ago. His blindness began by a gradual dimness. The patient is confident that the right eye began to fail before the left, as in his business he was obliged to use the right one alone. The left eye, however, soon became affected; and in two years he was blind of both. I find the optic discs quite white, but well margined; the vessels are of good size.

Further symptoms showed themselves twelve months after the blindness was complete. He then began to suffer severe pains in his legs, which at first took him suddenly when he was walking. Without prompting him, he said they were like "pinching the flesh;" and he once remarked, "they are like a dog biting me suddenly." It was not, however, until five years from the onset of the pains—that is, six years ago—that he found difficulty in walking. Then he found that after walking a little way he had to sit down; but he could sit easily, and could use his arms. In two years more—that is, four years ago—he began to experience difficulty in using his arms. Most unfortunately no continuous record has been possible. He gradually became worse in walking, and at length could not walk at all unless supported. Generally he was wheeled into the out-patients' room, but could walk when supported a little. The support was not enough to give him any real help in the way of power, but was enough to guide him. This fact conclusively shows that the difficulty in walking arose from defective co-ordination, and not from deficient muscular power—the paralysis being apparent merely. He remarked that when he attempted to walk he was lost. He could, however, get in and out of bed. Although he said his feet were numb, there was no apparent loss of sensation; but the compasses were not used. His arms had been so much affected that he could not feed himself; but they had improved, and now he could do so, and had even shaved himself. Still they were, he said, "numb;" and he complained bitterly (indeed, at his last visit, this was his chief and urgent complaint) of burning pain at his fingers' ends. They were sore near the nails from rubbing.

He died on 20th December 1864. He had been under the care of Mr Harston of Islington, and this gentleman kindly allowed me to make a post-mortem examination. The brain was everywhere pale, and the central parts were soft. The right optic thalamus was one-third less across than the left. The fornix and valve of Vieussens were soft also; and so, too, to a slight extent, were the lobes of the cerebellum. In this softening, however, there was nothing more than what one frequently finds in cases in which there have been no marked nervous symptoms during life. The cord looked healthy,

except for about two or three inches in the neighbourhood of the middle dorsal region. Its anterior surface appeared normal, but the body of one of the vertebræ was somewhat diseased. The dura mater was entire, but in the region just mentioned was thickened, and of a dirty-yellow colour, and was adherent to the posterior surface of the cord.

It must be kept in mind that in his case there had been no opportunity of following up the order of symptoms so carefully as is desirable in an obscure disease like locomotor ataxy. And it is especially to be observed that there is no note of the patient's condition for three months before his death. Moreover, from the disease beginning by amaurosis, the later phenomena would have been less striking to the patient, and this will account for a want of note as to any peculiarities in his gait when the difficulty of walking began.

This is the only case I have known in which amaurosis preceded the difficulty in movement. Amaurosis is by no means a common symptom in locomotor ataxy, but when it does occur it is in my experience (which is limited to four or five cases) unlike (at least unlike in the ophthalmoscopical appearances) amaurosis from disease of parts in the head. In all, the appearances of the optic nerves have been like those in this case, except that in one of them there was in the centre of the discs, obscuring the entry of the vessels, a white irregularly-margined cloud ; whereas the optic disc in amaurosis from intracranial disease always shows evidences of recent or past neuritis.

This distinction is, I think, of direct practical importance in two ways,—1st, that it will, if fully established, be another help in the diagnosis of difficult cases ; and, 2dly, that I think it will be found that this particular form of amaurosis, when it occurs alone, or, as some would say, when " uncomplicated," is (although, perhaps, at first due to purely local disease) not very unfrequently, as in this case, the first symptom of *progressive* disease of the nervous system. And I would particularly urge that, in cases of amaurosis of this kind at ophthalmic hospitals, inquiries should be made as to pains in the legs (carefully describing them), and as to numbness of the fingers.

*Microscopic Examination of the Corpora Quadrigemina, Spinal Cord, etc., by*
*Mr Lockhart Clarke.*

In neither the corpora quadrigemina nor the corpora geniculata was there any abnormal appearance. Of the condition of the optic nerves and tracts I am unable to give any opinion, as they were not amongst the parts which I received. The pons Varolli was apparently healthy ; but nearer the surface of the fourth ventricle, between the roots of the sixth and seventh nerves, the medulla was somewhat congested. The whole of the medulla oblongata was perfectly healthy ; nor in the spinal cord was there any evidence of disease above the second cervical nerves. At the level of these nerves, however, certain morbid and very peculiar changes began to make their appearance. The posterior white columns became exceedingly congested, and their superficial layers, to a depth varying from one-twelfth to one-twentieth of an inch, were transparent and pale, or grey, resembling exactly in colour the gelatinous substance around the extremity of the posterior cornu, and the connective tissue in the fissures, or in the spaces between the nerve-fibres. Now, the paleness and transparency of these layers, which, in a transverse section of the cord, contrasted so strikingly with the dark colour and opacity of healthy columns, were due to an entire absence of their nerve-fibres, which had undergone complete atrophy and disintegration, some granular débris alone remaining with the connective tissue of the part. The deep *lateral* layers between the posterior cornua were dark and healthy : but for a little distance on each side of the posterior median fissure the columns were paler, although not nearly so pale as at their surface ; for many of their fibres had wholly or partially escaped destruction, and lay at wide intervals in a bed of transparent tissue.

But these were not the only columns that suffered in this way ; for the posterior halves of the *lateral* columns, at their surfaces, were evidently damaged by the same kind of morbid change.

Below the third cervical nerves, and throughout the whole of the cervical enlargement, the cord was much flattened from before backwards. The posterior white columns were crowded with enlarged and congested bloodvessels; but in many sections only their superficial nerve-fibres had suffered atrophy and disintegration. A similar change was observed along the surfaces of the posterior halves of the lateral columns, and in some spots along the borders even of the anterior columns. In other sections, especially from the *lower* part of the cervical enlargement, the *deep* layers of the posterior columns, between the cornua, were more or less involved in the process of destruction; and it was very evident, in many places, that some fibres of the posterior roots, as they traversed these columns, were damaged in the same way. The grey substance, moreover, was not altogether healthy; for the posterior cornua were in many sections congested, and in some contained small areas of disintegration.

It was in the dorsal and lumbar regions, however, and especially in the former, that these morbid changes were most conspicuous. In many parts of the back, the dura mater was enormously thickened, and so adherent to the subjacent membranes—which were themselves unusually thick and adherent to the cord,—that it was impossible to remove it without tearing away some portions of the posterior columns. Sometimes it was glued down by what appeared to the naked eye as a thick, yellow layer of exudation, but which, under the microscope, presented the appearance of fibrous and elastic tissue containing some granular fluid. Sometimes the membranes were pinched up, as it were, into a longitudinal ridge or spine along the middle line of the posterior surface of the cord, like the spine of the vertebral column; and at one point they formed the sac of a cyst, about the size of a pea, which contained some granular fluid.

In most parts of the dorsal region many of the fibres of the posterior columns, particularly towards the surface, had wholly disappeared, and many that remained were damaged or broken up by advancing disintegration; but along the middle third of this region their number was so small that nearly every portion of these columns was granular and transparent. At the deeper parts of the same columns, between the basis of the horns, they were much more numerous on each side, but in the middle line they were nearly absent. It is also remarkable that the posterior median fissure was entirely obliterated. On the left side the posterior nerve-roots had shared the fate of the column through which they passed, for not a fibre, nor the fragment of a fibre remained; and even on the opposite side a few only, variously damaged, could be discovered. External to the cord, also, the same roots were much below their normal size.

The posterior commissure of the cerebral parts of the grey substance around the canal were somewhat injured by disintegration and were almost fused with the deep layer of the posterior columns.

The posterior surfaces of the lateral columns to about one-third of their depth had lost many of their fibres, and presented a somewhat transparent aspect, which contrasted rather strongly with the opacity of their deeper layers.

In other parts of the dorsal region the appearances were rather different, and the grey substance was more extensively damaged. On the left side the posterior horn was reduced to a long narrow strip. On the right side the caput cornu, or expanded extremity of the horn was preserved entire; but its cervix or neck was blended or fused with a long narrow tract of the posterior column in a state of transparent disintegration, and two other parts of the posterior columns between the cornua were affected in the same way. The lateral grey substance was almost wholly destroyed by one of these tracts of disintegration, and that on the opposite side as well as the intervening central gray substance were exceedingly damaged by lesions of the same kind. The superficial parts of the posterior columns had lost an immense number of their fibres, and were in consequence very transparent.

In the lower part of the dorsal region the grey substance, on the left side, was severed into parts, and strangely abnormal in shape. These changes were

not the result of accident or manipulation, for the cord here was perfectly cylindrical, and the section represented was sound or unbroken. The posterior columns had lost a large proportion of their fibres, and were consequently more or less transparent. In many parts of this region a line of complete disintegration extended along the surfaces even of the *anterior* columns.

In the lumbar enlargement the deep portions of the posterior columns between the cornua retained their opaque and natural appearance; and at this part, also, the posterior median fissure was still in existence; but on approaching the surface, the columns more and more were stripped of their nerve-fibres, and the fissure in the same proportion was closed and obliterated. The *surfaces* of the *lateral* columns were entirely destitute of fibres; and the *anterior* columns were not only affected at intervals in a similar way, but were somewhat damaged at their deeper layers by partial disintegration. The grey substance was extensively injured. Two large areas of finely-granular material, surrounding tortuous and congested bloodvessels, replaced a part of the anterior commissure; and two others, still larger, were found on the right side, at the junction of the anterior and posterior cornua. The *nerve-cells* of the anterior cornua were darker and browner than usual, in consequence of the greater number of pigment granules which they contained.

To whatever degree of degeneration the white columns were reduced, their natural form, and generally their normal size, were preserved by a corresponding hypertrophy of connective tissue abounding with its characteristic nuclei. In this tissue at wide but variable intervals, lay embedded the remaining nerve-fibres, with the debris of their neighbours in different states of disintegration. In some places they were severed into shorter portions, or into rows of globular masses, sometimes concentrically marked, and formed out of the white substance stripped from the axis-cylinders. In other places they had fallen into smaller fragments and granules, which either lay aggregated in the line of the fibre, or were scattered at irregular distances. Oil-globules and particles of different sizes were thickly interspersed between them, but were especially collected into groups of variable shape and size around the bloodvessels of the part. The larger vessels were, indeed, enveloped in layers of remarkable depth. At the *lower* part of the vessel only the larger and more widely separated oil-globules are shown; but at its upper part, the groups of smaller particles surrounding them are also represented.—*Lancet.*

### ENDOCARDITIS.   BY DR CORRIGAN.

DR CORRIGAN exhibited a specimen of endocarditis, complicated with ascites, which occurred in a girl, aged nineteen, who had been under treatment in Whitworth Hospital. The first point of interest in the case was, that the two diseases—ascites and endocarditis—did not, except in a very trifling degree, stand to each other in the relation of cause and effect; for, though there was a great amount of ascites, there was no turgescence either of the face or of the large veins of the surface, nor was there any evidence of mechanical obstruction; so that they were obliged to look at the ascites, although no doubt to a certain extent connected with the endocarditis, still as a distinct disease. With respect to the endocarditis there were some points of interest. When the girl was admitted her symptoms were the following:—Intense orthopnœa; blue lips; no turgescence of face, but excessive weakness of the circulation.

Endocarditis is a disease which the physician is obliged to diagnose as much by negative as by positive signs. Thus in the present case the first symptom was orthopnea, indicative of disease in either the respiratory or circulating organs. The next symptom was the weakened action of the heart; this is dependent on the same law which prevents the intercostal muscles from acting in pleuritis, viz., the instinctive refusal of the muscles to act over an inflamed surface. In the same way the muscles of the heart will not act where there is inflammation of the pericardium. From the operation of this law when endocarditis comes on suddenly, the patient sometimes dies as suddenly, from the refusal of the heart to act over the inflamed membrane. The two

symptoms which he had mentioned—orthopnœa and the weakened action of the heart—indicated that the disease was either pericarditis or endocarditis. The first hypothesis as to effusion of lymph was, however, negatived by the fact that there was no friction sound at any time; and when there is effusion distending the pericardium, the fluid always extends upwards as its principal direction, and we have dulness as high as the first or second rib. In the present case that sign was wanting; so that they had thus arrived by negative signs at the determination of the disease as endocarditis. The positive signs were, weakness of the heart's action and the bruit accompanying the action of the heart. The girl was relieved from the ascites by tapping; and her condition was so far improved that she left the hospital in a few weeks; but being unfortunately in poor circumstances, and unable to command the comforts necessary for one in her condition, she returned again and again with relapses, and finally came into the hospital, about six weeks or two months ago, sank gradually, and died a few days ago. The post mortem examination was, in a pathological point of view, very interesting, as regards the endocarditis, supporting the view which he (Dr. Corrigan) had put forward on former occasions, viz., that in cases of valvular disease the origin of the malady is the deposition of lymph, the result of inflammation. First, there is a deposit of lymph; this continues until cirrhosis is set up, and goes on daily, till at length the valves are contracted and incapable of performing their functions. In the present case, the progress of the disease could be traced from the post-mortem appearances, the valves of the pulmonary artery being healthy, transparent, and thin; but of the aortic valves one was an eighth of an inch thick, and there was a deposit between the membranes, which appeared to be lymph. This was a distinct proof that the commencement of the disease in this girl was the deposition of lymph between the folds of the valves; it was not absorbed, and it then took on the action of cirrhosis, which contracted the valve along its base, dragging down its upper or floating margin; and eventually there would have been great permanent patency of the aorta. With regard to the treatment of such cases, the principal points deducible from the views he had taken, if correct, was, that for a very long time the disease must be treated just as a case of deposition of lymph elsewhere would be treated— by antiphlogistic remedies, counter-irritants, and remedies of that kind which prevent the effusion and promote the absorption of lymph.—*Proceedings of the Pathological Society of Dublin, from the Dublin Quarterly Journal of Medical Science.*

---

## MIDWIFERY.

### ON THE ACTION OF ERGOT OF RYE UPON THE FŒTUS.    BY DR M'CLINTOCK.

THAT the administration of ergot of rye, in the second stage of labour, is very apt to be followed by injurious or fatal consequences to the fœtus, if its birth be delayed much beyond an hour and a half, is a proposition that will scarcely be called in question here; and I believe there is hardly any accoucheur of eminence or experience at the present day who is not of the same opinion with regard to this medicine. Indeed so notorious is this effect of ergot, when good, and given in full doses, that it has been proposed to call it the *pulvis ad mortem*, as a fitting substitute for its name of *pulvis ad partum.*[1]

[1] The evidence which might be accumulated on this point is overwhelming. On the other hand, Dr Denham, in a paper published in a back number of this [Dublin Quarterly] Journal, has expressed doubts as to the injurious effects of ergot on the fœtus; but a rigid examination of the cases therein detailed will show that they are not directly at variance with the principles above laid down. In like manner, a close analysis of the 173 cases reported by Dr R. U. West to the London Obstetrical Society, clearly demonstrates that very few of them can be taken as data for deciding the particular question before us; whilst the number which can strictly be said to bear evidence on the opposite side is extremely small—not more than about 12 cases. For although there are 28 of his cases in which the interval between giving ergot and delivery was two hours or upwards, still in 16 of them either the ergot produced no marked increase of pains, or only one dose was administered, or the liquor amnii was not evacuated when it was given; and, under any of these circumstances, hurtful effects to the child are not to be expected.

Soon after the introduction of ergot into obstetric practice its *modus operandi* in thus affecting the child became a subject of close inquiry. Its injurious action upon the fœtus was manifestly a great bar to its employment, a great check upon its usefulness, and hence accoucheurs were most desirous to discover how this baneful effect was brought about, in the hope of being able to obviate or correct it. Two opinions have been advanced to account for the destructive action of the medicine on the child. According to one of these the ergot acts in a physiological way, that is to say, it enters the circulation of the mother, and is conveyed, through the medium of the blood, to the fœtus. Now this is one explanation; and amongst its supporters we find the names of two of the highest authorities on the subject of ergot of rye—viz., Dr Beatty and Dr Hardy; and it is not, I assure you, without considerable apprehension and reluctance that I would venture to dissent from any opinion of theirs upon an obstetric subject, much less upon one which they have so closely and so successfully investigated. But "Amicus Plato, amicus Socrates, magis amica veritas." On the supposition that the poisonous property resided in some particular element or component part of the ergot, various attempts have been made to isolate the different constituents of the medicine, with the hope of obtaining the true parturifacient principle distinct and separate from the poisonous one. But all such attempts have been unsuccessful, and for a very sufficient reason I think. Many years ago I assisted in making a series of experiments with different preparations of ergot, but the results did not at all answer our expectations.

The other opinion, as to the *modus operandi* of ergot upon the fœtus, is to this effect—that the child is destroyed in consequence solely of the violent or continuous labour pains which the ergot excites.

Up to the present moment it is an unsettled question which of these two explanations is the correct one. Authors and practitioners are still divided upon it, though I am bound to say that the greater portion of the profession seem inclined to adopt the second theory as the correct one.

It is not my intention to weary you with any examination of the facts and arguments which have been brought forward in support of each of these theories by their respective advocates—I merely wish to lay before you, in a very general way, the results of my own experience and reflection on the point, and to state my reasons for believing that the second, or " mechanical theory," as it has been designated, embodies the true explanation of the mode by which ergot acts on the fœtus. This is not by any means an unimportant or speculative inquiry; on the contrary, it is one of the deepest interest to the obstetrician, and, like all questions relating to the action of remedies, has an intimate bearing upon practice.

At the outset of my professional life I adopted the opinion that ergot exerts a direct and specific influence on the fœtus. More enlarged observation led me to doubt, and subsequently to relinquish this idea—as, throughout a pretty wide field of experience, I did not meet with any evidence that could, in a direct, positive manner, lend it support. I have never met with a case where the child manifested, after its birth, symptoms of what might be called " ergotic poisoning;" and the cases I have seen where depression of the heart, or even the death of the child took place after ergot, and in the absence of its usual

---

In 95 (more than the half) of his cases the child was born *within one hour* from the giving of ergot; in 44 instances it was born within thirty-six minutes. In the great majority of instances the os was not fully dilated at the time of exhibiting the ergot, and in 38 instances it was only the size of a shilling or half-a-crown; and in 17 cases the membranes were still entire when the ergot was administered. In none of the cases was the fœtal heart examined. From this brief summary it will be apparent that in deciding the question—how far ergot can affect the fœtus?—Dr West's long list of cases, though valuable and interesting in many respects, carries very little weight. In one way they afford corroboration of the views expressed in my present communication, by showing that, where ergot is not given in a full dose, nor in the second stage of labour after the discharge of the waters, nor with the effect of exciting persistent uterine contraction—then, under any of these circumstances, there is little or no risk of injury to the child.

effects on the uterine contractions, were so very rare as to be quite exceptional, and could be equally well explained by the weakened state of the child (from the previous length of the labour at the time of giving the ergot—under which circumstances any further delay was of course fraught with imminent hazard to its existence. I fully admit that if the cases of this kind amounted to any considerable number, they might then justify an inference, which cannot, with any show of reason, be deduced from a very few solitary instances.

On the other hand, the common voice of experience proclaims that the danger to the child is in proportion to the intensity and uninterruptedness of the pains; and that where these are only imperfectly developed by the ergot, or are distinctly intermittent, hours may elapse with impunity to the fœtus.

We may look upon it, then, as sufficiently established that the influence of ergot on the fœtus, after the discharge of the waters, and dilatation of the mouth of the womb, bears a due proportion to the intensity of the uterine contractions. But something further, some evidence of a synthetic kind, is wanting to prove that the pains and the state of the fœtus stand in the relation of cause and effect. It may therefore be asked,—Do we ever see effects like to those of ergot produced in the fœtus by uterine contractions of spontaneous origin? To this very proper and pointed question my own experience enables me to give an affirmative reply.

It has been doubted by some writers whether powerful and continuous contractions of the uterus, such at least as ergot is capable of exciting, ever occur naturally. On several occasions, however, it has fallen to my lot to witness them, and no doubt many around me have done the same. This kind of uterine action has been, not unaptly, termed "tetanic," from the violence and persistence of the contractions. Here, then, is a case in which the pains possess the peculiar characters belonging to those which ergot excites; and in these cases of tetanic uterine action experience shows that, quite irrespectively of the length of the labour, the infant is very apt to be dead-born, unless art interfere to extract it, or to moderate the violence of the uterine contractions. For another reason, namely, the imminent risk of rupture of the uterus, we are generally obliged to give prompt assistance of some kind to the patient; but in any case the danger to the child is very great under these circumstances, and when still-born it has presented that livid congested appearance so often observed where ergot of rye has been given. Between the two classes of cases, those of ergotic and those of tetanic uterine action, there is the closest resemblance, *quoad* their destructive effects on the child. But this is not all. We may carry the comparison further, and we shall find that the effects on the fœtal pulse attributed to ergot are likewise produced in the case of tetanic uterine action. I have been careful to investigate this point; and in all the cases where I had an opportunity of examining, the fœtal heart was found notably diminished in strength and frequency, and at times irregular. These phenomena are identical, you observe, with those which ergotic contractions ordinarily produce. In both classes of cases the depression and irregularity of the heart's sounds were most marked immediately on the cessation of a pain. Indeed I have found it to hold good as a general rule, that the immediate effect of strong uterine contraction, after the escape of the waters, is to depress the pulse of the fœtus,—that is, to render the heart's sounds slower and feebler; and this fact in itself supplies us, I think, with a key to explain the mode of action of ergot upon the child.

Permit me now to recapitulate:—1. We have seen that, as a general rule, the danger to the child, after giving ergot, is exactly in proportion to the energy of the uterine contractions. 2. It is no less true that where but little uterine action is excited by the ergot, it does not seem to exercise a noxious influence on the fœtus. 3. The evidence that ergot is acting injuriously on the child is derived from the cardiac sounds, which become reduced in strength and frequency. 4. The immediate effect of uterine contraction upon the fœtal pulse is to produce a temporary diminution of its force and rapidity. 5. And, lastly, in the few cases I have seen, where violent and continuous uterine

action took place from natural causes, the fœtal pulse underwent the same changes as after ergot, and the children were born in a highly congested state, dead, or partially asphyxiated, unless delivered soon after the accession of these tetanic contractions of the uterus.

Of course it remains for future observers to corroborate or invalidate the facts I have adduced. But, assuming that my observations are correct and well grounded, I think it will be hard to avoid the conclusion that the action of ergot of rye on the fœtus is due solely to the uterine contractions excited by this medicine.

Let me here digress for a moment. I have had occasion to speak of certain changes in the characters of the fœtal heart's sounds, and to remark that in one class of cases (those of ergotic or tetanic uterine action) slowness, weakness, and irregularity are the precursors of the child's death; whilst in other cases, on the contrary (chiefly those of difficult labour), extreme frequency and weakness of the cardiac sounds are found to precede the fatal event. No doubt, the mode or cause of death might account for these differences. The subject, however, is one every way worthy of being closely investigated, and I recommend it to some of my junior brethren around me who have opportunities for prosecuting such an inquiry.

A very interesting question, originating out of what has been already stated, now presents itself to us, and it is this: How do the uterine contractions operate upon the fœtus?

There are three ways, I believe, in which the vital condition of the fœtus may be affected by the pains :—1st, The umbilical cord may be subjected to direct pressure from the contracting uterus, and if this pressure be so great as to completely stop the circulation through the funis, the death of the fœtus certainly and very speedily ensues. 2d, The compression which the brain and medulla oblongata undergo, where the head is wedged or impacted in the pelvis, has been deemed sufficient to endanger or to destroy the life of the child. It is very remarkable how great a change of configuration the head may bear consistently with the preservation of the fœtus. But here the change is slowly and gradually effected, whereas in the cases under consideration,—namely, those of strong and continuous uterine action, the change must, if it take place at all, be effected quickly and under "high pressure." It is only when there is resistance from the hard structures that pressure on the head can be really dangerous to the child, and therefore I believe that pressure on the head is very rarely a cause of fœtal death. If what I have just stated be true, it furnishes us with an instance of the occasional antagonism of maternal and fœtal interests. It is desirable for the mother that the pressure of the head on the soft linings of the pelvis should be as transient as possible, but for the child it is desirable that the compression of the head by the pelvis should be slowly and gradually made. *Lastly,* Persistent uterine contraction may affect the child by causing imperfect oxidation of its blood in the embryonic villi of the placenta.

There are the strongest anatomical and physiological reasons for supposing that the current of blood through the maternal cells of the placenta is checked during uterine contraction, so that the change in the fœtal vessels from venous to arterial blood is then arrested or imperfectly performed, just as the aëration of blood in the pulmonary vessels of a breathing animal, or in the branchial vessels of a fish, is stopped when the needful supply of air or of water is withheld.

We can, then, have no difficulty in understanding why pains of a violent kind, recurring at very short intervals, or succeeding one another without intermission, must operate prejudicially on the fœtus; nor why the child, when born under these circumstances, so commonly presents a congested livid appearance, and is so apt to be expelled in an asphyxiated condition.—*Proceedings of the Dublin Obstetrical Society, from the Dublin Quarterly Journal of Medical Science.*

# Part Fourth.

# MEDICAL NEWS.

## MEDICO-CHIRURGICAL SOCIETY OF EDINBURGH.

### SESSION XLIV.—MEETING VII.

*3d May* 1865.—Dr Douglas Maclagan, *President* of the Society, in the Chair.

#### I. SANGUINEOUS APOPLEXY IN A CHILD.

*Dr Watson* showed two sections of the upper portion of the right hemisphere of the brain, together with the cerebellum, pons, and medulla oblongata, from a girl of nine years of age, who died from sanguineous apoplexy, ushered in with epileptiform symptoms. The first attack had occurred several months before, from which, however, she rallied, with permanent, though partial, paralysis of the right arm and leg. The sections of the right hemisphere showed the upper part of a large cavity containing the remains of an altered fawn-coloured coagulum. This was the only lesion, as far as a careful examination could show, which corresponded with the occurrence of the original apoplectic attack and the paralysis. All else was quite recent, consisting of a clot which occupied the lateral ventricles, and extending through the third and fourth ventricles, terminated by enclosing the surface of the medulla oblongata, and the under surface of the cerebellum. Dr Watson indicated that the chief points of interest in this case were, 1*st*, The occurrence of sanguineous apoplexy in a child of tender years, who displayed no marks of any hereditary syphilitic affection; 2*d*, The only lesion a careful search could discover corresponding in its changes with the date of the first apoplectic attack was upon the same side as the hemiplegia; and, 3*d*, The only vascular lesion which could be detected was a small speck of calcareous degeneration of the right middle cerebral artery where it gave off the posterior communicating artery.

#### II. ARTERIES OCCLUDED BY ACUPRESSURE.

*Dr Watson* showed five preparations of arteries occluded by *acupressure*, in which there was no destruction of the coats of the vessels in the way of either sloughing or ulceration. In every one at the point occluded a small conical plug of rosy lymph was firmly adherent to the internal coat of the artery. In none of the cases did secondary hemorrhage occur.

#### III. ULCERATION OF LARYNX, TRACHEOTOMY, HÆMOPTYSIS.

*Dr Watson* showed the larynx and trachea of a patient on whom he had performed tracheotomy on account of ulceration of the glottis, with destruction of the left aretænoid cartilage and part of the epiglottis. The disease was due to syphilis. No ulceration had been going on for some time, but with every change of weather, especially from dry to moist, symptoms of œdema glottidis set in. It was on this account tracheotomy was practised. There was no bleeding to speak of during the operation; but after the patient had been in bed for about half an hour, violent hæmoptysis set in, which, although not directly fatal, induced such extreme feebleness that the patient sunk in the course of a few days. After death, although the wound, larynx, trachea, bronchi, and lungs were carefully examined, the source of the bleeding could not be detected.

#### IV. FRACTURE OF SYMPHYSIS AND ANGLE OF LOWER JAW.

*Dr Watson* showed a specimen of fracture of the lower maxilla, exactly at

the symphysis, a fracture at the angle upon the left side co-existed. Dr Watson stated that a fracture in the former situation was a rare accident, and so far as his observation went was always complicated with a second fracture about or near one or both of the angles of the injured bone. He believed that, in point of time, the fracture at the angle occasioned by a fall or blow upon the chin occurred first, and that the force still continuing to act, after the occurrence of this fracture, the anterior portion at the seat of injury was forcibly driven backwards, and gliding over the angle of the bone, which was retained in its position by the masseter and pterygoids, the angle of the jaw was separated from its fellow, and the arch consequently gave way at its centre. In this respect it resembled a bridge. So long as the lateral supports remained intact the keystone formed its strongest point, and solution of continuity there could hardly, by any possibility, occur from a force acting from above; but as soon as the side support was broken, or, if we could suppose such a thing, yielded outwards, then a second solution of continuity was more likely to take place in the situation of the keystone than anywhere else.

### V. AMPUTATION AT ANKLE WITH AN ANTERIOR FLAP.

*Dr Watson* showed a specimen of comminuted fracture of the tuberosities of the os calcis, produced by direct violence. It was attended with extensive laceration of the soft parts, and required amputation to be performed at the ankle-joint. This, as the soft parts forming the ordinary heel-flap was destroyed, he effected by dissecting up the soft parts from the dorsum of the foot as far forward as the instep, and having completed the disarticulation, and sawn off the ends of the tibia and fibula, this flap was folded down, forming a long anterior flap, exactly as in the amputation in the lower third of the leg, according to Mr Teale's method. The operation was undertaken as affording a more satisfactory site for division of the bone than the amputation in the lower third of the leg, although the resulting stump in such a case could not be expected to be capable of sustaining the weight of the body as in the method by the heel-flap.

### VI. REMOVAL OF PORTIONS OF CATHETER FROM THE BLADDER.

*Dr Watson* showed five portions of catheters, three gum-elastic, two metallic, which he had, within a recent period, extracted, on as many different occasions, from the bladder of a highly hysterical young female. She, it seemed, suffered from retention, and for its relief employed a catheter. According to her own account, by carrying it in her pocket the instrument bent and frayed, and when in her bladder the point *came* off. The fragments were always extracted with facility when the patient was under the influence of chloroform. The metallic with of course greatest ease, as their site could be more easily recognised by the feeling communicated to the urethra forceps or dressing forceps, which he had indifferently used for the purpose of extraction.

### VII. NEW APPARATUS FOR AFTER-TREATMENT OF EXCISION OF KNEE-JOINT.

*Dr Watson* showed the apparatus which he had employed with most satisfactory results in the five last cases of excision of the knee-joint. It consisted of a long gooch splint, an iron rod, a gypsum bandage, and a swinging cradle. The splint extended from the fold of the buttock to two inches beyond the heel, and was applied behind the limb and joint. At the lower part the centre portion of the splint was cut out for four or five inches, so as to leave a horse-shoe opening for the heel. When the splint was applied the two side portions passed along on the level of the malleoli, and projected beyond the sole. The iron rod, which extended from the groin to the toes, was modelled to the outline of the limb, and possessed so much rigidity as to be able to bear the weight of the limb without bending. At the knee the rod formed an arch, so as to leave the surface at this part free from all pressure. At three points hooks, by which it might be suspended from the chain of the swinging cradle, projected from the rod. In application, the gooch splint above described, and the rod

having been adjusted, an open wove bandage was rolled round the limb and apparatus from the toes to the groin, leaving the site of the operation bare to the whole extent of the long anterior flap. The bandage, having then been soaked with a cream of gypsum, retained the limb and apparatus immovably rigid. When the plaster consolidated the whole was slung from a Salter's swinging cradle by the hooks attached to the rod. In the last case of excision in which he had employed this method, the originally applied apparatus was left untouched for two months, by which time the tibia and femur were firmly consolidated, the wound having *entirely* healed a fortnight after the excision was performed. Dr Watson stated that in these cases, as a rule, he employed no dressings of any kind, leaving the wound open to the air; he allowed crusts to form, and avoided all interference till the wounds had cicatrized beneath the crust. Then, and not till then, as indicated by the crusts becoming spontaneously detached, what of them remained were picked away, and the sutures removed.

### VIII. STEEL CATHETER FOR STRICTURE CASES.

*Dr Watson* showed a new pattern of the conical probe-pointed catheter, made of highly tempered steel, which he had upon a former occasion shown the Society. In this new instrument, although as small at the point as the original instrument, it gradually increased in size in the shank up to a No. 7 of the ordinary scale. The orifice, near the distal extremity, was placed in the straight part of the catheter, thus rendering the curved portion more rigid than in the former model. The shape of the instrument was that of a short-beaked sound. The gradual increase in size rendered the instrument easy of introduction in cases of very tight and tough strictures; but, on account of its attaining the magnitude of a No. 7, it was of course best adapted to such strictures situated at or near the bulb of the urethra. Dr Watson could speak with every confidence of it as an instrument extremely easy of introduction, and as one over which the operator had infinitely more control than over an ordinary silver catheter of small size, or even the more slender steel instrument he had previously shown.

---

# PROCEEDINGS OF THE EDINBURGH OBSTETRICAL SOCIETY.

## SESSION XXIV.—MEETING VII.

### 8th March 1865.—Dr GRAHAM WEIR, *President*, in the Chair.

#### I. OBSERVATIONS ON THE FIRST STAGE OF LABOUR.

*Dr Andrew Inglis* read a paper on the above subject, which will be found at p. 24 of this number of the Journal.

*Dr Pattison's* experience led him to to think that the membranes were frequently fully separated, and a bag protruding, yet the os long of being dilated.

*Dr A. R. Simpson* remarked, that in the induction of premature labour the os gets soft and relaxed when you simply introduce the catheter and allow it to remain for a time, without separating the membranes all round. The most favourable cases in ordinary labour, as Dr Inglis said, are those where the membranes are separated before. But how is such separation produced but by slight and painless contractions, which we know often go on for some time before labour. Such painless contractions may continue to dilate the os till the first stage was nearly completed, and one strong pain would be sufficient to bring the head through. Dr Inglis thought protrusion of the bag caused by the separation produced relaxation of the os, but the converse might also hold true, that there was no protrusion without relaxation. Dr Inglis's plan however, he thought, might be a good one, especially in cases of long and tedious rigid os, where, by separating the membranes, you might hasten the progress.

*Dr Keiller* was very sorry that he did not hear Dr Inglis's paper read; but,

with reference to Dr Simpson's remarks, he (Dr K.) considered him right in considering that the separation of the membranes in normal cases was usually produced by uterine contraction. When anything, as e. g., an india-rubber dilater, is introduced into the os, and kept for a time within and in contact wth the cervix uteri, that part gradually gets softer and more relaxed. The great point is to get a good bag of membranes down through the os. He had often observed, in cases of contracted brim, where the bag could not thus get down, you had often to wait long for the dilatation of the os taking place.

Dr *Charles Bell* thought you could have no bag of membranes without con-traction of the uterus. Now, by the act of separating the membranes, you produce a certain amount of irritation which would produce uterine contrac-tion. The merely separating the membranes, without the irritation arising from doing so, would have no effect in promoting labour, as they are so slightly attached that they can be as easily detached as one sheet of paper can be from another.

Dr *James Sidey* remarked, that he had recommended to Dr Inglis the sepa-ration of the membranes round the os as preferable to any other method of inducing premature labour; he (Dr S.) employed this method, and although not so speedy in its effect as some others, he found it produced a safer and more satisfactory labour. The class of cases in which Dr Inglis recommended the same proceeding was where there were long-continued uterine contrac-tions, wearing out the strength of the patient, but producing no effect on the os. In such cases, Dr Inglis found that the separation of the membranes sometimes caused a cessation of the ineffectual pains, and when labour again set in, it went on with great rapidity, the os in the meanwhile having become relaxed. Dr Sidey thought that Dr Inglis was quite right in directing his attention to any means likely to procure a quick and safe delivery, and making it known to the Society, and also in asking the assistance of the Fellows in ascertaining whether there was any good to be got from his suggestion, viz., of separating the membranes at the beginning of labour.

Dr *Weir* believed such cases were due to irregular muscular contraction, and by separating the membranes you directed the movements and allowed the bag to get down to the os.

Dr *Frazer* inquired, whether, in separating the membranes, there was not some risk of injuring the placenta in cases where that organ lay near the os.

Dr *Inglis* remarked, that with Dr Hamilton's sound you could feel when you came upon the placenta. The position of that organ should also be ascertained by auscultation before proceeding.

### II. ON THE MODE OF COMPRESSING THE FEET OF FEMALES IN CHINA.

The *Secretary* read the following notes, which had been drawn up by a patient of Professor Simpson's, from Hong Kong:—

" Chinese women usually commence to operate upon the feet of their female children at the age of four or five years, by binding a strip of cloth about two yards long tightly round the toes, which they turn inwards towards the sole of the foot; the bandage is then passed round the back of the heel, crossed on the top of the instep, and bound round the leg. This bandage is never taken off, but drawn tighter every two or three days; and at the end of the year the toes have become completely embedded in the sole of the foot, except the big toe, which remains in its natural position. Another bandage is then put on, and arranged so as to cause the big toe to curve down and round towards the heel, round which the bandage is passed, so as to push it towards the centre of the foot.

" In about six months, after having been tightened every day, this will have caused the big toe almost to meet the heel, and the other toes to be embedded in the arch of the foot. The top of the big toe and the ball of the heel are the only parts of the foot which touch the ground. This, of course, causes the instep to become very high. The foot and leg are very thin. The foot

appears nothing but bone.  Though these two processes usually occupy about eighteen months without using any very great force, in some instances the whole is accomplished in three months.  This is done by tightening the bandages every day, and the unfortunate child suffers intense agony, and cannot sleep on account of the burning sensation in the foot, which is described as if a hot iron were being held to it.  So much force, however, is rarely used.

" A Chinese lady of high birth, who possesses the true ' golden lilies,' cannot walk without assistance, and has an attendant on either side, on whose shoulder or arm she leans, and even then walks with great difficulty.

" When a Chinaman is seeking a wife, he desires to see not her face but her *feet*, and according as they are large or small he admires her.  The lower class of women only make their children undergo the first process, which, though it makes them walk awkwardly, or rather hobble—to use an expressive word —does not prevent their moving about quickly.  The lowest class of women, comprising the boat-population, and those who work in the fields, do not have their feet interfered with at all, and you will find that they have very small and beautifully-formed feet.  The bandages are kept bound round the feet during the whole lifetime, or if they are taken away, the foot after a time resumes something of its natural shape, though the toes (four toes) remain curled under.

" The origin of the custom is, I believe, attributed to one of the emperors, whose favourite wife proved unfaithful ; and the emperor ordered that all women of high birth should have their feet compressed in such a manner as to compel them to remain in their husband's house, whether they would or no.  As in all countries royalty sets the fashion, so in China it has been followed by all classes ; but amongst the middle class of people the custom is becoming much less frequent.  The small foot is about four inches in length, and I believe, in some instances, three and a-half inches.  The foot which has only gone through the first process is from seven to eight inches long and quite pointed."

*Dr Pattison* remarked, that when in China he was told that an iron shoe was put on shortly after birth ; but Professor Simpson some time ago exhibited a foot of a Chinese lady at the Medico-Chirurgical Society, and the description then given of the method of compression corresponded with that in the paper now read.

### III. CASE OF ABORTION.

*Dr Keiller* exhibited a specimen of an entire ovum expelled at about two months.  He said it illustrated a point to which, although mentioned in some books, sufficient importance was not given.  It was the great care medical men should have in expressing an opinion regarding the duration of pregnancy from the size of the fœtus alone.  Great caution, in all such cases, should be exercised in deciding the probable age of the fœtus, for the ovum may die and the membranes continue to grow, so as to make the ovum seem of earlier growth ; while, in judging from the membranes, these may be dropsical and much larger than the period of pregnancy would lead one to expect.

---

# REPORT OF THE TRIAL OF GEORGE STEPHEN FOR MURDER,

### At Aberdeen, 18th and 19th April 1865.

By ALEXANDER SILVER, M.A., M.D., Assistant to the Professors of Materia Medica, and of Medical Jurisprudence in the University of Aberdeen.

ON Tuesday the 18th, and Wednesday the 19th days of April 1865, George Stephen, wood merchant, Port Elphinstone, Inverury, was tried at the Spring Circuit Court of Justiciary, held at Aberdeen, before the Lord Justice-Clerk and Lord Cowan, for the crime of murder, " in so far as on the third day of December

1864, . . . in or near the plantation on the north side of the avenue leading from the turnpike road to Thainstone House, . . . he did wickedly and feloniously attack and assault the now deceased Ann Milne or Joss, or Forbes, . . . and did, with an axe or some other lethal weapon to the prosecutor unknown, strike her one or more blows on the head, whereby she was wounded on the head and her skull fractured, and she was mortally injured, and died within a few hours thereafter, and was thus murdered." . . .

The points on which this case chiefly depended for its interest were,—1st, The completeness with which the murder was proved against Stephen; 2dly, The state of the prisoner's mind at the time of the commission of the act; and, 3dly, The state of his mind when the sentence of death should have been carried into effect. The prisoner pled guilty to the charge of murder; but a special plea of insanity at the time of the alleged commission of the deed having been put in on his behalf, the trial proceeded.

To take events in their proper sequence, it was proved that the prisoner had been long connected with this woman (twenty-seven years, as stated in one of her letters); that he had been accustomed to visit her at her own house; that she had frequently gone from Aberdeen to Thainstone, a distance of fourteen or fifteen miles, for the purpose of meeting him, and, according to her own account, of receiving money from him. Some of the letters found in his possession, and proved to have been written for her, contained threats of exposure if her demands were not complied with, and one, making an appointment with him for the 3d of December, stated, that she did not want to come to his house (of which he appeared to have complained on a former occasion), but that she *would not* go home till she should have seen him.

On the morning of the 3d of December 1864, Ann Forbes, after borrowing some clothes from a neighbour, set out from Aberdeen, intending to walk to Thainstone and back again. This was about 8 A.M., and nothing further is known of her movements until about 12 o'clock, when she called at an inn at Kintore, asked for half a gill of whisky, of which she drank the half, leaving the other half until her return, at which time she promised to pay for it as she had done on previous occasions. She was next seen a little beyond Thainstone seated on a heap of stones, and asked a boy passing if it was past one o'clock (the letter making the appointment had fixed the time between one and two o'clock). This witness particularly noticed a strip of red flannel she had over her head, and stated that a little beyond and nearer Inverury he met George Stephen walking in the direction of the woman he had just seen, and noticed an axe under his arm. Shortly after a woman passed and also noticed Forbes still sitting on the stones, while a short distance beyond she met Stephen, and saw him enter the wood along with her. She also remarked the axe he was carrying.

What occurred within the wood was unobserved, though, as will be seen, the proceedings may be guessed at with tolerable certainty.

Stephen was next seen walking homewards, without the woman, and apparently carrying something, though the witness who gave evidence on this point could not tell what it was.

About half-past two o'clock, while beating the wood at Thainstone for game, a boy came upon a woman, afterwards identified as Ann Forbes, lying on her face, but slightly inclined to her left side, in an insensible state, and with a wound on the back of her head from which blood was flowing. The piece of red flannel formerly alluded to was still round her head, but had been torn, apparently by the blow which caused the wound. Her person lay upon, or close to, a petticoat which had been spread out on the ground.

In this state she was found by Dr Irvine, who examined the wound and removed some pieces of bone from it. When moving her, an envelope was noticed in her breast, which was addressed "Mr George Stephen, Wood-merchant," but contained nothing except a piece of brown paper.

She survived till about eight o'clock P.M., but continued totally insensible up to the time of her death.

The person of George Stephen was examined that evening; but the examination of the spot where the woman was found and the inspection of the body was postponed to another day. When discovered, Forbes lay in an open grassy spot, thickly covered with withered leaves, and with her head directed from the avenue, which was close at hand. Where her head had lain was a considerable quantity of clotted blood, and some oatmeal pulp, similar to that which was afterwards found in her stomach.

The body was examined on Monday the 5th of December, forty-one hours after death.

The following is the report of the inspection, etc. :—

"We hereby certify on soul and conscience that, between the hours of seven and eight o'clock evening, on Saturday the 3d day of December 1864, we examined the person and clothing of George Stephen, wood-merchant, residing at Port Elphinstone, near Inverury, Aberdeenshire, and that we detected nothing unusual either on his person or clothes, with the exception of a slight rent, apparently recent, on the outside of the right sleeve of his coat, nearly midway between the parts corresponding with the wrist and elbow.

"We further certify on soul and conscience, that, on the Monday following (the 5th day of December 1864), we inspected the dead body of Ann Milne or Joss, or Forbes, wife of William Forbes, residing at Gordon's Court, Virginia Street, Aberdeen, the body then lying in the porter's lodge at Thainstone House, parish of Kintore and county of Aberdeen, when the following appearances presented themselves, viz. :—

"Externally.—The body sparingly clothed and the clothes much dilapidated. The front of the body, including the lips and nails, markedly pale. Dependant parts of the trunk and limbs livid. Backs of the right hand and fingers stained with blood. Blood in a dried and crusted state on the middle of the forehead, on the left side of the face, and on the hair of the head generally, except on that over the right temple. The pupils of the eyes widely opened. Joints stiff. Portions of the skin, not exceeding three-sixteenths of an inch in breadth over the outsides of both haunches, in a dry, brown, and horny state. Old scars in both groins. A large wound of the scalp, very irregular in shape, with blunt and very irregular edges on the upper and back part of the head towards its right side. In its longest direction, which was from a point corresponding with the crown of the head to a point an inch to the right side of the prominence of the hind head, the wound measured four inches ; measured from the crown of the head to a point an inch and a-half above the prominence of the right temple, its extent was three and a-half inches. At or near the middle of the wound its edges were one and a-half inches apart. From the lower border of the wound an irregular flap of skin extended upwards to the length of an inch and three quarters. A second and superficial scalp wound, three-branched or T shaped (the largest measurement of which was a quarter of an inch), situated at a point three-quarters of an inch to the left of the central part of the first wound.

"(Internally).—Scalp pale and bloodless. A clot of blood a quarter of an inch in greatest thickness under the scalp, on the left side of the head, in the neighbourhood of the left ear, measuring in greatest breadth four inches. Blood in the canal of the left ear. In the situation of the large scalp wound, the wall of the skull deficient over an irregular space, approaching to a four-sided shape. The larger measurement of this breach in the skull (which was nearly from before backwards) was two and a-quarter inches, and its shorter, one and three-quarter inches. Loose fragments of bone, of small size and irregular shapes, adhered to the edges of the breach in the skull. There ran from the anterior and right corner of this opening in the skull, a fissure or linear fracture, in a direction downwards and forwards towards the base of the skull for three and a-half inches ; a second fissure from its posterior and left corner downwards and forwards to the posterior hollow at the base of the skull. The outer covering of the brain torn to the extent of three-quarters of an inch at the part corresponding with the back of the breach

in the skull.  Skull unusually thick and solid.  The posterior part of the right half, or hemisphere of the brain proper, irregularly torn up to the depth of an inch and a-half.  A thin layer of clotted blood over the exterior of the right half of the brain beneath its outer covering.  Interior of the brain unusually pale and bloodless.  Mouth, throat, gullet, and air-passages natural.  Blood in about equal and moderate quantities in the cavities on both sides of the heart,—the blood chiefly fluid, but containing clots, partly red, partly yellow.  The interior of the left lung of a bright-red colour and firm consistence.  Thin fluid in considerable quantity in the interior of the lowest division of the right lung.  The parts within the cavity of the belly pale and bloodless.  Thin white pulpy matter in the stomach, containing particles of oatmeal, and smelling faintly of spirits.  The womb unusually small and its opening closed.

"Nothing else unusual observable on the exterior of the body, within the cavities of the head, chest, or belly or about the top of the spine.

"We also certify on soul and conscience, that from the above inspection of the body of Ann Milne or Joss, or Forbes, we are of opinion that the large wound of the head, the fracturing of the skull, and the consequent loss of blood, had been the cause of her death, and that these injuries had been the effects of violence inflicted during life.

(Signed)      "F. Ogston, M.D.
              Alexander Silver, M.D.
" Aberdeen, 8th December 1864."        Al. Irvine, M.R.C.S. Eng.

After reading this and another report to be mentioned presently, Dr Ogston was examined as to the prisoner's state of mind at the date of the murder.  He said, that on the evening of the murder he saw the prisoner at Kintore while in custody, but that he had very little conversation with him, not enough to form an opinion as to his insanity; that there was a degree of levity or indifference in his manner, but whether this was natural he had no means of judging; the prisoner was also quite intelligent, though he (Dr O.) had *very* little means of judging that.  He also stated, that he saw the prisoner again on Sunday in the presence of the murdered woman, when he was very quiet, though the muscles of his face appeared to be quivering and he seemed agitated.  At that time he saw nothing to lead him to judge of the prisoner's disposition.

On searching Stephen's premises, three axes were discovered and handed over to Professors Ogston and Brazier for examination.  On one of these, the head of which corresponded very closely in point of size with the breach in Forbes's skull, were discovered reddish stains, which, on a chemical and microscopical examination, proved to be blood.  On the others nothing beyond stains of rust could be made out.  In a crevice of the wood, near the head of the first-mentioned axe, certain minute scarlet fibres were also discovered, which, when viewed by the microscope, and compared with those constituting the piece of red flannel formerly mentioned, as having been found round Forbes's head, were seen to correspond in every respect.

Several witnesses were called, who spoke to the absence of any excitement or anything unusual in the prisoner's demeanour on the day the murder was committed.

On the part of the defence no attempt whatever was made to gainsay the committal of the deed, it being entirely limited to the proof of the prisoner's insanity.

The principal facts proved in connexion with this were as follows :—In the course of 1864, the prisoner, who up to that time had a good business, suffered some severe pecuniary losses, which appear to have affected him considerably.  After this he appears to have altered in his demeanour, which was described as having been cheerful before this event, but dull and "dumpish" after it.  Even before these losses, however, he is described as having been troubled with a peculiar hesitation in his speech, which was said to have come on about twelve

months before any other peculiarity was observed. One Sunday morning in August 1864, he is said to have come down from his bedroom in a highly excited state, and shaking violently, and to have declared to his nieces, who lived with him, that he did not know what to do with them now, as his money was all done, and that there was neither meal nor fire in the house; although such was not really the case. He is also said to have complained that he could not sleep, his bed being only a heap of stones. Some time after this, Dr Thomson of Inverury was called in, who ordered a blister to the back of his neck and some purgative medicine. He does not appear to have improved much at first, since in October he is described as having several fits of an epileptic nature, for one of which Dr Paterson, assistant to Dr Thomson, was called. After this date, however, he appears to have got decidedly better, and the delusions seem to have been removed, though a childishness of manner remained behind. This was evinced in a tendency to talk of trifling circumstances, and a continual recurrence to such, although attempts were made to change the subject to others of more importance.

The principal medical evidence brought forward in his behalf was that of Drs Thomson, Inverury, and Jamieson, superintendent of the Aberdeen Lunatic Asylum.

*Dr Thomson* stated that he had known Stephen upwards of forty years, but had never attended him professionally until September last, about the 23d or 24th of the month; that he then found him leaning against the door of his house apparently stupid and confused, but that he noticed nothing particular in his eye at that time, though he had observed a change in his general appearance for months before; that his utterance was affected, and that he did not appear to have the perfect use of his tongue, it being apparently paralyzed, which is often a symptom of general paralysis. His treatment was by blistering and aperients, intended to remedy a loaded state of the brain. He (Dr T.) had no doubt that Stephen was labouring under disease of the brain, and he had some difficulty in getting him to apply the blisters that were ordered. On one occasion, when visiting Stephen, he noticed the glare of a madman in his eye, and warned his friends to watch him closely. At this time Stephen was labouring under a disease of the brain, which was accompanied by delusions and a childishness of manner; this he (Dr T.) did not think curable, and would be likely to produce insanity, though at that time there was no good reason for sending him to an asylum. He also stated that he would not be surprised though under irritation Stephen had committed murder, as he would have difficulty in resisting an impulse; and, further, that imprisonment would probably have a beneficial effect on such a state of mind.

When cross-examined, Dr Thomson said that the state of his affairs would have a bad effect on Stephen's mind; that he was somewhat recovered by November, though not completely, and that he (Dr Thomson) did not expect him ever to be better. He also mentioned that his evidence was founded partly on observation, partly on hearsay.

*Examined by the Lord Justice-Clerk.*—Dr Thomson said that he was called to visit Stephen in consequence of the delusions Stephen was labouring under, and that these continued up to the end of October, but that, sometimes at least, he would be capable of knowing right from wrong, while he would know that crime would be followed by punishment, and that if he knocked a man or a woman down with an axe he would kill him or her. Dr Thomson further stated, that epileptic fits were sometimes accompanied by delirium, which would wear off after the attack; that the last epileptic attack Stephen had was about the 18th or 20th of October, but that he believed that Stephen's mind had been weakened for months before he saw him.

*Dr Jamieson* stated that he had visited Stephen but only in prison, with a view to ascertain whether he laboured under insanity, and that in his opinion Stephen was labouring under disease of the brain. He was led to form this opinion from the man's general aspect and manner, his particular mode of articulation, his admission of the imperfect way in which he slept, his general

inattentiveness and confusion of mind when spoken to, and his apathy and want of curiosity. This opinion would have been strengthened had he been informed that the man had suffered from epileptic fits and partial paralysis. He said that excitement, acting upon a person in such a state, might produce an uncontrollable impulse to commit an act of violence, and that insane persons who were also epileptic were more dangerous than those who were not affected by epilepsy. He was of opinion that, from what he had observed himself and from what he had heard, that at the time the prisoner was labouring under the delusions already mentioned, he was of unsound mind, and that he would not have hesitated to grant a certificate to that effect. He also said, that he would not have been surprised to hear that the prisoner had committed a deed of violence early in December, his state in the month of October being as above detailed; on the contrary, such a deed would be a probable result of the brain disease.

*Cross-examined by the Advocate-Depute.*—Dr Jamieson stated that though he had the opportunity of talking with the prisoner, he could make out no delusion about him, nor could he say that he did not know right from wrong. *Examined by the Lord Justice-Clerk.*—Dr Jamieson said that he had visited the prisoner four times, viz., on the 10th and 16th of December, and on the 19th and 30th of March, and that his answers applied chiefly to the first of these visits; that he endeavoured to detect delusions, but could not; and that he talked to the prisoner chiefly about his personal feelings, his feelings to others, and on his ideas on the subject of his stomachic ailments and prison diet, and that on the 10th December there was no symptom of insanity about the prisoner except his peculiar appearance, which was the result of brain disease, not of insanity.

This concluded the evidence in the case.

In summing up, the *Lord Justice-Clerk* drew the attention of the jury to the fact, that they had not to decide as to the prisoner's insanity at the time of the trial, but as to his state of mind on the 3d of December; for were it made out that he was insane at the time of the trial, he would not have been allowed to plead to the indictment; he then proceeded to say:—

" But I must tell you further, and tell you very decidedly, in consequence of the line of argument adopted by the prisoner's counsel, that disease of the brain is not insanity. Disease of the brain is bodily disease, and insanity is mental disease; and no amount of bodily disease will justify you in pronouncing that this prisoner was insane on the 3d December, or at any other time. Mental disease is often produced by disease of the brain—often accompanied by it—but what you have to try is not whether he had disease of the brain, but whether that produced insanity. There are some other matters connected with the doctrine of legal insanity which it is quite necessary to give you directions about at the outset. Insanity is a term capable of being used in several meanings; and it is very often used by gentlemen of the medical profession in a totally different sense from what is in use in courts of criminal jurisdiction. A man's mind may be weakened by disease, and may, in a certain sense, be called insane; but not on that account does he cease to be morally and legally responsible for his actions. There are many persons whose minds are naturally weak, and men whose minds may have been weakened or impaired by the action of bodily disease; but those persons are not held irresponsible for their actions. But in order to justify a verdict of insanity in a case of crime, you must find that man's mind not only weakened and impaired by disease, but be satisfied that he does not know the difference between right and wrong in the crime with which he is charged. Either he does not know the act with which he is charged, or he does not know the kind of act, or if he does know it as a physical fact, he is unable to know he is guilty in a moral or legal sense, and incapable of appreciating its consequences and effects. While a man is in that condition he is not in the eye of the law insane. If he knows what he is doing—if he understands that in committing the crime of murder, for instance, he is really killing his victim—if he knows that that is a sin—that it is wrong and not right—that such an act will bring him under the cognizance of the criminal law, and lead to punishment, then he is morally and legally responsible, no matter how weak otherwise his mental faculties may be.

Extraneous causes, such as excitement and provocation, have nothing to do with the question of insanity. A man whose mind is weakened or impaired may be more easily excited and provoked than another, just as a man in bad health may be easily irritated; but does he, therefore, cease to be a responsible agent? That is quite out of the question."

Speaking of the state of the prisoner's mind at the time the deed was committed, his Lordship said:—

"Then, Gentlemen, to complete the digest of the evidence as shortly as I can, it is necessary to advert to the evidence of Dr Ogston, the chief medical witness for the Crown. His evidence bears, in the first place, chiefly on the state of the body of the deceased, and the cause of death; and I need not enter into detail on that. But he says he saw the prisoner upon the day immediately succeeding the murder. He was then in presence of the body. He said he was very calm, but he could see that the muscles of his face were quivering a little, betraying a certain amount of agitation; and I think that what Dr Ogston says as to the prisoner's demeanour was corroborated by everybody who saw him, either on the afternoon of the 3d or on the Sunday after he had been apprehended. They all describe him as being perfectly quiet; no appearance of agitation or excitement."

In another part of his charge, his Lordship laid down the law of lunacy in criminal cases as follows:—

"Now, Gentlemen, as I said before, you must not mistake brain disease, or weakness of mind produced by brain disease, for that kind of insanity which alone exempts from legal responsibility. I have told you already, and I repeat it, that unless a man is in such a condition from mental disease as to be bereft of reason, and not able to understand what he is doing,—if he does not know what is the act which he commits, and is unable to appreciate its nature and quality, or to understand its consequences and effect,—if that is true, then no doubt the person of whom it is proved is insane, and not legally responsible. But if, on the other hand, you are satisfied that at the time he committed the act the prisoner knew the difference between right and wrong, understood the nature of the act he was committing and the consequences that would follow; then, however much his mind may have been impaired by disease, however much he may have been excited or irritated, however much he may have been influenced by the operation of extraneous causes, he is a moral agent, and answerable to the law."

After twenty minutes' deliberation, the jury, by a majority of nine to six, found the prisoner guilty as libelled, and he was accordingly condemned to death.

A great deal of interest having been taken in the case, application was made to the Sheriff, who, after this sentence had been passed, ordered an examination of the prisoner, in terms of the Lunacy Act (20° and 21° Vict. cap. 71).

In Clause LXXXIX. this act provides that,—"If any person, while imprisoned in any prison or other place of confinement, under any sentence of death, transportation, penal servitude, or imprisonment, or under charge of any crime or offence, or under any civil process, shall appear to be insane, it shall be lawful for the sheriff of the county where such person is imprisoned to inquire, with the aid of two medical persons, as to the insanity of such prisoner; and if it shall be certified by such sheriff and such medical persons that such prisoner is insane, it shall be lawful for one of her Majesty's Principal Secretaries of State, upon receipt of such certificate, to direct, by warrant under his hand, that such person shall be removed to such asylum as the said Secretary of State may judge proper and appoint," etc.

In accordance with this Act, a certificate of the prisoner's insanity, signed by Drs Macrobin and Fraser of Aberdeen, was forwarded to Sir George Grey, who sent down directions that the sentence was not to be carried into effect, but has not yet issued a warrant for his consignment to any asylum.

The completeness of the proof in this case is one of its most remarkable features, for except the whole affair had been seen, it would have been difficult to have made the evidence stronger. The appointment, the meeting, and entering the wood together, the axe with its stains and woollen fibres, and,

finally, the motive in Forbes's continual persecution and threats of exposure, all go towards rendering the case one of the best instances of the power of circumstantial evidence on record. From circumstances previously mentioned, the proceedings within the wood might be deduced something as follows :—Having gone into the wood a sufficient distance to screen them from observation, and having reached a spot tolerably free itself though sheltered on all sides, Stephen may have asked Ann Forbes to sit down while they discussed the subject of her visit; having removed one of her petticoats for the purpose of protecting them from the wet ground, she appears to have spread it out and been in the act of rising, when the prisoner, who must have stood behind her, struck her a fearful blow with his axe. Only one blow appears to have been struck, for, though the wound in the scalp was very irregular, when struck by a broad flat body it usually splits up in various directions, acting like a brittle substance rather than as a tough tenacious membrane. What is perhaps unique in this case is the circumstance, that the breach in the skull, allowing for some irregularity of its edges, almost exactly corresponded with the head of the stained axe. This may have been owing in this particular instance to the great force with which the blow must have been inflicted.

With regard to the mental condition of Stephen, it seemed to be more that of a man of low mental organization than of one actually insane; he presented during the whole proceedings the appearance of the utmost indifference, indeed he seemed by far the least concerned in the whole Court. Once or twice during his trial he appeared to sleep, and when the jury returned and were about to give their verdict, being detained by another case which was going on at the time, he coolly continued to stare round the court-house, never once turning his eyes towards the assize. Even after the Lord Justice-Clerk had pronounced his sentence in a manner which affected every one else present, the prisoner moved off in the calmest possible manner, and immediately on his return to gaol is said to have coolly demanded his dinner. When confronted with his murdered victim, as stated by Dr Ogston, he evinced no other sign of emotion than a slight twitching of the muscles of his face. From first to last his conduct has been as cool as it could possibly be, and the fate that now awaits him will probably be to him a far greater punishment than hanging would have been.

On the merits of the legal doctrines, propounded by the learned Judge, it is not for me to speak, though they have been already commented on pretty extensively by the general public. In all probability they will be viewed differently by lawyers and medical men, particularly by those practitioners connected with the management and treatment of the insane.

Since writing the above, a letter has been received containing Her Majesty's pardon of George Stephen for the crime of murder, and a commutation of the sentence of death passed upon him for one of penal servitude for life. In accordance with this, Stephen has been conveyed to the Perth Penitentiary instead of a lunatic asylum as was originally anticipated. This apparent change in Stephen's sentence has been rendered necessary by the Perth Penitentiary not being recognised as a lunatic asylum under the Lunacy Act, so that penal servitude had to be substituted for the confinement in a lunatic asylum, that he might be received at Perth. He will, however, be treated in all respects as insane, and not as an ordinary convict.

---

# ARMY MEDICAL DEPARTMENT, AND CONCERNING THE LATE INFLUX OF MEDICAL CANDIDATES.

CONSIDERING the number of candidates that lately presented themselves for examination at the Army Medical Board—upwards of four score and ten—it would seem that there is no scarcity of the medical material; and, without doubt, it will rejoice the hearts of the authorities at Whitehall Yard that they

are so easily enabled to fill up the existing vacancies, and to obtain such numerous applicants to their advertisement with so little trouble, thereby enabling them to dispense with the assistance, timely rendered, of the late acting assistant-surgeons.

Neither those ardent youths who aspire to the honourable position of assistant-surgeon in Her Majesty's service, nor their happy friends, can form any idea of the duties they are called on to perform. They are in great measure consigning themselves to a life of self-willed banishment; but

> "Hope springs eternal in the human breast,
> Man never is, but always to be blest."

So says the paraphrase; but the old worn-out and aged military medical officer thinks otherwise, when functional decay manifests itself in the infirmities of old age, and the constitution, broken down through toil and exposure in a tropical clime, and nature demands peaceful retirement, this aged public servant, after a life of plodding usefulness, is often compelled, from some cause or other, to toil on insensible to time's control.

In the subordinate ranks of the Medical Department, and indeed in not a few of the walks of life, influence, from connexions or from some other means, has as much, if not more, to do with a man's advancement in life than any merit inherent in himself. How often do we see men prospering in life, not through any merit or talent of their own, but solely from having "greatness thrust upon them."

The medical candidate having his intellect polished through the medium of the Netley School, emerges therefrom full of critical lore, undismayed at the prospects and difficulties before him. Encouraged by his ministering angel, he goes boldly forward, determined to succeed; if possible, to become an administrator in the ranks of the department; be a reformer of evils and abuses; and bring his acute knowledge and medical acumen to bear on subjects on which less able men could make no impression Should some cause or other impede his advancement to those high administrative positions, he must content himself with the higher ranks of the executive; and although ere this discontent may pervade his feelings, and he may have become irritable and irascible, there is still scope for his philanthropy and alleviating powers; and, as his ideas and feelings have become matured, he will in all probability have observed that numbers of deserving medical officers have been slighted and passed over, through no want of merit on their part. Still in all justice let it be said, that it is often impossible for the head of the department, even with the complicated confidential returns to aid him, to select at all times the "right man for the right place."

The duties of the military medical officer differ widely from those performed by his brethren practising in civil life; though both are exercising the same profession, they are engaged in totally distinct departments from one another.

From the day the young medical officer enters Her Majesty's service, he undergoes a continuous system of military training. It is necessary for the proper performance of his duties that he should be intimately acquainted with the duties the soldier is called on to perform, and learn the working of the interior economy of the service; but more particularly that which refers to a regiment. The medical officer is bound and restricted by rules and regulations often complicated and cumbersome, which, in numerous instances, embarrass him in the due performance of his duties, the prejudicial effects of which are to leave him open to be assailed in a needless manner by other officials It will therefore be observed that the medical officer, being only responsible for his advice, is by no means invested with that power which some suppose themselves possessed of. And the following clause from the late "Indian Warrant" will show that the authorities by no means wish them to acquire habits of independence, as the holding of civil Indian appointments might render them:—"Officers so employed might acquire habits of independence inconsistent with a due performance of regimental duties, and would have to return to a regiment at an age when men in general do not easily bind themselves to the

practice of subordination required from a regimental officer of inferior rank." The above is pretty conclusive, and for the future excludes all Queen's officers from holding any appointment unconnected with H. M. troops, as it might give them a spirit of independence which is inconsistent with their duties.

As the duties of the military medical officer are different from those in civil life, a corresponding change takes place in his habits; he follows a daily routine; and while during warlike operations he is equally exposed to as great danger as the combatant, and equally liable to the fatigues and hardships consequent on this kind of life, he is more liable to be attacked, from the nature of his avocations, by some of those dangerous epidemics, and other equally powerful though insidious diseases, which decimate armies; and should any of these epidemic scourges at any time make their appearance, in one or other of the numerous British dependencies, he is equally liable to be ordered there, to ameliorate the sufferings of the community, and be equally exposed to its infectious influences, the dangers of which, in numerous instances, are tenfold greater than those arising from war.

By looking at the long list of assistant-surgeons, and the average number of promotions in preceding years, it will be observed that the annual number of steps is amazingly small; and if promotion should proceed at a like rate, the chance of those now entering the service of ever obtaining to the higher branches of the department is very limited. For the better elucidation of this, it is only necessary to show the average rate of promotion from assistant-surgeon to surgeon during three years, commencing with 1860:—" On the 1st January of that year (1860), there were 1075 medical officers on full-pay. During the year, 38 assistant-surgeons entered the service, 32 medical officers died, 7 resigned, 11 retired upon permanent half-pay, and 17 were placed on temporary half-pay." Again,—" On the 1st January 1861, there were 1033 medical officers on full-pay. During the year, 22 assistant-surgeons entered the service, 15 medical officers died, 9 resigned, 8 retired on permanent half-pay, and 27 were placed on temporary half-pay."

The looker-on would naturally suppose that this imposing array of deaths, resignations, and retirements, would bring with it a corresponding amount of promotion; nevertheless, by referring to the records, we can only trace fifteen promotions as having taken place within the two years. In 1860, eleven promotions, and in 1861 only four promotions from the rank of assistant-surgeon to that of surgeon; and in the latter year three of the four promotions were conferred on gentlemen for service on the west coast of Africa; and as officers serving on that station are placed on a different roster from other medical officers, and are only available for that particular duty, and their promotion from their particular service being special, it may be said that only one promotion actually occurred which benefited the regular medical officer.

" On the 1st January 1863, there were 983 medical officers on full-pay of all ranks. During the year, 49 medical officers were brought in from half-pay, 57 assistant-surgeons entered the service, 17 died, 17 resigned, 10 retired upon permanent half-pay, 13 were placed on temporary half-pay, and the services of 4 were dispensed with."

Of the 49 medical officers brought in from half-pay, we conceive that a large proportion of this number were assistant-surgeons, who had, from reduction of the establishment, been placed on half-pay as a temporary measure shortly after passing through Netley School. The promotions of this year were 19, and of this number several may be accounted for on account of the threatened troubles with the United States. Since the above-named year, promotion would seem to proceed in the same even slow rate; and should no increase take place, or no amelioration of the condition and position of the present assistant-surgeons, it is very possible, nay even probable, that the generation of assistant-surgeons now entering the service may have to serve in the rank of assistant-surgeon for upwards of seventeen years, or even longer.

A near approximation to the rate that promotion will go, to those entering the service at the present time, may be obtained in the following manner:—

From the list of medical officers on full-pay, corrected up to 31st March 1864, we find that the total amounts to 1025, including all ranks. Of these, 671 are assistant-surgeons, the remainder being above that rank; and, assuming that the number who remain in the service over 25 years is counterbalanced by the number put on temporary half-pay who die, and those who leave the service from other causes, we can arrive at a pretty accurate result, by finding that 16 years and 192 days' service bears the same proportion to 25 years' service that the number of assistant-surgeons bears to the number of medical officers of all ranks;—thus:

| No. of Medical Officers. | | No. of Assist.-Surgeons. | | Years. | | Years. | Days. |
|---|---|---|---|---|---|---|---|
| 1025 | : | 671 | : : | 25 | : | 16 | 192 |

As a considerable number of assistant-surgeons was required to fill up the existing vacancies at that time, i.e., 31st March 1864, we have no hesitation in arriving at the conclusion of the correctness of our estimate as above, of promotion not being obtained under 17 years of service.

In recording the above facts, we have no wish to deter any one from entering Her Majesty's Service; but if their object is to obtain more than a bare competence, they will do well to consider the slow rate of promotion, as well as the already swollen list of assistant-surgeons, and they will in all likelihood come to the conclusion that their opportunities are but limited, and that the advantages of being connected with the Army Medical Department do not compensate for the self-inflicted banishment, and the dangers and epidemics incident to a tropical clime, which they are called on to undergo.

Regarding the emoluments derivable from the service, they are not such as to compensate by any means for the time spent in acquiring professional knowledge. The actual daily pay, at first sight, may appear large, in comparison with that of a young subaltern officer; but, in the latter instance, it is not uncommon for a young officer to attain a higher grade in a very short period of time, and receive consequent increase of pay; while, in addition to this, there is always open to him a multiplicity of appointments, which carry with them additional pay and allowances; whereas in the case of the medical officers no inducements of such a nature are seen looming in the distance, and the only increase of pay which he can avail himself of is the usual respectable increase of eighteenpence per day for every five years of service; and after twice five years has been completed, even this small pittance also ceases till promotion ensues, no matter what period of time may elapse. In addition to this, the medical officer is equally liable for all regimental subscriptions, as well as other casual and incidental expenses too numerous to mention; and as promotion seldom takes place in his old regiment, when gazetted to a new corps he is again called upon to pay heavy additional contributions on the whole amount of daily pay, and not only on difference of pay between that of assistant-surgeon and surgeon, as is the case with combatant officers, who almost invariably get promoted in the same regiment. When all these are deducted from a medical officer's pay, it dwindles down materially, and is not that handsome sum which shows so splendidly and largely on paper.

In former days, a certain grade existed—1st-Class Staff-surgeon—which was looked upon by all branches of the department as an intermediate link between the executive and the administrative ranks, and was considered as a stepping-stone to the latter. This has been superseded by the present grade of surgeon-major, a rank obtainable by all after twenty years of full-pay service, no matter what the merits or the demerits of the officer may be. Old officers who had attained the first-named rank have always looked upon themselves as aggrieved, having been taught that he who obtained the rank of 1st-Class Staff-surgeon was more certain of rising to the administrative branch than a staff or regimental surgeon.

Considering the numerous articles we have perused on the grievances of the Medical Department, it is evident there is a considerable amount of discontent and wide-spread dissatisfaction in the ranks of the department. Numerous

solutions and suggestions regarding this difficult question have been pointed out, and urged on the authorities in a spirit of courtesy. Nearly all may be summed up as follows, to wit :—

1*st*, Earlier promotion from the rank of assistant-surgeon to surgeon.

2*d*, Increase of pay in the executive branches.

3*dly*, Earlier retirement,—say from fifteen to twenty years of full-pay service.

4*thly*, Increase of retiring allowance.

Each and all of these have been so largely commented upon that it is needless to enter into the subject ; still it is not out of place to reiterate even here, that, by granting the above, the authorities would offer a stimulus to young and able men to come forward and allow some little chance of gaining the higher branches ; it would also open a wider field for exertion, and give a healthy tone to rivalship, besides infusing a certain amount of vigour into the department ; and the authorities would always be able to command the best men from the Medical Schools, men of vigorous intellect and full of physical energy. It is undeniable that the officer at the head of the department has difficulties of no mean nature to contend with, and that pressure is brought to bear upon him which he is sometimes unable to resist, when striving to obtain benefits and rectify grievances. Nevertheless, outcry is so loud and numerous withal, and the grievances are so pointed, that it is necessary that something should be done to allay the present great dissatisfaction.

We are aware that numerous difficulties surround the subject, but we think that it would be advisable for the authorities to make a timely concession to a wide-spread grievance ; and such would be both grateful and commendable, and would induce first-class men eagerly to aspire for the service.

---

## THE BRUCE OF GRANGEHILL AND FALKLAND SCHOLARSHIPS AND BURSARIES.

WE have much pleasure in stating that Mrs Tyndall Bruce of Falkland has given to the University of Edinburgh the sum of £10,000 to found three scholarships of £100 each, three bursaries of from £30 to £35 each, and a prize of £20 in the Logic Class, in honour of her late distinguished uncle, John Bruce, Esq., of Grangehill and Falkland, who was Professor of Logic between the years 1774 and 1786. The object of this noble gift—the largest that has ever been made to the University of Edinburgh for the general purposes of education—is, as stated by the generous donor, " To promote the wellbeing of the University, but specially the encouragement and promotion of studies in the departments of Classical Literature, Moral Philosophy, and Mathematics, and to afford pecuniary assistance to meritorious students who may desire to prosecute in the University the studies of Divinity, Literature, Science, Law, or Medicine." The conditions and regulations in the deed of gift are in every respect most liberal and judicious. The patronage of the scholarships and bursaries is vested in the Senatus Academicus. The duration of the scholarships is three years, and in the third year the student may prosecute his studies in an English or foreign university. The scholarships are to be conferred on students under twenty-one years of age who have passed their examination for the degree of Master of Arts. The first scholarship at the end of next session (1865–1866) for three years, the second at the same time for two years, and the third for one year ; so that a scholarship may fall vacant at the end of each session. No student can be appointed to a scholarship who holds a scholarship or fellow-in any Scotch university. The first bursary is to be conferred on a student of the second year for three years at the end of the session 1865–66, the second at the same time for two years, and the third for one year, that a bursary may fall vacant at the end of each session.

## THE MEDICAL ACT AMENDMENT BILL.

WE may state with confidence that it is not the intention of the Government to assume any responsibility in respect of this Bill during the present session. It is understood that opposition to it will arise from various quarters, and especially in connexion with the exclusion of the Bachelorship of Surgery of the University of London from the Register, and with the stated rights of certain of the colonial universities. It is not probable that sufficient progress could have been made with the opposed Bill to afford a prospect of legislation this year, and we can only hope that next year the difficulties anticipated may be satisfactorily smoothed away. The present position of affairs is in the highest degree unsatisfactory, since the Register affords little protection either to the public or the profession.— *The Lancet.*

## DR MACLAGAN.

THIS good old man—who never made an enemy and never lost a friend—the valued family doctor and friend—the public-hearted citizen—the genial companion of our best men for fifty years—died on 6th June—gathered into the garner of the great husbandman like a shock of corn fully ripe. His well-known person, his hearty smile and kindly greeting, have been missed from our streets for more than a year, and for some weeks he has been gently dying—*felix opportunitate mortis,* with his unfailing life-companion and her seven sons, an unbroken family, around his bed. Dr Maclagan was born in Edinburgh, in February 1785. He took his degree in 1805; and, having resolved to join the medical service of the army, he went to London, studied at St George's Hospital, and became a member of the Royal College of Surgeons of England in 1807. His first service was with the 91st Regiment. With the 91st he went, in 1809, to Walcheren, and had the mortification of seeing the splendid battalion to which he belonged reduced by death and sickness in a few weeks from a thousand men to something less than one effective company. In November 1811, he sailed for Lisbon, to join the army under Lord Wellington. He was appointed staff-surgeon to the 9th Portuguese Brigade, which he joined in the investing ground before Badajoz. He continued to serve with the Fourth Division till September 1814. He was present at the storming of Badajoz, and at the battles of Salamanca, Vittoria, Pyrenees, Nivelle, and Nive, and, therefore, in due time received the Peninsular medal, with six clasps. His professional skill and ceaseless activity in the discharge of duty secured for him the most flattering expressions from all those under whom he served, including, among others, a special notice in an order of the day by Marshal Beresford, who praised him " for the promptitude and zeal displayed by him in the care of all the wounded, in having them accommodated and attended to, and their cases treated on the spot." The result of these distinguished services was his promotion to be Physician to the Forces, which rank he held when the Portuguese army went home from the campaign in France; and his return to England, *viâ* Lisbon, instead of a more direct route, was owing to his detention by Sir Benjamin d'Urban, Quartermaster-General of the Portuguese army, who stated that he valued Dr Maclagan's services so highly "as to be obliged to make a point of retaining him to superintend the hospital arrangements of the Portuguese army." In 1816, having gone on half-pay, he settled in Edinburgh. From this period to his death, Dr Maclagan is identified with his native city. He was elected President of the Royal College of Surgeons in 1826. In 1848, he became a Fellow of the Royal College of Physicians, and President in 1856. He thus had the unique honour of having been President of both the great medical incorporations of Edinburgh. Dr Maclagan leaves seven sons. His eldest is our excellent Professor of Medical Jurisprudence; another, Colonel Robert, is a distinguished officer of Engineers.—*Scotsman.*

## INFLUENCE OF THE IMAGINATION ON THE FŒTUS IN UTERO.

*(To the Editor of the Edinburgh Medical Journal.)*

SIR,—I shall offer no apologies for requesting the favour of your inserting the following striking cases of double hare-lip, which have fallen under my care, and which, I think, cannot fail to convince the most sceptical of the fact, that the imagination of the mother has much to do with the perfect or imperfect development of the fœtus in utero.

Nearly two years ago I attended a very shrewd and intelligent young woman, about twenty-three years of age, of a thin, spare, strumous habit, and exceedingly nervous temperament, during her first confinement. The labour was severe and tedious, and the child of great size and unusually muscular. It is either Mauriceau or La Motte who says, that this is a very common circumstance in cases where there is great and irreparable deformity. In the case I am relating, the infant had a fearful-looking double-fissured hare-lip, with a broad and long cleft in the palate, and from this distressing and unlooked-for casualty there was an unusual silence in the apartment immediately after the birth; but the mother instantly suspected that there was something wrong with her child, and at once guessed, not only the nature, but the extent of the deformity.

The *second case* occurred a little more than twelve months afterwards. The mother was a very accomplished and well-educated person. It was her fourth child. The other three were all well formed. The labour in this case also was more tedious than usual, and the child stout and muscular. Immediately after birth there was the same ominous silence, and from the same cause as in the last case, which was not at all lessened by my demeanour, impressed as I was with a feeling that certainly some fatality was attending my midwifery practice. Almost in the same language, and in a moment, the mother exclaimed that her infant was deformed, that she was sure it was so, exactly as in the case of her neighbour's child.

I shall now refer to the supposed causes of these disfigurations,—the one following so soon after the other. In the first instance, the mother from her childhood had attended church, sitting in a pew not many yards distant from that of an old man of a most forbidding aspect, with a double fissured, ill-conditioned hare-lip, exactly resembling those of the two infants. I am in the practice of seeing this person almost daily in the streets, and his appearance is the most revolting that can be well conceived. My patient had never paid much attention to him before, nor did his appearance cause any particularly disagreeable feelings, until after her marriage and the commencement of pregnancy. On the occurrence of the latter, there came a fascination about him, which she could not resist, and which she manifested by going oftener than usual to church, and while there, shifting her position in order to have a more perfect and full view of the deformity. These disagreeable sensations continued up to the time of her confinement, accompanied with most distressing prognostications, particularly in her dreams, as to what her child would be like; all of which she concealed from her family, even her husband and mother never suspected the distress and sufferings she underwent.

The second case happened in the same street, and within a few doors of the other. In childhood the mothers had been playmates and friends, and when the younger appeared with her infant out-of-doors, the other, who knew of the deformity, had a vehement desire to raise the shawl and look at the child; this she did frequently, even waylaying her during her walks, and scarcely could be debarred from visiting, although there had been for years a coldness between them. Neither of the parties had ever heard of any of their relatives having any such deformities in their families before. Both children were spoon-fed, became gradually much emaciated, and died, the one in the sixth, and the other in the eighth month,—apparently from teething. I believe that there are more cases of hare-lip than is generally supposed, the majority dying

during infancy, partly from want of proper nourishment; while the imperfect performance of mastication, etc., leads to many, and fatal diseases of the digestive organs. It is remarkable that in a midwifery practice, including nearly three thousand cases, these are the only deformities I have had of any kind whatever.    J. P.

## DR CHRISTISON.

At the recent Commemoration at Oxford, the degree of D.C.L. was conferred upon Professor Christison of the University of Edinburgh.

## APPOINTMENT.

Professor James Spence has been appointed Surgeon in Ordinary to the Queen, in room of the late Dr David Maclagan.

## PROFESSOR CZERMAK.

We understand that Professor Czermak of Prague has been appointed Professor of Physiology at Jena, and has already commenced his Summer Course of Lectures there.

## PUBLICATIONS RECEIVED.

Brochard, — Sea-Air and Sea-Bathing for Children and Invalids. By M. le Dr Brochard. Translated and Edited by William Strange, M.D. London. 1865.

Caplin, — Selection of Documents and Autograph Letters in Testimony of the Cures effected by the Electro-Chemical Bath of J. F. I. Caplin, M.D. Translated from the French Edition. London, 1865.

Casanova, — Contribution to Physiology and Medical Jurisprudence. By John N. Casanova, C.M.D. London, 1865.

Lunacy, 7th Annual Report of the General Board of Commissioners in, for Scotland.

Pauli, — Der Group. Von Dr Friéderich Pauli. Wurzburg, 1865.

Sansom, — Chloroform: its Action and Administration. By A. E. Sansom, M.B. London, 1865.

Sédillot, — Traité de Médecine Opératoire: Bandages et Appareils. Par le Dr Ch. Sédillot. Third Edition. Vol. 1. Paris, 1865.

Smith, — Hay-Fever, Hay-Asthma, or Summer-Catarrh. By W. Abbotts Smith, M.D., etc. London, 1865.

Solly, — Surgical Experiences: the substance of Clinical Lectures. By Samuel Solly, F.R.S., etc. London, 1865.

Solomon. — Tension of the Eyeball, Glaucoma, etc. By James V. Solomon, F.R.C.S. London, 1865.

Stanski, — De la Contagion dans les Maladies. Par M. le Dr Stanski. Paris, 1865.

Wilson, — Student's Book of Cutaneous Medicine and Diseases of the Skin. By Erasmus Wilson, F.R.S. London, 1865.

## PERIODICALS RECEIVED.

Berliner klinische Wochenschrift,—Nos. 22 to 25. 1865.

Births, Deaths, and Marriages, Monthly Return of, for May 1865.

British Medical Journal, — June 3 to 24, 1865.

Bulletin Générale de Thérapeutique.—April 15, 1865.

Gazette des Hôpitaux,—Nos. 61 to 72. Paris, 1865.

Gazette Hebdomadaire de Médecine, etc.,— Nos. 21 to 25. Paris, 1865.

Gazette Médicale de Paris,—Nos. 21 to 24, 1865.

Medical Times and Gazette,—June 3 to 24, 1865.

Medizinische Jahrbücher,—No. 6, 1864; and Nos. 1 and 2, 1865. Vienna.

Revue de Thérapeutique Medico-Chirurgicale,—June 1, 15. Paris, 1865.

Virchow's Archiv, Nos. 4 and 5. Berlin, 1865.

Wochenblatt der Zeitschrift der Aerzte,— Nos. 48 to 52, 1864; and Nos. 1 to 15, 1865. Vienna.

# Part First.

=====

## ORIGINAL COMMUNICATIONS.

ARTICLE I.—*Report of Clinical Cases, under the Care of* Professor SPENCE, *in the Surgical Wards of the Royal Infirmary, from July* 1863 *to October* 1864. By WILLIAM RUTHERFORD, M.D., late Resident Surgeon.[1]

THE cases given in this Report are the more important ones which occurred during the period.

### WOUNDS OF JOINTS.

The following five cases of the above injury, all of which occurred during last summer, will be found interesting, both on account of the nature of the injuries, and the treatment adopted.

CASE 1.—P. C., æt. 21, a strong, healthy young man, was brought to the Infirmary from a neighbouring part of the country on the evening of May 28. In the morning he had been violently ejected from a colliery waggon, against some wooden stakes, the sharp point of one of which penetrated, on the inner side of the patella, into the left knee-joint, producing a slightly lacerated wound, which easily admitted the forefinger into the joint. The knee was acutely painful, even when at rest, and its temperature had risen very considerably. Hæmorrhage from the wound had been but slight. There was also a lacerated scalp wound of a couple of inches in length, unaccompanied, however, by headache or other nervous symptom. He complained of great pain over the spine, in the interscapular region, but no fracture of the vertebræ or ribs could be detected. He had great thirst, but the skin generally was cool, and the pulse only 68. A flannel bandage was rolled round the chest; the wounds of the scalp and knee were carefully cleaned, and their edges brought together by silver sutures, and the limb was placed on an inclined plane. In similar cases, Mr Spence had hitherto employed irrigation to keep the joint cool, but had been so dissatisfied, both with the difficulties which attended

---

[1] During the period, Dr Cleghorn, Dr Rhind, and myself, successively acted as resident surgeons. The cases are abbreviated from the regular reports of the Wards, which were carefully kept by Messrs Burt, John Rhind, M'Donald, Stone, Muir, Paterson, and Anderson.

its proper application, and the unsatisfactory results which had almost always followed its use, that he determined to try ice. A large gutta-percha bag, containing ice, was placed upon the knee, the wound of which was covered with a small piece of dry lint, which, again, was covered with a large sheet of gutta-percha, so as to render it impossible for the wound to become wetted by the accidental escape of water from the bag. Forty minims of the solution of muriate of morphia were given to soothe uneasiness and procure sleep. During the night he slept soundly, and next morning he had no headache, and the knee was painless.

31st.—Stitches removed from both knee and scalp; wound of the former healed by first intention, that of the latter had suppurated. Pulse 90.

1st June.—Pulse 100; severe headache. No opiate given since night of admission. Hair to be cut short, and cold applied to the head; and a medium dose of castor oil given. 2d.—Had a slight rigor; tongue furred; pulse 102. Headache rather abated; knee slightly swollen, but quite cool. The ice was discontinued for a few hours, and cold water substituted, but as the knee soon became painful, and rapidly rose in temperature, ice was again had recourse to, with the consequent disappearance of these untoward symptoms. In the evening, erythema showed itself around the scalp wound. 3d.—Erythema extending. Pulse 104, weak. Four ounces of brandy to be given daily. Fifteen drops of tinct. ferri sesquichlor. every three hours. Saline drinks. Notwithstanding the presence of erythema, cold water was kept applied to the head, as it greatly relieved the headache. 4th.—Delirium. Erythema not extending. Pulse 110. To have six ounces of brandy instead of four. 5th.—Slept well during the night. Delirium gone. Pulse 96. Erythema disappearing. 8th.—Pulse 82, of much improved strength. Erythema quite gone. Brandy diminished to three ounces. Ice removed from the knee, and muslin wetted with cold water substituted, as it was found sufficient to check the tendency to increase of temperature. Cold to the head discontinued. 15th.—Scalp wound healed, and pain in the back disappeared. Limb removed from splint. Beer substituted for brandy. Convalescence then rapidly became established, and he was sent to the Convalescent House on 28th June, being then able to walk with the aid of a crutch. He presented himself at the clinique on 28th July; his health was completely recovered, and there was not the slightest stiffness of the joint.

The above was not by any means a favourable case for testing any method of treatment. The patient's constitution certainly was excellent, and the treatment was begun before the joint had inflamed. Still, however, the head injury, and the consequent erythema and delirium, were most serious complications, both from the constitutional irritation which they induced, and the great difficulty experienced in keeping the limb at rest during the delirium.

The case was a typical one for the employment of bleeding, purging, and other antiphlogistic treatment, enjoined at an early period in the treatment of such injuries by many surgeons. Instead of these, however, strong beef-tea, and other nutritious articles of diet, were given from the first.

The following case, which was also one of wound of the knee-joint, forms, in some respects, a striking contrast to the above :—

2.—A. E., æt. 36, admitted 7th July, fell upon the broken edge of an earthenware basin,—the sharp edge entered the left knee-joint, dividing the tendon of the quadriceps extensor, and grazing the upper border of the patella. He was shortly afterwards conveyed to the Infirmary. The wound was sharply incised, and admitted two fingers into the joint. Bleeding—which had been considerable immediately after the accident—had ceased. The edges were brought together by silver sutures ; the limb placed on an inclined plane, and ice applied to the joint in a manner similar to that adopted in the former case. Mr Spence, however, expressed a very doubtful prognosis, for, in addition to the considerable extent of the wound, the patient was syphilitic, and a primary union was, on that account, very doubtful. 8th July.—Pulse 80. Joint painless. 9th.—Pulse 100, soft. Tongue furred. Bowels moved by enema. Joint somewhat swollen, but painless. 10th.— Redness and tension of the wound. A few stitches removed. 11th. —Tension increased ; all stitches removed, and narrow strips of plaster substituted. No union had occurred. During the night the joint had been painful. After the removal of the stitches a little bleeding from the wound took place, which was encouraged by the removal of the ice for an hour. 13th.—Erythema appeared around the edges of the wound. Suppuration within the joint. Ice discontinued, and a light linseed-meal poultice substituted. Pulse 104, weak. To have have four ounces of brandy daily, and fifteen drops tinct. ferri sesquichlor. every three hours. 14th.— Erythema spread over nearly the whole thigh. Joint exceedingly painful. Wound enlarged to permit of free discharge, and a counter-opening made on the inner side of the joint. Opiates given frequently. 15th.—Had a severe rigor. Pulse 120. An abscess, which had insidiously formed on the inner side of the thigh, was opened. To have three ounces of sherry in addition to the brandy. 16th.—Pleurisy on left side. Bronchitis. 17th.—Pain in hepatic region. Numerous abscesses formed in the thigh and upper part of the leg, and he grew gradually weaker and died on 30th July. The autopsy revealed the usual results of pyæmia in the liver and lungs. The suppuration in the thigh and leg had occurred within the sheaths of the muscles. The synovial membrane of the joint was covered with weak, flabby granulations, and the cartilage eroded. The question of amputation was considered on the 14th, when it became evident that suppuration was about to extend up the thigh ; but the severe erythema, the patient's general

state, and the excessive mortality after amputation for inflammation of a joint, contra-indicated any active interference.

3.—D. W., æt. 23, a strong, healthy man, received, a couple of hours before admission on 31st May, an incised wound on the inner side of the ankle, opening into the calcaneo-astragaloid joint. The wound, about two inches in extent, was produced by a blow from an adze. There was slight hæmorrhage, easily arrested by cold. Silver sutures; application of ice; union by first intention. Pain, which was severe before, entirely ceased on the application of the ice, and never returned excepting on the fifth day, when the ice was removed for a short time. The severity of the pain and alarming rise of temperature necessitated, however, its re-application for four days longer. He was dismissed on 28th June completely cured.

4.—J. B., æt. 23, admitted on 23d June, shortly after his having received a blow from an adze, over the anterior border of the inner malleolus of the right ankle. There was a semi-lacerated wound, an inch and a half in length; anteriorly it opened into the ankle-joint, and posteriorly it extended deeply into the osseous substance of the malleolus. The hæmorrhage, which was pretty copious, was suppressed by cold; the wound was cleared, but not entirely, from particles of dust and sand; sutures introduced; limb laid on a splint, and ice applied. Permanent union by first intention took place in the front of the wound, which opened into the joint; and superficial union of the lips of the wound over the bone on 1st July; however, a small abscess began to form in the angle of the wound over the bone. Tepid-water dressing was substituted for the ice. The progress of the case was tedious, owing to the formation of small sinuses above the ankle. No inflammation, however, ever appeared in the joint. Dismissed cured on 12th August.

It is probable that, had the occurrence of inflammation not been long delayed by the low temperature employed, the wound of the joint not having firmly closed, and the joint not having had sufficient time to recover from the irritation of the wound, it would have participated in the inflammation, and a result, much less satisfactory, have ensued.

5.—Mary R., æt. 50, admitted 25th August 1864. On the preceding day her right ankle was caught in the shears of a reaping machine, which divided all the tendons on the front of the ankle, together with the anterior tibial artery and nerve, and cut into the tibia, just above its articular surface, to the depth of nearly half an inch. There had been but slight hæmorrhage, as, owing to the bluntness of the blades, the artery had been torn. A medical gentleman secured the limb by side splints, and ordered cold-water cloths to be constantly applied; the wound was, however, allowed to remain open. On arriving next day at the Infirmary, after having travelled many miles, she was suffering from severe prostra-

tion, and acute pain in the wound and joint. The latter did not appear to have been opened, in the first instance ; but on the ankle being extended during the examination of the wound, a small opening was made in the capsule of the joint. Fortunately, no symptoms of irritation had appeared in the wound, so that sutures were introduced ; and as the temperature of the dorsum of the foot was already lowered, by deficient circulation, it was deemed imprudent to depress it further. A small bag of ice was, therefore, placed on either side of the ankle, and the limb secured on a wire splint, with the foot raised. As her strength had suffered severely, a daily allowance of three ounces of wine was ordered. Shortly after the application of the ice, the pain entirely disappeared, and she slept soundly during the night without the aid of an opiate. 26th.— Pulse 90. Tongue furred. Bowels moved with the aid of an enema. Ordered fifteen drops tinct. ferri sesquichlor. thrice daily. 29th.—All stitches removed, and tepid-water dressing substituted for the ice, as a thin slough was beginning to form on the edges of the wound. Primary union had, however, occurred to a slight extent. 30th.—Joint a little painful and somewhat swollen. 3d August.—A small abscess on the inner side of the joint. The further progress of the case was tedious. The slough separated, and a weak, granulating sore remained, which healed so slowly that she was unable to leave the hospital before 28th October. Then, however, she was able to walk with the aid of a crutch. The joint was perfectly movable.

*Remarks.*—In treating a wounded joint, two objects are aimed at,—to obtain primary union, and to prevent inflammation of the joint. For their accomplishment, local and general measures have been proposed, the former of which are the more important, and include rest, position, and cold.

The employment of ice as the cooling agent is not a novel practice, but it is not so generally adopted as its merits appear to warrant.

It is much to be preferred to irrigation with cold water, both, because a *dry* cold is obtained, and a lower temperature produced, and because it can be much more easily and efficiently applied. With irrigation, the wound and surrounding skin are constantly kept in a sodden condition, so that primary union is frequently prevented, and a painful state of the skin often produced. Moreover, the patient is liable to catch cold, from his clothes and the supports of the limb being constantly kept soaking with wet ; for, in the case of the knee and ankle, at least, it is almost impossible to confine the water to the limb, and prevent its wetting everything. With ice, everything can be kept perfectly dry, by enclosing the ice in gutta-percha bags, and by adopting the additional precaution of covering the wound with a large sheet of the same material, to prevent any accidental escape of water from reaching it. Were it even possible, however, to use irrigation without wetting the wound

and surrounding skin, ice would nevertheless be preferable, on account of the lower temperature produced by it. When Cases 1 and 2 were under treatment, the supply of ice several times became exhausted, and during the intervals the joints had to be kept cool by irrigation, which was never adequate to prevent a rise in temperature, and the joints from becoming painful; the re-application of the ice always depressed the former and removed the latter.

The semi-anæsthesia produced by ice is generally sufficient to remove the pain until suppuration sets in; afterwards, its influence in that respect is not so decided. In order, however, to keep the patient at perfect ease, the bag must be promptly refilled as soon as the ice has melted; for, unless an equable low temperature be prescribed, pain is not completely removed; in some cases it is even exaggerated, and possibly the non-attention to this may, to some extent, explain the difficulty with which ice is borne by some patients of irritable temperament. The low temperature, so far from being hostile to the nutrient changes requisite for primary union, is, through its power of preventing inflammation, one of the best aids to primary union that can be adopted. During the late campaign in Schleswig-Holstein, the surgeons to the allied armies applied ice to all wounded joints, and to nearly every stump after amputation; and the general conclusion arrived at was, that where it was employed, primary union was more frequent and more extensive, and that the ensuing suppuration was always less. These results ensued whether the ice was supplied only until the commencement of inflammation, as practised by Langenbeck, or when it was kept applied until the wound had almost entirely healed by the second intention. The practice of continuing the application of cold after inflammation has set in has generally been considered a dangerous one by the surgeons of this country; but, contrary to what might have been anticipated, the cases treated on the principles of Esmarck succeeded almost as well as those where Langenbeck's practice was adopted.

Joints wounded by gunshot are, of course, much less amenable to conservative treatment than where the wound is simply incised. In the former case, to be sure, the wound may be of such slight extent, as regards the joint, that they are not very formidable. There occurred, however, in the above-mentioned campaign, a gunshot wound of the knee-joint, of a very serious nature, which was successfully treated by ice. The patient, a young man, received a shot on the outer side of the thigh; the ball passed obliquely through the outer condyle of the femur, down through the knee, and out through the inner tuberosity of the tibia. Ice was carefully applied for many weeks until the wound had nearly healed, and complete recovery *without* anchylosis followed. Langenbeck was of opinion, that the synovial membranes having, in all probability, escaped injury, had favoured the happy result; but he at the same

time attributed the success very largely to the careful application of ice from the commencement.   That ice, however, is not sufficient to prevent inflammation, even in those cases where the wound is incised and clean, was evident from Case 2.   There, however, as previously stated, the patient's constitution was largely to blame. It would, however, be worth while investigating, whether or not a lower temperature than that produced by ice might not advantageously be employed in some cases where the tendency to inflammation is unusually decided, either from constitutional peculiarity or the nature of the wound.   We do not as yet know the lowest temperature at which a part of the body in a normal state may be kept by *dry* cold, without serious interference with its nutrition ; still less do we know to what depth of temperature a wounded part may be safely lowered.   Should it be found that a lower temperature than that produced by ice can be borne, its production could be effected without much difficulty by the employment of various frigorific mixtures, graduated according to the temperature required ; and they could without difficulty be prevented from coming in contact with the wound, by enclosing them in thick gutta-percha bags, and by laying a sufficiently large sheet of the same material between the bag and the wounded part.   In applying ice to a wound, common bladders ought never to be used if gutta-percha can be obtained, as they very soon allow the water to ooze through, and, notwithstanding the low temperature, soon undergo decomposition ; moreover, gutta-percha is cheaper, and bags can be made very easily, by simply wetting the margins of the pieces to be joined together with chloroform, and holding them in apposition until they dry.   In the removal of loose cartilages from the knee-joint, by subcutaneous or direct free incision, were ice constantly applied to the part after the operation until all tendency to inflammation had ceased, the serious consequences which have so frequently followed these operations hitherto would in all probability be frequently averted ; possibly also its employment might often be found serviceable after subcutaneous section of tendons and several other operations.

## INJURIES OF THE HEAD AND SPINE.

CASE 1.—Alex. L., æt. 26, admitted on account of fungus cerebri, had sustained compound fracture of the frontal bone, with severe concussion, ten weeks previously.   The medical gentleman who attended him removed the broken fragments, had the head shaved and ice applied.   On the second day, as the state of coma continued, he was bled largely and purged ; notwithstanding which, and in spite of the continued application of the ice, he did not recover consciousness until the seventh day after the accident.   A portion of the dura mater sloughed, permitting the formation of fungus cerebri, a considerable portion of which also sloughed off.   The treatment adopted in the hospital consisted of astringent applica-

tions, such as alum and sulphate of copper, with pressure by plaster and bandages. He went to the country two months after admission, the wound having almost entirely cicatrized, and the fungus disappeared.

2. Mary K., æt. 17, fell while getting out of a cab in motion; the wheel passed over her head, and produced an extensive lacerated scalp wound. Silver sutures introduced; hair cut short; ice applied. On the fifth day the pulse rose to 98, and she was consequently ordered 25 min. of antimonial wine and 2 drs. of liq. ammon. acet. every three hours. Next day the pulse fell to 80, and the antimonial wine, etc., were discontinued. Recovery.

3. Francis M., æt. 42. Three lacerated scalp wounds of considerable extent, produced by blows from a hatchet. Erysipelas. Recovery.

4. Mary M., æt. 64. Wound of eyebrow, from falling down stairs. Deep-seated suppuration in orbit. Recovery.

5. Patrick M., æt. 42. Punctured wound of eyebrow. Suppuration in orbit. Erysipelas. Meningitis. Death.

6. C. F., æt. 39. Semi-lacerated wound, from blow of poker, over left parietal bone. Recovery.

7. Eliza C., æt. 38, a fortnight before admission, fell down stairs and struck her head violently against the wall; was unconscious for half an hour afterwards; had suffered since the accident from frequent sickness, pain passing round the head, and occasional rigors. A tolerably distinct depression was felt under the scalp, over the coronal suture. Pulse 104, weak; tongue furred; surface covered with cold perspiration. Hot fomentations were applied to the head to relieve the pain, and she was kept quiet in bed for a fortnight. Recovery.

8. Margt. L., æt. 23. Two semi-lacerated wounds of the scalp, produced by falling down stairs. Erysipelas. Bronchitis. Recovery.

9. Catherine G., æt. 12, injured the back of her head by falling backwards down a stair. Chorea ensued, but no depression or fracture could be detected, and, as she was otherwise well, expectant treatment was adopted. She rapidly recovered; and left the hospital three weeks after the accident, completely recovered.

10. Henry H., æt. 53, fell backwards from a hay-stack, a height of fifteen feet, upon the ground, and injured his spine. When he endeavoured to move, he complained of acute pain in the lumbar spine. No fracture of the vertebræ could be detected. During the first day after the accident, sensibility and motion of the lower limbs remained unimpaired; but on the second morning, he discovered that he was unable to move them, and that he could not micturate. On the second day, six ounces of blood were removed by cupping in the lumbar region, after which ice was applied to the spine; a purgative was administered, and he was ordered to take ℥ss. liq. ergoti, thrice daily. No improvement followed. Fever-

ishness, accompanied by excessive thirst, supervened; the pulse rose to 120; the limbs became insensible on the fourth day. He grew gradually weaker, and died a month after the accident. An autopsy could not be obtained.

Mr Spence's opinions upon head injuries have been so fully given in former reports that it is unnecessary to repeat them in this.

## AMPUTATIONS.

### PRIMARY AMPUTATIONS.

#### *Shoulder-joint.*

1. James M., æt. 23, on the day previous to admission had had his right arm crushed by a thrashing machine. Soon after the accident, his medical attendant amputated below the tuberosities, without the aid of chloroform. On admission, Mr Spence re-amputated at the shoulder-joint by a long external and internal flap, as the flaps in the previous operation had been made much too short. Recovery.

#### *Arm.*

2. Mary C., æt. 60. Compound dislocation of elbow and wrist. Amputation at middle of arm by equal flaps. Pyæmia. Death.

#### *Fore-arm.*

3. Alex. D., æt. 11. Hand and lower part of fore-arm crushed by printing-machine. Amputation by equal flaps below elbow. Recovery.

#### *Hip-joint.*

4. E. B., æt. 6. Half an hour previous to admission the wheel of a heavily laden cart passed over her right knee and left thigh, producing a severe lacerated wound of the former and compound comminuted fracture of the latter, with extensive disorganization of the textures on the front of the bone, to within three or four inches of Poupart's ligament. There had been considerable hæmorrhage after the accident, which had ceased, however, before reaching the Infirmary. Brandy and ammonia were freely given to rally her from the shock; and when she had sufficiently recovered, Mr Spence amputated at the hip-joint by a long posterior and short anterior flap. He experienced considerable difficulty in disarticulating, owing to the shortness of the upper fragment of the femur rendering extension and rotation very difficult; there was but little hæmorrhage during the operation, and none secondarily. Sloughing of a portion of the short flap occurred; and, notwithstanding the liberal administration of stimulants and nutritious soups, she grew gradually weaker, and sank on the fifteenth day after the operation.

#### *Thigh.*

5. James B., æt. 9. A cart-wheel had passed over the right foot

and ankle, producing compound comminuted fracture of the tibia, with fracture and partial dislocation of the astragalus. He was brought to the Infirmary at night, shortly after the accident; as the temperature of the foot was good, and the textures did not seem to be very extensively disorganized, the artragalus was replaced, and the limb laid on a pillow, and lightly covered with cotton wadding. Next day gangrene appeared, and Mr Spence amputated below the knee; but finding that, even there, the textures had also suffered from the injury, he had recourse to amputation at the lower third of the thigh by long anterior flap. Necrosis of some small fragments of the lower extremity of the bone rendered the progress of the case tedious. Recovery.

### Leg.

6. John M'D., æt. 22. Railway waggon passed over ankle, producing such complete disorganization of the parts, that amputation was performed below the knee immediately after admission. Recovery.

7. Robert F., æt. 24. Foot and ankle crushed by a cart-wheel. Amputation below knee by modified circular method. Recovery.

8. A. M., boy, æt. 5. Foot and ankle severely lacerated by a weaving machine; severe hæmorrhage. Amputation at middle of leg. Sank on the second day after the operation.

9. Margaret H., æt. 60. Compound dislocation of ankle, fracture of inner malleolus, with extensive laceration of soft parts above the ankle. Amputation below knee by long posterior flap. Erysipelas; pyæmia. Death.

### Foot.

10. Robert K., æt. 21. Four outer toes crushed by railway waggon. Toes and portions of the corresponding metatarsal bones amputated. Recovery.

### SECONDARY AMPUTATION FOR INJURY.

### Fore-arm.

11. Helen T., æt. 24. Had the back of her hand punctured by a table-fork seven weeks before admission. Extensive sinuses had formed, which palliative measures failed to cure. Amputation at middle of fore-arm. Recovery.

### Thigh.

12. Jane G., æt. 20. At the age of ten years had had her right leg and foot extensively burned. The development of the limb was in consequence arrested, and ulcers frequently formed in the cicatrix. Amputation at upper third of thigh by equal flaps; the long anterior flap could not be adopted, owing to the extension of the cicatrix on the anterior aspect of the thigh. Recovery.

SECONDARY AMPUTATION FOR DISEASE.

*Arm.*

13. E. B., æt. 23.  Synovial degeneration in wrist and elbow. Amputation at lower third of arm.  Recovery.

14. Mary M'D., æt. 18.  Excision of the elbow for scrofulous disease had been performed a year previously by Mr Spence.  Four months previous to her re-admission the same elbow became swollen and covered with small ulcers.  Amputation at middle of arm by equal flaps.  Recovery.

*Fore-arm.*

15 E. B., male, æt. 23.  Caries of carpus and metacarpus of two years' duration.  Amputation at middle of fore-arm.  Pyæmia. Death.

*Wrist.*

16. A. C., male, æt. 17.  Nine years before, Mr Spence had removed the fore-finger for caries, which had now attacked the carpus.  Amputation at wrist by double flap.  Recovery.

*Thigh.*

17. W. H., male, æt 17.  Ulceration of cartilages and synovial degeneration of knee; had been several times in the hospital, and palliative measures had been used for a lengthened period without avail.  Amputation by long anterior flap at lower third of thigh. Pyæmia.  Death.

18. Mary L., æt. 12.  Scrofulous disease of knee of eight years' standing.  Amputation at lower third of thigh, long anterior flap. Recovery.

19. Jane D., æt. 40.  Scrofulous disease of knee of five years' duration.  Amputation at lower third of thigh, long anterior flap. Recovery.

20. S. R., female, æt. 16.  Scrofulous disease of knee.  Amputation at lower third of thigh, long anterior flap.  Recovery.

21. Janet M'D., æt. 46.  Scrofulous disease of knee of four years' duration.  Amputation at lower third of thigh, long anterior flap. Pyæmia.  Death.

22. Peter K., æt. 10.  Ulceration of cartilages and synovial degeneration of knee.  Amputation at lower third of thigh, long anterior flap.  Recovery.

23. Jane M'D., æt. 22.  Scrofulous disease of knee, suppuration within the joint.  Amputation at lower third of thigh, long anterior flap.  Recovery.

24. Wm. R., æt. 16, an extremely delicate youth, had had scrofulous disease of knee for three years.  On admission, palliative measures were tried; the limb was laid on a wire splint, and lead and opium fomentations were constantly applied; suppuration within the joint occurred, and amputation was had recourse to at the lower

third of the thigh by long anterior flap. Died from exhaustion eight days after the operation.

## Leg.

25. Eliza L., æt. 37. Large constitutional sore on lower part of leg, of 2½ years' duration, which had resisted ordinary curative measures. Amputation below knee, long posterior flap. Recovery.

26. E. M., female, æt. 48. Talipes varus of left foot, with a large ulcer above the ankle, which had resisted all palliative remedies. She had taken opium for fifteen years. At her own urgent request the limb was amputated below the knee by long posterior flap. Large opiates required to be given to procure even broken sleep. Secondary hæmorrhage occurred, and the wound had to be laid open to arrest it. The whole surface of the wound sloughed. Died on the seventh day after the operation.

## Ankle.

27. George M., æt. 16. Scrofulous disease of ankle. Syme's amputation. Recovery.

28. Colin M., æt. 14. Serpiginous ulceration of foot and toes of two years' duration. Syme's amputation. Recovery.

29. John C., æt. 12. Caries of tarsus. Amputation by large internal flap. Recovery. This modification of the usual operation was rendered necessary by ulceration on the outer aspect of the heel.

30. Janet K., æt. 16. Caries of tarsus. Syme's amputation. Pyæmia. Death.

*Remarks.*—The results of these amputations are not so favourable as those of several former years,—a circumstance which must be attributed to the unusual prevalence of pyæmia, which was not, however, confined to Mr Spence's wards, but prevailed throughout the whole Surgical Hospital. Of the ten deaths, six were due to pyæmia; two of these were cases of primary amputation, and the remaining four, amputations for disease. Recovery was hardly to be expected in the case of the little girl (4.) who was the subject of amputation at the hip-joint. The loss of blood, severe shock, and the severe injury to the other limb, greatly lessened her chance of recovery; and, moreover, her constitution was one of the most delicate. Strange to say, two years before the accident, she had had scrofulous disease of the knee, of the same limb which was amputated; and, but for the opposition of her mother, the limb would then have been removed by a surgeon, who deemed the disease not amenable to milder treatment. Under Mr Spence's care she had quite recovered, and was able to run about when the unfortunate accident occurred. In Case 8, death was due to sinking on the day following the operation : the excessive hæmorrhage which followed the accident being the chief cause. In Case 24, palliative measures were fairly tried before having recourse to the operation,

which was performed to afford him the only remaining chance, and that a very slender one, considering the degree of weakness to which the lingering disease had reduced him. In Case 26, the operation was very reluctantly undertaken, because of the unhealthy state of the patient's constitution, due both to her having had syphilis and to her habit of opium-eating; it was, however, almost an operation of necessity on account of the incurable ulcer, for which she had been previously treated in Mr Spence's wards; and as the patient was most anxious for the operation, and willing to incur its risks, in order to get rid of her deformity and disease, the chance was afforded her. As the stump was very carefully opened on the same day as the operation, on account of reactionary hæmorrhage, the sloughing of the wound which afterwards occurred was probably entirely due to her depraved state of health. The successful result in the case of amputation at the shoulder-joint was remarkable, considering that the patient had sustained a severe injury, two amputations so near the trunk, and had travelled a distance of twenty miles, all within thirty-six hours. Had the patient's constitution not been most robust, the issue would probably have been different.

Were the statistics which these operations afford taken as illustrating the general results of Mr Spence's amputations, the same error as is always committed· by those who draw up statistics from a short period, more especially if it be one where an exceptional state of things exists, would be fallen into. This serious mistake was committed by Peacock, in the last published statistics of operations performed in the Edinburgh Royal Infirmary. These statistics, always quoted when the results of operations in Edinburgh are referred to, are calculated seriously to mislead, because they are almost entirely limited to a period when there was quite an unusual number of primary operations for accidents sustained in extensive railway works which were then going on. There is reason to believe that were the statistics of the hospital for a number of years back published, they would be found to compare very favourably with those alluded to. I am not at liberty to refer to any operations excepting those performed by Mr Spence. It may be interesting, however, to take a single operation and compare the results obtained by him with those given by Peacock. According to Peacock, the mortality following amputation of the thigh, primary and secondary taken together, is 1 in 3. In the cases of primary amputation of the thigh performed by Mr Spence since his connexion with the Infirmary, the average of deaths has been about 1 in 2; while out of 52 cases of amputation for disease of the knee-joint only 8 have died, or 1 in $4\frac{1}{2}$. The statistics of many operations, however, will require to be given in a much more complete form than they have usually been before they can be thoroughly reliable as a means of estimating the value of the operation or the practice of the surgeon. In amputation of the thigh,

for example, cases of primary and of secondary amputation, with rates of mortality so widely different; amputations in the lower, middle, and upper thirds, with a different rate of mortality in each situation; amputation for various diseases, no two of which have the same rate; amputations performed at different ages; and amputations, followed by various methods of after-treatment; all these widely different varieties, each of which may preponderate, are thrown together, and the resulting statistics deprived of much of the value they might otherwise possess, were a more minute and special system adopted.

As the treatment of a case previous to and after operation is a subject upon which the greatest difference of opinion prevails, a short account may be given of Mr Spence's practice in this particular. Previous to the operation, an alterative or mild purge is given if necessary, in order that the digestive system may be placed in as favourable a condition as possible. On the morning of the day of the operation, the patient receives nothing but a cup of tea, some dry toast, and a little brandy or wine, in order to lessen the liability to sickness under chloroform; a precaution, which, if adopted in several continental hospitals, would save the operator from frequent inconvenience, and the onlookers from disagreeable sights. Should the patient evince much nervousness, he is put under chloroform in bed, and afterwards taken to the operating theatre, and not allowed to awake unless under exceptional circumstances, until he has left it. Twenty or thirty minims of sol. mur. morph. are usually given immediately after his being placed in bed, but if there is any tendency to sickness, the liq. opii sedativ. is preferred. During the progress of the case, opiates are given at bed-time when required. At first the diet is simple but very strengthening, consisting of strong beef-tea and farinaceous food, by-and-by fish, and afterwards, a chop or steak is added, as the appetite improves. Although Mr Spence gives stimulants largely, no such thing as a routine practice, in that respect, is pursued; in many cases none are given at all, in others, only malt liquors, while in some, brandy or wine are given from the beginning: the state of the pulse and the appearance of the wound being the chief indications attended to in each case. Tonics are given if required; they usually consist of quinine or gentian, with hydrochloric acid. As regards the local treatment: during the operation,—ligatures, torsion, and cold water are the hæmostatic agents employed, and the wound is closed immediately on the cessation of the oozing by silver sutures, no strips of plaster are employed at this stage, as is the practice of many surgeons, but a simple strip of dry lint is laid along the wound, and a roller applied if there be any tendency to oozing; it is removed, however, soon after the patient is placed in bed, in order that the wound may be kept as cool as possible. The sponges used at the operation are always previously soaked for half an hour in dilute hydrochloric acid, in order that they may be thoroughly

cleansed from decaying organic particles.   When tension occurs in the wound, sutures are of course removed as may be deemed necessary, and short strips of adhesive plaster substituted, which is always heated by being dipped in very hot water, which is always more easily obtained than a hot iron, makes the plaster lie much smoother, and does not alter the composition and diminish the adhesiveness of the plaster, as the hot iron is apt to do.   For the dry lint dressing, lint dipped in hot water and covered by gutta-percha, is substituted as inflammation and suppuration set in.   If there is the slightest tendency to fetor, the wound is washed with dilute Condy's solution (permanganate of potash) ; the two stimulating lotions employed are zinc lotion, and dilute solution of chlorinated soda, —the latter is preferred when fetor is present, and is, both on account of its stimulating and disinfectant properties, one of the best lotions that can be used.   Nitrate of silver or sulphate of copper are used for exuberant granulations, or when the surface is grey or diptheritic. If sloughing occur, a light charcoal poultice is constantly applied, followed by chlorinated soda lotion, when the greater part of the slough has separated.   Erythema is treated by hot fomentations, and the internal exhibition of tinct. ferri sesquichlor.   Erysipelas is treated similarly ; but dusting with flour is sometimes substituted for the hot fomentations.   If pyæmia attack the patient, quinine and stimulants are chiefly employed.

Very different from the above simple and successful treatment is that adopted by most continental surgeons : instead of keeping the wound cool, more especially immediately after the operation, they cover it with all sorts of heating dressings, so that primary union is extremely rare, and gangrene, erysipelas, and pyæmia, the most common of accidents.   By Langenbeck and Willms, in Berlin, however, a treatment similar to that above given is adopted, with the best results ; and Maisonneuve, in Paris, employs a treatment based on similar principles ;—glycerine and water are his most frequent applications ; aromatic wine if the wound be indolent, and very dilute phenic acid when a disinfectant is required.   He properly avoids the use of sponges for dressing ; but, on the other hand, he almost never passes a stream of simple water over any wound, however fetid, so that, although the treatment is a great improvement upon that pursued by most other French surgeons, it is still inferior to that employed in this country.   Nélaton pours alcohol over the cut surface immediately after the operation, with a view of forming a coating of coagulated albumen, and to disinfect the wound ; primary union is consequently rare in these cases, and the frequence of dangerous complications but little affected.   The prominent feature of French surgery is, the number of most ingenious devices, whereby a clean cut surface may be avoided, on account of the supposed dangers which it entails.   A proper hygienic regime is generally overlooked in the hospitals, with the disastrous consequences which every one knows.   That pyæmia is largely owing to non-attention to

hygienic measures, is an opinion which obtains but little credence; only the other day, Maisonneuve delivered a lecture in which he said, that the idea of pyæmia being in any way connected with peculiar atmospheric changes, or insufficient ventilation, is quite a mistake, and that, on the contrary, it is altogether dependent upon the entrance of pus into unclosed veins. Whatever be the proximate or immediate causes of pyæmia, one thing seems quite certain, viz., that in proportion as the wards of an hospital have been kept pure and clean, with an abundant supply of fresh air; and in proportion as wounds have been dressed lightly, simply, and cleanly, the frequency of pyæmia, erysipelas, and gangrene, has greatly diminished.

### EXCISION OF JOINTS.
#### *Shoulder.*

1. Jane C., æt. 20. Caries of head of humerus. Excision by linear incision. Recovery.

#### *Elbow.*

2. M. C., female, æt. 22. Synovial degeneration. Excision by linear method. Recovery.

3. E. S., female, æt. 19. Synovial degeneration. Excision by linear method. Recovery.

4. David S., æt. 15. Synovial degeneration of fifteen months' duration. Incipient phthisis. Excision by linear method. Pleurisy Death.

5. John H., æt. 30. Six weeks before the operation, he had sustained compound fracture of the radius and ulna, near the elbow-joint. Mr Spence put up the fracture in rectangular pasteboard splints; the patient insisted upon going to the country; he returned with a number of suppurating sinuses leading into the joint. Excision by linear method. Secondary abscesses. Recovery.

6. Wm. A., æt. 56, sustained a compound comminuted fracture of the condyles of the humerus, two years previously. A surgeon had removed some fragments of bone, but did not excise the joint: almost complete anchylosis followed. A week previous to admission he fell upon the same elbow, inflammation and suppuration resulted, and the consequent formation of two sinuses leading to the joint. When the inflammatory action had somewhat subsided, the joint was excised by the linear method. Pyæmia. Death.

#### *Wrist.*

7. Wm. F., æt. 30. Rheumatic arthritis of wrist, of three years' standing. Joint excised by two lateral incisions. The wound progressed very slowly; necrosis of the lower end of the radius occurred; and, four months after the operation, severe hæmorrhage occurred from ulceration of the radial artery, in consequence of which amputation was performed below the elbow by equal flaps. Recovery.

On dissection, the extremities of the bones were found to be quite bare.  The whole thickness of the lower fourth of the radius had necrosed; the bones seemed to be generally in an unhealthy state, for both the radius and ulna at the seat of amputation were found to be unusually soft and spongy.

### Knee.

8. Jane M., æt. 20.  Ulceration of cartilages, which the actual cautery and other remedial measures had failed to check.  Excision by semilunar incision.  Patella allowed to remain, and immediately after the operation the limb was placed on Ferguson's splint; and, three months afterwards, it was laid between two long bags filled with sand, and at the end of another month the limb was put up in a starch bandage, with lateral pasteboard splints, and she was allowed to walk about with the aid of a crutch.  The slow recovery was due to necrosis of some small fragments of the lower end of the femur.  A satisfactory result was, however, obtained; the joint being slightly movable, but sufficiently rigid to afford an excellent support.

*Remarks.*—In excision of the elbow, Mr Spence generally adopts Langenbeck's incision.  With it the operation is a little more difficult than with the H incision; but the difference is of little moment.  The wound can be much more easily dressed, and is not subject to the protracted healing which follows the non-union of the transverse part of the H incision by first intention.  Further, passive movement can be begun much sooner with the former than with the latter; the only objection to which it is open, is the tendency to the accumulation of discharge within the inner part of the wound, which, however, may be easily remedied by piercing the inner flap, and drawing the ligatures through the counter-opening.  In doing so, the position of the ulnar nerve must be regarded.  In Case 4, phthisis existed in the first stage, previous to the operation, and some surgeons would probably, on that account, have declined to operate; but, although the result was unfortunate in this case, it has been the experience of Mr Spence, and also that of Langenbeck, that very frequently, in such cases, the phthisis, if not far advanced, so far from becoming active after the operation, undergoes improvement, and that, not only after the wound is healed, but during the healing process.

Although the case of excision of the wrist terminated unfavourably, Mr Spence intends repeating the operation on the first suitable case, as the result was probably altogether due to peculiarity in the patient's constitutional state.

Langenbeck, who, perhaps of all surgeons, has had the greatest success in excision of joints, always performs the operation subperiosteally; that is, before removing the bone he makes a longitudinal incision through the periosteum, and carefully separates it

from the bone to be removed; the procedure is difficult, more especially in the case of the wrist, but the results are excellent : of the many cases in which Langenbeck has excised the elbow subperiosteally, in only one has necrosis taken place, and it was a case where the joint was excised for compound comminuted fracture by gunshot, of the lower extremity of the humerus. After all excisions Langenbeck places the limbs in plaster of Paris immediately after the operation, and they are kept at rest in it until the wound has nearly healed. In excision of the knee he always adopts a linear incision, with a central arch round the inner border of the patella; he leaves the patella, unless there be anchylosis or extensive disease, and avoids division of the ligamentum patellæ, or of the tendon of the quadriceps extensor; the patella is everted, and the bones sawn from behind, forwards; he is careful to remove only a thin plate from the head of the tibia, so as to preserve intact the attachment of the lig. patellæ to the tubercle. This method is more difficult than those in which a semilunar or H incision is employed, and the patellar ligament or quadriceps extensor tendon divided, but it is followed by the most satisfactory results. A counter opening for discharge is made into the joint, on the outer side of the patella, which is the dependent part when the patient lies on his side, as he may easily do when plaster of Paris is used. Langenbeck uses longitudinal incisions in excision of the knee and elbow, chiefly because of their greater adaptability to the after-treatment by plaster of Paris; for, as the wound must remain uncovered, a longitudinal break in the plaster detracts less from its strength than a transverse one is found to do.

In excision of the ankle, in which the above-mentioned surgeon has been so successful, he makes a longitudinal incision over the lower end of the tibia, and not over the fibula as is usually recommended, and he never makes a transverse or crescentic incision on the outer and anterior aspect of the joint. In some of Langenbeck's cases, the patients walked so well after recovery from the operation, that, unless on close inspection, one could hardly have told that an operation had been performed.

*(To be continued.)*

---

ARTICLE II.—*On the Origin and Circulation of Nerve Force. The Address to the Edinburgh Harveian Society, in 1865.* By J. M. STRACHAN, M.D., L.R.C.S.E., Dollar.

IN searching for a suitable subject as an address to this Society, one's thoughts naturally turn to the discovery of the great man whose name we bear, and after a lapse of upwards of two hundred years we are led to inquire whether that discovery has produced

the results which might have been, and probably were, expected from it.

Regarding the fact of the circulation of the blood, very little was required, after the labours of Harvey, to render the discovery complete; so that in that direction little further progress could be made. But to the contemporaries of Harvey it must have appeared probable, that his discovery would lead to great advances in knowledge of the functions of the circulation, and of the manner in which these are performed. A careful consideration must, I fear, force upon us the conviction that this expectation has scarcely been realized.

The chief functions of the circulation are the generation of nerve force—the supply of new, and the removal of waste materials from the system—and the production of animal heat. Regarding the manner in which these are effected, our ideas are nearly as vague and imperfect as those which prevailed in the time of Harvey.

I am desirous of drawing the attention of this meeting to the first of these topics, namely, the production of nerve force; because, many years ago, I was led to conceive a hypothesis different from the opinions generally received, which seemed to me to give a clearer view of the nature and action of the nervous system.

What I have to suggest is merely a hypothesis unsupported by proof. But it must be remembered that all the opinions on this subject which now prevail, as well as those which have passed away, are entirely hypothetical, and, from the subtle nature of the nerve force, it is probable that it is only by one hypothesis succeeding another that the true theory will be discovered, which then, and not till then, will be capable of being demonstrated by experiment.

It is of great moment that, if possible, we should have clear views as to where and how the nerve force is produced, and it is chiefly to this point that I wish to draw the attention of this meeting.

I have searched carefully the works of a number of modern physiologists, to discover what are the prevailing opinions regarding the nature of the nerve force, or the manner of its production; but I find that many avoid the subject altogether, and, of the rest, the opinions are so vague that it is impossible to get from them any definite idea.

Unzer says,—" All the phenomena of motion and sensation manifested through the nerves render probable the existence of a remarkably subtle fluid essence which is present invisibly in the medulla of the brain and nerves, and is the means whereby all the functions of both are performed. It is termed vital spirits or nervous fluid; but it is not known how and when it contributes to the animal actions. It is not that fluid which is seen in the medulla of the brain and nerves, but a much more subtle spirit imperceptible to the senses. It is inferred from the phenomena

which betray its existence, that this nervous fluid is a remarkably subtle fluid, a spirituous vapour which can be neither aqueous nor glutinous, nor elastic, nor ethereal, nor electrical."

Bennett says,—"The theory at present entertained on the point is, that whilst the grey matter eliminates or evolves nervous power, the white matter simply conducts to and from the ganglionic structure the influences which are sent to or originate there.

Carpenter,—"All that we know regarding the conditions on which the production of nervous force is dependent, supports the belief that evolution involves a change of composition in the nervous matter; this change specially consisting in the cessation of its existence as a living tissue, and in the combination of oxygen with its constituents; so that, in their restoration to the condition of inorganic matter, the vital force which was previously in operation in the growth and development of the tissue is set free under this peculiar form. From this it appears that a reaction takes place between the elements of the nervous tissue and some material supplied by the blood, which is much more rapid in its character than the process of cell development, and which is especially concerned in the production and maintenance of nervous activity."

Forty years ago, there was much controversy as to whether the nerve force was identical with electricity or galvanism. The question has never been satisfactorily settled, and the combatants are now enjoying a kind of armed truce. But the immense increase of our knowledge of the nature and applications of galvanism during the last few years, makes it probable that when the warfare again breaks out it will lead to important results.

In the meantime, I think we may fairly assume that nerve force and galvanism, if they be not identical, are yet so analogous, that in speaking of the one we may use the same terms as are usually applied to the other, and take it for granted that it is governed by the same or similar laws.

From the quotations I have made, it appears that the opinion almost universally received is, that nerve force is generated either in the nervous centres or in the nerves themselves. But it must be borne in mind that this opinion is entirely hypothetical, as it has never been demonstrated either that nerve force is actually thus generated, or that the brain or nerve substance is more likely to have this power than any other animal tissue; and it may be that our adherence to this preconceived but unproved opinion keeps us from the truth, just as the established belief in the time of Harvey, that the arteries were filled with air, so long retarded his great discovery.

I am desirous of suggesting a hypothesis that I have long thought to be more consistent with the known facts, and more capable of explaining the various phenomena of the nervous system. Its foundation consists in the idea that nerve force is generated by the chemical actions which occur in the change of arterial into

venous blood; consequently that it is produced in the capillary bloodvessels which pervade every part of the animal machine.

By the combination of oxygen with carbon and hydrogen, and by the other chemical actions which are constantly going on in the capillaries, we know that a large amount of galvanism must be evolved; and if we choose to admit the identity of galvanism and nerve force, we can easily believe that from this source there must be produced a supply sufficient to explain all the actions of the nervous system. But if this identity be not admitted, we are at least justified in assuming that the force, whatever it may be, may be produced in the same way, or that the two forces are correlated and convertible into each other.

As galvanism, to produce any manifestation of itself, must be collected and conveyed by proper conductors, so the nerve force, being thus generated, is received by the minute nerve filaments which everywhere accompany the capillary vessels.

The ultimate fibrils into which nerves may be divided, are found by the microscope to be tubules containing a clear limpid fluid, surrounded by white medullary substance. It has till lately been supposed that this white substance was the conductor of nerve force; but experiments prove that it is a non-conductor of electricity, and it is probable that it is also a non-conductor of nerve force. Besides, it is found that the central tube, invested merely with a delicate transparent membrane, proceeds much farther after it has ceased to be surrounded by the white medullary covering. It is probable, therefore, that the true conductor is the clear fluid in the centre of the nerve tubule, the white medullary substance being an isolator of the tubules from each other and from the surrounding parts.

We find it impossible to convey galvanism from one point to another, or indeed to obtain any proof of its existence, except by establishing a circuit by which the influence, after running its course and producing whatever effects may be required from it, returns to the same point from which it at first proceeded. It is probable that the nerve force is subject to a similar law, and that each tubule, after running a certain course, returns to the same capillary vessel from which the force it conveys originated. Let us suppose that at one extremity it originated on the inner, and at the other terminated on the outer, surface of the capillary vessel, whilst the intervening membrane isolated the one nerve extremity from the other, it would then resemble a galvanic battery, and there would be established a circuit of nerve force, in which the current would continue so long as the chemical action lasted in the capillary vessel; and the supply of materials for the chemical action, by the circulation of the blood, being continued and uninterrupted, so would be the current of nerve force.

This idea of a continuous current in the nerves is much more consistent with the phenomena of life than the generally received supposition that nerve force is dormant until called into action by some stimulus.

It is probable that organic life consists in the continuance of these two circulations, that of the blood and that of the nerve force, dependent on and influencing each other.    But the various animal actions, such as sensation and muscular contraction, must be performed, not, as usually supposed, by the instantaneous production of nerve force, but by the interruption or modification of the current already existing.

We have thus a constant current of nerve force, from and to every point of the system, produced and sustained by the circulation of the blood, which will be influenced by every change in that circulation; and which, on the other hand, by its changes, will modify, more or less, the circulation of blood, especially in the capillaries.

These two circulations, thus kept up and influencing each other, constitute, as I have said, organic life, and we may conceive that an animal might live with those innumerable nerve currents totally distinct and unconnected with each other.    We know, however, by the phenomena, that, at least in the higher orders of animals, the various parts of the body are connected with each other in a wonderful manner, and that this is effected by the nerves.

For a nerve action to be transmitted from one part to another it is not necessary that there should be a nerve tubule originating in the one part and terminating in another; we know that if a conductor conveying an electric current be brought into proximity with another conductor, it will induce in it also an electric action.    If we admit that the same may take place with currents of nerve force, it will greatly simplify our ideas of the nervous system.    In the plexuses, ganglions, and nerve centres, the tubules are brought into such proximity that the currents contained probably influence each other by induction.    Thus, a modification of the current in one nerve tubule will affect not only that current, but also that of all the other tubules with which it is brought into this induction contact.    And when we consider the extensive and complicated manner in which the nerve tubules are brought into proximity, we can easily conceive that every nerve tubule may, directly or indirectly, influence every other nerve in the body.

I have spoken of the ganglions and nerve centres only, as the organs where the nerve tubules are brought into such proximity that their currents can influence each other; but it is probable that they have also nerve tubules originating and terminating in themselves, and receiving nerve force from their own bloodvessels.    There can be no doubt that it is so in the brain; and that it is only a small portion of that organ that can properly be considered as a nerve centre.    The large expansion of the hemispheres must be looked upon, not as a nerve centre, but as a mass of peripheral nerves which, in the cortical part, are brought into connexion with the capillary bloodvessels, and thus receive nerve force.

It is probable that each tubule of the brain, like those of the rest of the body, after running a certain course, returns to the same

point from which it originated, and that thus there is in it a con-
tinuous circuit. And just as some modification in the current of
the nerve tubules originating in a muscle produces relaxation or
contraction,—in a secreting organ, secretion,—so, a similar modifi-
cation in the tubules of the grey matter of the brain produces mental
phenomena.

The nerve tubules of the brain during their course come into
induction contact with each other, and with the tubules from the
various parts of the body, and the modifications of the currents in
the brain nerves, are capable of influencing the currents in all the
nerves with which they are connected, and of being influenced by
them in return.

We can thus understand how an impression, being made on a
sensitive tissue, will modify the current in the nerve tubules origi-
nating in a muscle producing reflex action; or the impression may
be first communicated to a brain nerve, producing a mental action,
and by it to the muscle as in voluntary motion.

Thus, the nervous system consists of innumerable circuits of
nerve force, originating at the peripheries of the nerves, these
circuits being so connected that every one is capable, directly or
indirectly, of influencing every other.

This hypothesis, which I have endeavoured to explain, appears
to me to give a clearer view of the nervous system than any of
those which have hitherto prevailed. If it were accepted as an
established theory, the origin and circulation of nerve force would
be as easily and clearly understood as the circulation of the blood
now is. Not only so, but it would be seen, that hitherto the use
of the circulation of the blood has been very imperfectly understood;
as it would then be evident that its chief function is the carrying of
nerve force to every part of the system, and that to this function
the supply of material for the growth and repair of the tissues is
entirely secondary.

The following is a brief recapitulation of the principal points I
have suggested :—

1st, That nerve force is generated in the capillaries by the
chemical actions in the change of arterial into venous blood.

2d, That this nerve force is received by nerve tubules, which,
after a longer or shorter course, return to the same point, so as to
establish a circuit, or circulation of nerve force.

3d, That the circulation of nerve force is as continuous and un-
interrupted as that of the blood.

4th, That animal actions are performed by modifications of this
nerve current.

5th, That probably the consensus of nerves is accomplished by
their influencing each other after the manner of induction.

After many years' consideration, I cannot help believing that there
is more or less of truth in these views ; and I would hope that some
one more capable of conceiving and performing the necessary obser-

vations and experiments may be induced to devote his attention to the subject.  At present, our knowledge of the nervous system is so limited, and our theories so vague and unsatisfactory, that the study of the subject by some of our accomplished physiologists would certainly yield important and valuable results.

Article III.—*Remarks on Abortion in the Early Months of Pregnancy.* By Charles Bell, M.D., F.R.C.P.E., etc.

(*Read before the Edinburgh Obstetrical Society,* 22d *March* 1865.)

When we consider the delicate texture of the embryo, and the apparently slender attachment of the ovum to the womb, it is remarkable that abortion occurs so seldom, while it proves the admirable arrangement which has been made for the safety of the fœtus.  It is, however, more frequent and is attended by more serious results than is generally supposed.  In evidence of these facts, we find few married women who have not miscarried; and at whatever period this misfortune takes place, it is liable to produce a severe shock to the constitution, and to be accompanied by hæmorrhage; and although Dr Collins asserts that this seldom proves fatal before the sixth month, it is often very profuse and unmanageable.  Even when the immediate symptoms and results of abortion are apparently of little importance, it often lays the foundation of delicate health, which may ultimately prove fatal. At the same time it is extremely apt to recur; as an example of which, Heberden mentions the case of a woman who miscarried five and thirty times.  Abortion is therefore an event which women cannot guard against too carefully, or the accoucheur watch with too much attention.

Numerous causes have been assigned as productive of abortion; and Dr Whitehead, as well as some foreign authors,[1] go so far as to assert that it is rendered epidemic by certain states of the atmosphere. Sourcerotte states that it is so common in some mountainous districts, that the women, in consequence, are in the habit of descending to the plains whenever they become pregnant, in order to avoid the misfortune.  It is, however, in this country most commonly the result of sudden mental or bodily shocks,—such as, frights, surprises either of a pleasant or unpleasant kind, violent fits of passion, slipping of the foot, or stumbling, which are generally forgotten by the patient; and when abortion takes place, as a consequence of some of these causes, it is ascribed to some trifling circumstance which has just occurred,—such as the smell of a candle that has been blown out, or to the hurriedly raising the arms[2] above the head.  But some remarkable contrasts to these cases

[1] Le Dictionnaire de Medécine, vol. iv.            [2] Ibid.

have been recorded by different authors. Mauriceau relates the case of a woman who fell from a height of three stories on the pavement and fractured her arm, in endeavouring to escape from a fire, yet she did not miscarry. Dr Whitehead also mentions the case of a woman whose skull was fractured by a blow from an axe, yet she went to her full time and was delivered of a healthy child. Ulceration of the os uteri is a frequent cause of abortion, and judging from my observation, it is generally present in those cases in which this event recurs repeatedly at the same period of gestation. The sex of the fœtus has been assigned as a cause of abortion ; some authors asserting that the male fœtus is very apt to be aborted ; while others, along with Desormeux,[1] consider this an erroneous supposition, arising from the enlarged clitoris being mistaken for the penis, and that miscarriage is more likely to occur when the fœtus is a female. We thus find that there is great diversity of opinion in regard to the cause of abortion, and that there is scarcely a circumstance which has not at one time or another been assigned as productive of it.

The following cases afford interesting examples of abortion in which the embryo is blighted or arrested in its development at an early period, but is not expelled for some time after ; while the secundines continue to increase in size, and form a firm attachment to the womb, and are not separated without considerable hæmorrhage. The first case, however, scarcely comes under this class ; but I am induced to relate it, as showing the effect of sudden fright, not only in occasioning miscarriage, but in producing effusion of blood in the fœtus. I was requested some years ago to see Mrs P., who had been only a few months married, and who was represented to be suffering from severe pains occurring occasionally in the lower part of the abdomen, which had been preceded by vomiting. On my arrival, I found she had just been delivered of a fœtus apparently in the third month, which had a large ecchymosis on its head. The only cause she could assign for her miscarriage was a fright she had about ten days before, by a large black cat springing upon her unexpectedly when she opened the door of a closet.

The next case was that of a lady, who, when I first saw her, was under the care of a country practitioner, in consequence of having just miscarried in the third month, but for which she could assign no cause. She was a remarkably well-formed woman, rather above the middle size. She enjoyed in general good health, and had apparently an excellent constitution. She had had several children at the full time, and one in the eighth month, previous to my seeing her. As she was doing well, I saw her only once on that occasion. But on the 7th July, about ten months after the above attendance, I was again requested to see this lady, who had come to town. I found her suffering from flooding, but she had no pain or bearing down, and her pulse was 72, of natural strength. The flooding was pre-

[1] Le Dictionnaire Medicale, vol. iv. p. 451.

ceded by a watery discharge. She thought she was in the third month of pregnancy. She was ordered to take twenty drops of the aromatic sulphuric acid in water every three hours.

*8th July.*—Passed a good night, and had no flooding, and on examination, per vaginam, the os uteri seemed closed.

*9th.*—There was a slight return of flooding during the night, but it soon ceased. Skin cool, pulse natural. To take the following draught :—℞. Sulph. magnesiæ, ℨiij.; Acid. sulphur diluti, gt.xv.; Inf. rosæ, ℨ j.—M. ft. haustus.

*15th.*—Has had no uneasiness or return of discharge since last report, and she feels stronger. The areola was dark-coloured and puffy; but the papillæ had a shrunk and faded appearance, which led me to conclude that the fœtus was blighted, and that it would soon be expelled. To remain quiet in the recumbent posture for some days.

*4th August.*—Complaining much of flushing of her face, and a sensation of heat all over her, but her skin was cool, and her pulse 84. These symptoms were relieved by taking a mixture of spirit of Mindererus and nitric ether.

*5th.*—Slight return of flooding, but no pain nor bearing down; she has a sensation of throbbing in her breasts, which she thinks have become fuller and her stomach larger. Skin cool; pulse 100, soft. This increase in the pulse seemed to be owing to the excitement of receiving visiters.

*6th.*—In the morning she seemed much as she was yesterday. On examining the breasts, the papillæ were more flattened, and the nipple less prominent. In the evening, she had pains in her back and loins, returning at irregular intervals. Bowels moved by a colocynth pill and a seidlitz powder. To have twenty drops of laudanum, which was to be repeated if the pains continued.

*7th.*—The pains were allayed by the laudanum, but they returned this afternoon, accompanied by a watery discharge, which was followed by a slight flow of blood. On examination, per vaginam, the womb was found high in the pelvis, and the os tincæ seemed small. Pulse 120, feeble. The opiate was repeated with relief, and she took two doses of the diluted sulphuric acid. She would not allow the application of cold externally. In the evening she felt drowsy and chilly.

*8th.*—Complaining of increased chilliness and a weight above her eyes, and occasional pains; face slightly flushed. Pulse 108. To have a tablespoonful of the following mixture every four hours :—℞. Mist. camphoræ, ℨiss.; Liq. ammoniæ acetatis, ℨi.; Aq. cinnamomi, ℨss.—M. ft. mistura. In the evening she felt more comfortable; pulse 100. To take a laxative pill at bedtime.

*9th.*—Was sent for at 7 A.M., and found that she had had regular pains from 4 P.M. until shortly before my arrival, but I found her quite free from pain, and there was very little discharge. She had twenty drops of Battley's tincture of the ergot and an enema.

The pains were not renewed, but when the enema came away the fœtus was expelled at the same time, showing that it must have been lying in the vagina. The bowels were much loaded; and, as the fœtus was received in the pot de chambre, I could not discover whether or not the membranes had also been expelled. I suspected, however, that they were still retained in the uterus. I therefore examined it; but from its being high in the pelvis, and the cervix so much directed towards the sacrum, I was obliged to hook it down with my finger before I could reach the os. I could not, however, feel the membranes. The walls of the uterus felt thick, and there was considerable tenderness to the touch. The ergot was repeated without effect. I felt more anxious about the membranes being expelled, as they were retained for some weeks after her last miscarriage, and occasioned considerable hæmorrhage before coming away. She felt much relieved after the fœtus was expelled and seemed very cheerful. She had no return of pain, and she had scarcely any discharge. The fœtus seemed to be between the third and fourth month.

10*th.*—Remarkably well; no pain. Lochia scanty. Pulse 100, and soft. To have castor oil.

18*th.*—She gradually improved in health and appearance, and was able to be in the drawing-room on the 16th; but this morning she was not very well, and became feverish toward evening. To have the camphor mixture and the spirit of Mindererus in a draught at bedtime.

19*th.*—Had a good deal of bearing-down pain for two hours in the morning, which expelled what appeared to be the membranes, along with some clots, one of which was nearly as large as the unimpregnated uterus, and had an opening in it at the one end. It had the appearance as if the membranes had been inverted and filled with blood. The other clots had no definite form and were much smaller. After these clots came away there was very little discharge, but she felt much exhausted. She was inclined for food, and took some coffee with bread and butter. To have oil in the morning.

20*th.*—Much improved. Bowels moved by the medicine. Pulse natural. Lochia very trifling in quantity. To have nourishing diet.

23*d.*—Has been going on well, but she looked pale. The lochia small in quantity and very pale. To go to seaside in the evening.

30*th.*—Had slight rigors during the day, and felt feverish. Pulse rapid and feeble. She said she had a return of discharge in considerable quantity the evening she left town, and she has felt so weak and feverish ever since that she has not been able to be out of bed. To have a mixture of camphor and Mindererus.

1*st September.*—Was seized with violent flooding at three in the morning, and when I arrived, at 7 A.M., she was blanched, faint,

and almost pulseless. Cold had been applied before my arrival, and she had two doses of Battley's tincture of the ergot, which brought on strong uterine pains and moderated the discharge. I repeated the ergot and plugged the vagina, and continued the cold applications. After taking the ergot she was seized with pain low down in the back, which became so severe that it was necessary to give her thirty drops of laudanum. Although she was not conscious of any bearing down during this pain, there must have been considerable uterine action, as the handkerchief used in plugging the vagina was partly expelled. There was no return of the flooding, but, judging from appearances, she must have lost in the course of the morning from fifty to sixty ounces of blood.

As she was in a very alarming state of weakness, I suggested a consultation, and Dr Beilby was called in, and he advised the plug to be removed, in order to ascertain the state of the parts. On examination, the walls of the womb seemed much in the same state as formerly described; but in place of the cervix being towards the sacrum, it was now turned to the pubis, and could be easily reached by the finger, and the posterior lip was found to be thin, and its internal surface was occupied by a ragged fleshy mass, which Dr Beilby considered was the placenta, and endeavoured to remove it by means of a pair of long forceps and blunt hook; but he could bring away only small portions, which had a blanched appearance. I afterwards examined her, and, in consequence of her having had another dose of the ergot, I was enabled to reach the mass more easily with my finger, and brought away a portion the size of the point of my little finger. It had quite the appearance of macerated flesh. The mass seemed so firmly attached that it was considered desirable to leave it alone, more particularly as the patient had become much exhausted and required rest. The pulse gradually improved towards evening, and there was no return of hæmorrhage.

2d.—Has passed a tolerably good night, and has had no return of pain nor hæmorrhage. On removing the plug, the fleshy mass seemed larger and lower down; but as it was still firmly attached, Dr Beilby was afraid to use any force in removing it, lest the womb might be injured. He now supposed it to be a polypus with a broad base. To be kept quiet and to have arrow-root for food.

3d.—Has had a pretty good night and feels stronger, there having been no return of pain or discharge; pulse 84. On examination, the fleshy mass seemed much the same in size, but was obviously in a state of decomposition from the offensive ichorous discharge that was coming away. A small portion was removed by the finger, which had the same appearance as what had been taken away before, and it had an offensive smell. Her bowels not having been moved for some days, she got an enema, which produced nausea, but it acted well, and she was much refreshed by having her bed made.

*4th.*—Had severe pain in the womb during the night, which seemed to have forced off the fleshy mass, as the nurse found it in the bed in the morning. It was broken in small pieces, and was in a complete state of decomposition. On examination per vaginam, the os uteri was sufficiently open to admit the finger easily, and the internal surface of the cervix felt smooth, and there was only a small quantity of pale discharge coming from the womb.

In the afternoon she had a rigor, which was followed by feverishness; and in the evening she complained of headache and pain in the left groin, which was slightly tender to the touch. Skin hot; pulse 100; feet cold. She had a peculiar quickness in speaking,[1] which was foreign to her; and from a certain wildness in her manner, I feared that some head affection was coming on, which opinion was not lessened by her expressing a wish to sit up in bed, saying she " was quite able to do so, and that she would not require any medicine next day." To have half an ounce of the spirit of Mindererus, in water, every three hours.

*4th.*—Passed a good night, and had a great deal of sleep. Pain in the groin and headache gone. The discharge very pale and scanty. Bowels open. Was very anxious to be allowed to sit up to read. To have castor oil, and afterwards to have chicken soup and a piece of chicken.

In the evening she was free from all uneasiness, her bowels having been freely acted on by the oil.

*6th.*—Feels very well. On examination, the os uteri was dilated, and the walls of the organ still thickened. The discharge very offensive. To have injections of the sulphate of zinc.

*15th.*—Much improved in appearance, having lost the sallow colour. Feels stronger, and can sit up for a considerable time without fatigue. Appetite good. To have quinine and aromatic sulphuric acid.

*22d.*—Improving, but has a slight return of discharge, which induced the nurse to stop the injections. To continue the tonics.

*25th.*—Although much stronger than she has been since her illness, she still feels very weak on first getting out of bed. There is still some leucorrhœal discharge and the os uteri is swollen, conveying the sensation to the finger as if it were everted; but there is no pain on pressure. She has occasionally a sense of pain and weakness over the sacrum. To continue the tonic, and have wine or beef-tea immediately on getting out of bed.

From this period she gradually regained her usual health. In giving up my attendance, I pointed out the propriety of living absque marito for some months; whether this advice was followed or not I cannot say; but I was not called to attend her for three

---

[1] It is of great importance to observe the tone and manner of speaking in puerperal patients, as it often happens that the approach of some serious attack may be prognosticated by sudden change in the voice, which, in head-affections, assumes a hurried and sharp tone.

years, when I delivered her of a fine healthy child in the eighth month, after which she made a good recovery, and enjoyed excellent health for several years, but ultimately died of cancer of the ascending colon.

The next case is that of a lady who miscarried in her sixth pregnancy. She sent for me on the 27th September, when she informed me she thought she was in the third month of pregnancy, and that she had flooding for several days, which had produced an overpowering feeling of exhaustion. She had no pain, and could assign no cause for the discharge. Her breasts were soft and flabby, and although the areola was dark coloured, the papillæ were depressed and faded-looking, which led me to suspect that the fœtus was blighted. I recommended her, however, to keep the recumbent posture, and to take ten drops of the elixir of vitriol three times a-day.

28th.—Slight discharge. The os tincæ felt closed, and there were no clots in the vagina. To continue the treatment.

29th.—The oozing of blood still continuing, I plugged the vagina. In the evening, labour pains came on, and I removed the plug to ascertain the state of the womb, when I found it completely anti-verted, and the os tincæ nearly as high as the promontory of the sacrum, and I had some difficulty in drawing it down, when it appeared very small, although the discharge continued to trickle from it in small quantity. The pains being lingering, I gave her a dose of the ergot, which had the effect of increasing their frequency without rendering them more effectual; and as she became much exhausted after suffering for upwards of an hour, I gave her twenty drops of laudanum, in the hope of procuring her some rest, and before leaving plugged the vagina.

30th.—In place of relieving the pains, the opiate seemed to increase their efficacy, and they continued to return regularly until twelve last night, when they ceased. On removing the plug, the fœtus was found lying in the vagina. It was in a state of decom-position, and the cord gave way in removing it, leaving the membranes in the womb. The fœtus seemed only in the second month, and had a large ecchymosed tumour on its head. A dose of the ergot was given, with the view of renewing uterine action and throwing off the membranes, but without effect.

1st October.—Had a return of uterine pains in the morning, which expelled the membranes. There was only a trifling discharge of blood, and when I saw her she felt quite comfortable.

She went on well, and was able to be out of bed on the eighth day, although the discharge had not ceased, and she soon after-wards left town. I, in consequence, did not see her until the 15th November, when she informed me that she had never recovered from her miscarriage, and that the coloured discharge had rather increased lately. I ordered her to take twenty drops of the elixir of vitriol three times a-day, and to confine herself to the sofa.

*16th November.*—Is decidedly worse, the flooding having much increased during the night. The vagina much relaxed, and the os uteri considerably dilated; and there was a bright-coloured discharge, almost like arterial blood, flowing from it. She was very pale and felt weak. Plugged the vagina, and ordered her to have two grains of the acetate of lead and quarter a grain of opium, to be taken every four hours.

*17th.*—Being unable to see her, Professor Simpson kindly visited her to-day for me, but ordered no change.

*18th.*—Passed a good night and feels rather better. On removing the plug, several clots came away; and although the discharge was trifling, I again plugged the vagina. Bowels moved.

*19th.*—Feels better; but as the discharge is still continuing, she expressed a wish to see Dr Thatcher, who ordered her to have an enema. On removing the plug, to have the vagina injected with a decoction of oak bark, which was to be repeated according to circumstances. To continue the plug. To have wine and arrow-root.

*21st.*—Much the same. Dr Thatcher again saw her, and recommended the remedies to be continued.

*27th.*—Little change. To have fifteen drops of the tincture of the muriate of iron, three times a-day, in a wineglassful of water.

*2d December.*—No material change, and as she thinks the iron increases the discharge, it was stopped, and a teaspoonful of the following mixture to be taken instead, three times a-day:—℞. Tinct. kino, Tinct. catechu, ā ā ℥ss.; Mucilag. acaciæ, ℥ij.; Aquæ fontanæ, ℥iiss.—M. ft. mistura.

*7th.*—Has had no discharge since she began to take the above mixture.

*10th.*—Gaining strength, and has had no return of discharge.

I afterwards attended this lady in three confinements at the full time, which were all natural and unattended by any threatening of hæmorrhage or untoward symptom.

The following cases are examples of that rare form of abortion in which we find what is called the tuberculated ovum, in which the fœtus is blighted or arrested in its development, while the membranes continue to increase in size and assume externally a thickened and fleshy condition, while internally they have a tuberculated appearance. It has been correctly illustrated by Albinus, Ruysch, Denman, and Granville, and is clearly described by Professor Simpson, in his Obstetric Works, under the head of Congestion of the Placenta.

I was requested, on the 27th November, to see Mrs B., a lady of delicate constitution, who had married late in life, being considerably above forty, and therefore despaired of having a family. She informed me she had observed a spot of blood on her chemise on getting out of bed, and that she felt much exhausted, but she had no pain nor sickness. She had not been unwell for five months, and soon after her last monthly period she suffered from morning sick-

ness, and she observed that she gradually had become stouter until a few weeks ago. Since then she has remained the same size. She has enjoyed the best health for the last five months, with the exception of an acute pain in the lower part of her stomach, which came on about three months ago, which she attributed to sitting on the damp ground while taking a sketch, but as it lasted only a few hours, she thought nothing of it. She has not had any sensation of quickening, and does not believe she is pregnant. To keep the sofa, and to take thirty drops of the diluted sulphuric acid should any discharge come.

*30th.*—There was a slight return of discharge on getting up, but it did not continue. To take the acid twice a-day.

*1st December.*—Was rather out of spirits, and complained of giddiness. Had slight discharge in the morning on getting out of bed. To take a wineglassful of the following saline mixture, and to repeat it every three hours if the bowels do not act.—R. Sulph. magnesiæ, ʒi. ; Acidi sulphurici diluti, ʒi. ; Inf. rosæ, ʒxii.—M. ft. mistura.

*25th January.*—Have not seen the patient since last report, and was requested to see her to day in consequence of her having severe pains in her back and stomach, and extending down her thigh, which came on at six in the morning, and continued to return at intervals until shortly before my arrival. There was slight discharge of blood, and she had had a tendency to flooding some days previously, which she thought was merely her monthly discharge, and as it was more scanty than usual, she took a long walk, which increased it considerably, but it did not continue. She then thought it of no importance. She ridiculed the idea of her being pregnant, and in this opinion she was joined by her husband, who was a medical man. I was convinced, however, that she was pregnant, and suggested the propriety of my being allowed to examine her per vaginam, in order to enable me to pursue a more satisfactory treatment, but she would not consent to this. I therefore merely gave her a teaspoonful of the tincture of Valerian, and ordered linseed poultices to be applied to the stomach.

In the morning the pains had left her, and there was scarcely any discharge ; she was therefore allowed to get up to have her bed made, and took some negus.

*26th.*—Has had no return of pains, and there was very little discharge. She showed me a mass which had passed from the vagina in the morning without pain. It was about the size and shape of a large duck egg, pale in colour, and had a fleshy feel. On laying it open, there was found a fœtus about the size of a bee, showing that it had been blighted between the first and second month of gestation. The eyes were large and prominent, and the extremities were just beginning to sprout from the trunk. The whole internal surface of the membranes had a nobular or tubercular appearance, producing the effect as if the space between the amnion and chorion

were occupied by varicose veins or folds of the umbilical cord. These tubercular bodies were of a deep-purple colour, and some of them were as large as a horse-bean, and hung pendulous into the cavity of the ovum, and seemed to be covered only by the amnion. The umbilical cord was about two inches and a half in length, and was nearly as transparent as the swimming-bladder of a fish, to which it bore a considerable resemblance. The liquor amnii was about a drachm in quantity and of a reddish colour.

27th.—Doing well, there being no pain and the lochia moderate in quantity. To have a dose of rhubarb and soda; to be repeated if necessary.

28th.—She now mentioned that she recollected, when she must have been eight weeks pregnant, that she had a sudden fright from a child running towards the drawing-room window, which was open, and she thought it had fallen out of the window. She was immediately seized with pain in the lower part of the abdomen, and a nervous tremor which continued the remainder of the day; but she felt quite well the following day, and the whole circumstances escaped her recollection until this morning.

It is very probable that this shock destroyed the fœtus, although the secundines continued to increase. Her recovery, although slow, being occasionally interrupted by feverish attacks, was ultimately complete; and she became pregnant, and I delivered her with forceps, at the full period, of a fine healthy boy, fourteen months after this miscarriage. The placenta adhered nearly throughout its whole extent, and I had some difficulty in removing it, as the womb continued to contract and relax irregularly on my hand for fully half an hour. She made an excellent recovery, however, and was able to be in the drawing-room in three weeks after her confinement.

Thirteen months after her confinement, I was again requested to see this lady, when she was again suffering from symptoms of miscarriage; but, as I was out of town, Professor Simpson saw her. She was then (22d April) apparently about four months pregnant, and as there was a considerable amount of flooding, she was ordered to take four grains of gallic acid every three hours. This medicine not only moderated the flooding, but also diminished the quantity of urine.

23d April.—I saw her this morning, when there was little discharge and no pain. To continue the gallic acid.

27th.—Scarcely any discharge, and no pain. On examination per vaginam, the ovum was protruding through the os uteri like an egg. There being no chance of her going on, I informed the husband that it was desirable that she should have some of the ergot to stimulate the uterus into action to expel the ovum, but this he opposed.

28th.—Dr Simpson therefore saw her to-day, and agreed in the propriety of the ergot being given, and on the second dose of

thirty drops of Battley's tincture being taken, the ovum was expelled without pain. On laying it open, it was found to be another example of the tuberculated ovum, and in every respect the same in appearance as the one which was expelled in the first pregnancy. She made a good recovery.

I was again hurriedly called to see this lady in the following December (14th), eight months after her last miscarriage, when she stated that she had a sudden discharge of blood without any pain, while she was sitting on the sofa in the drawing-room. She complained of a rolling sensation in the lower part of the abdomen as if from flatulency, but she had no pain and no discharge. On examination, the womb felt low in the pelvis, and the os tincæ was slightly dilated. She was ordered to take four grains of gallic acid, in the form of pill, every three hours, and, if pain came on, to take five-and-twenty drops of morphia.

*15th Dec.*—Passed a good night, having taken the morphia, and she has neither pain nor discharge. To take twenty drops of the diluted sulphuric acid, in place of the gallic acid, three times a-day.

*16th.*—Feels quite well, having had no return of pain or flooding. The bowels moved by a lavement. To continue the acid, and to keep the recumbent posture. To have moderate diet.

*20th.*—Has gone on well until this afternoon, when she had a return of flooding without pain, and seemed to have lost about four ounces of blood. There was no change in the state of the womb, and the discharge was checked by a dose of the sulphuric acid, which was to be repeated if necessary.

*24th.*—Has had no return of discharge. I applied the stethoscope, and thought I heard the placental souffle, but it was very indistinct.

*29th.*—There was a slight return of discharge without pain. The breasts and abdomen still increasing in size. To continue the acid.

*30th.*—Again examined with the stethoscope, but could hear no sound. To take nourishing diet and continue the acid.

*31st.*—Slight discharge of grumous fluid, much paler than blood. On examination per vaginam, the uterus did not seem larger than in the third month of pregnancy; but the os was sufficiently large to admit the point of the finger. No pain. To return to the use of the gallic acid.

*1st January.*—Passed a rather restless night. Has no inclination for food, which she ascribes to the gallic acid. Pulse 62, natural. Bowels constipated; very little discharge. To take the sulphuric in place of the gallic acid. To have a full dose of Gregory's mixture; to be followed by a lavement.

*4th.*—There was no change in her symptoms; and as I had no hope of her going to her full time, I ordered her to have a stimulating enema, and to take thirty drops of the tincture of the ergot every half-hour until she had taken two drachms; but on my return in the evening there was no change, except that the discharge was diminished.

*5th.*—Has taken two drachms of the ergot and an enema without producing any effect on the womb. On examination, however, the os tincæ seemed to be occupied by a hard substance, the true nature of which could not be ascertained.

*7th.*—The symptoms much the same, except that the discharge had assumed the same rusty appearance as in her previous miscarriages. I therefore anticipated that the ovum would prove to be the same in character as on former occasions, and pointed out to the husband the advantage of its being removed as soon as possible; and as the ergot had proved ineffectual in rousing the action of the womb, I proposed introducing a sponge-tent. To this, however, he objected; and I then requested that Dr Simpson should be consulted, which was done, and he agreed with my suggestion, with the addition of lavement and a hot hip-bath in the morning.

On examination per vaginam, the uterus was found to have become much antiverted, and I had some difficulty in reaching the os and introducing a small sponge-tent. Its lips were thin, and the body of the organ was painful on pressure.

*8th.*—Passed a good night and had no pains. The tent was lying in the vagina, and the womb was more antiverted than it was the previous evening. She got an enema, which had the effect of lowering the os, and enabled me to introduce another tent more easily.

In the evening she had slight pain, and a stretching sensation in the lower part of the abdomen; but there was no excitement or heat of skin, and the pulse was only 72.

*9th.*—I was sent for at 5 A.M., and found that labour-pains had come on soon after composing herself to sleep, and had continued to return at regular intervals until my arrival, when they were less severe, but were accompanied with distressing nausea. With the view of obtaining sleep, she had taken thirty drops of morphia, which she immediately rejected.

I gave her thirty drops of the ergot, and repeated the dose in a quarter an hour with the most marked improvement in the strength of the pains; but she became drowsy about 9 A.M., and I left her with instructions to have an enema when she wakened.

On my return, I found that the ovum had been expelled along with the enema. It was about the same size, and presented the same general character as those she had been delivered of in former occasions. I gave it to Professor Goodsir, who laid it open, and found the fœtus in the centre about the size of a small bean. The membranes had the same tuberculated appearances as those already described. The patient made a good recovery.

On the 15th December following, I was again requested to attend this lady, when she said she was afraid she was going to have another miscarriage, as she was in the fifth month, and had not felt movement; she was therefore anxious to know if she

should pursue any particular course. The breasts were enlarged, the areola dark coloured, the nipple erect; the abdomen was considerably fuller than in her previous pregnancies at the same period; her general health was good; and she had had no uneasiness of any kind, except a feeling of languor. I was therefore in hopes that she was going on favourably, and merely ordered her to take gentle exercise, to keep her bowels regular by mild aperients, and to avoid as much as possible exposure to sudden shocks or surprises of any kind.

*20th December.*—There was a slight pale discharge from the vagina, unaccompanied by pain or uneasiness. To take fifteen drops of the diluted sulphuric acid three times a-day, and to increase the dose if the discharge becomes more profuse or of a darker colour.

*27th.*—Has continued much the same as at last report until last night, when she felt very uncomfortable, and has had frequent pains in her back and the lower part of the abdomen, which were not removed by taking a dose of morphia. She has in consequence had little sleep. At 8 A.M., when I arrived, the pains had ceased, and there was scarcely any discharge from the vagina. She felt, however, much sickness. Being satisfied that the cessation of the pains and discharge was in consequence of the expulsion of the ovum into the vagina, I merely ordered an enema to be given, in the hope that when it came away the ovum would also be expelled, which was the case, and the sickness also subsided.

The ovum was entire, and presented exactly the same appearance as all the former ones did. On laying it open, the fœtus was found about the size of a bee, and the membranes were thickened and tuberculated. The liquor amnii was of the colour and consistence of serum, and amounted to about three drachms.

Her recovery was more rapid than usual, although there was a threatening of hæmorrhage three weeks after her delivery, which readily yielded to the use of the gallic acid.

### Concluding Remarks.

It appears from the history of the above cases that it is impossible to guard against abortion in every instance, as for the most part the causes take effect suddenly and unexpectedly. Our object therefore should be more particularly directed to its treatment, so as to diminish as much as possible the effect on the constitution, and to place the patient in the most favourable position for regaining her health. These objects will be most satisfactorily attained by ascertaining early the true condition of the ovum, and assisting the womb to expel it without delay, if there is reason to believe that it is blighted, in place of vainly attempting to check the natural efforts of the womb to free itself of what has actually become a foreign body and a source of irritation and disease.

The symptoms which seem to indicate the death of the early

fœtus are a blighted and shrunk condition of the papillæ of the breasts, and a relaxed and flabby state of the nipple, accompanied by a watery or rusty discharge, coming from the womb without pain or uterine effort, which are generally attended by extreme lassitude and exhaustion, amounting in some instances to faintings. In such cases, I should advise, even where there has been no flooding, the use of the ergot; and if it failed to promote the action of the uterus, then I should recommend the introduction of sponge-tents, which will be sure to accomplish the object. Should flooding come on, which is almost certain to occur to a greater or less extent, it may be necessary to employ plugging along with the external application of cold. To these remedies may be added the internal use of large doses of the elixir of vitriol or the gallic acid. When uterine action is attended by severe pain, opiates or chloroform may be given with advantage. In order to restore the tone of the system after abortion, I have found chalybeates with port wine the most useful tonics.

In making these practical remarks, I am fully aware they contain nothing that is new or original; but I am induced to urge their application in consequence of having observed the absurdity of attempting to prevent abortion taking place after it had become obvious that the fœtus was blighted; and I have known tedious bad health produced by the blighted ovum being retained in the womb.

---

ARTICLE IV.— *On some Congenital Deformities of the Human Cranium.* By WM. TURNER, M.B. (Lond.), F.R.S.E., Senior Demonstrator of Anatomy in the University of Edinburgh.

*2d, Congenital Deficiencies in the Cranium.*—The very remark-able skull-cap, the anatomical peculiarities of which I shall now describe, was obtained from a woman, æt. 25, by Dr T. J. Maclagan, of the Dundee Royal Infirmary. From some unusual circumstances connected with the death of the patient, which were of interest in a medico-legal point of view, the calvarium was sent to Professor Maclagan, by whom it was exhibited to the Medico-Chirurgical Society of this city, at a recent meeting.[1] Through the kind permission of both those gentlemen, I have been allowed to examine and report upon its very noteworthy teratological characters.

In the middle of the interparietal or cerebral portion of the occipital bone was an oval opening with a smooth and rounded margin. Its long axis was vertical, and measured one inch; its transverse diameter was a little more than half an inch at its

[1] See the May number of this Journal, p. 1040.

widest part. From the upper end of this opening a suture half
an inch long passed upwards in the middle line, and reached the
lambdoidal suture close to its superior angle. The opening ex-
tended at its lower end as far as the occipital protuberance. Dr
Maclagan states, that a cystic tumour containing a clear fluid was
situated over this part of the cranium, that it had been observed
from childhood, and that prior to its removal a few days before
death, it had assumed the size of a hen's egg. The opening was

Figure 2.[1]



closed up by a membrane. At the commencement of the posterior
slope of each parietal bone, an oval opening, possessing a rounded
margin, was situated, the inner end of each of which was about
half an inch from the middle line. The long axis of each aperture
was transverse ; that on the right side measured 8-10ths of an
inch, that on the left 6-10ths; the antero-posterior diameter of the
right was at its widest part 6-10ths, whilst that of the left was only
4-10ths of an inch. Each opening was in the recent state closed
in by a membrane, which Dr Maclagan tells me was cribriform,
though, from difficulties connected with the performance of the
sectio, it is not possible to say exactly what the cribriform state of
the membrane was due to, but it is probable that small veins,
proceeding to the superior longitudinal sinus, passed through it.

From the inner end of the opening in the left parietal bone, a
suture was directed inwards as far as the middle line, and then
extended downwards and backwards for $1\frac{1}{4}$ inch, occupying the
position of the posterior end of the sagittal suture, and joining the
lambdoidal suture at its superior angle. No suture proceeded from
the inner end of the opening in the right parietal region, but from
the indented appearance of the bone, one may perhaps have existed

[1] Figure 2 from a drawing by my pupil, Mr Richard Caton.

at an earlier period, and in the course of time become obliterated. Several very small vascular foramina were situated in the bridge of bone which passed between the two parietal openings, and these evidently had transmitted minute veins to the superior longitudinal sinus. The groove for this sinus occupied its usual position in the middle line of the inner surface of the anterior part of this skull-cap, but when it reached the region in which these parietal openings were situated, it was deflected, and passed close to the inner end of the right parietal opening. Continuing its course downwards, it preserved its position to the right side of the middle line, and passed close to the right side of the aperture in the cerebral part of the occipital bone. From the inner end of the left parietal opening a groove extended inwards and joined the groove for the superior longitudinal sinus. It was situated anterior to, and quite independent of, the suture already described, and, judging from its appearance, had probably lodged a small venous sinus. At the outer end of each parietal opening the bone was elevated externally, and apparently corresponded to the parietal eminence.

The parietal bones, anteriorly, also presented some unusual appearances. Thus, the right bone sent a beak, half an inch long, forward into the frontal bone, which occupied the position of the anterior half of the fontanelle, and in this respect corresponded with some of the scaphocephalic crania I described in my paper in the Natural History Review, already referred to. Faint traces of a suture, visible only externally, might be seen commencing at the coronal suture, immediately to the left of the base of the beak. It extended in an interrupted manner backward for about one inch and a half, and then disappeared, so that behind this spot to one midway between the two parietal openings, the sagittal suture was entirely obliterated, and the two parietal bones were completely blended together. The coronal, lambdoidal, and posterior end of the sagittal suture were well marked, both internally and externally. The skull-cap was oval in form. Its extreme length was 7·1 inches; that of the parietal bone, including the beak, 6·2 inches. The greatest breadth of the frontal region was 4·1; of the parietal, 5·; of the occipital, 3·4 inches. The horizontal circumference was 19·7 inches. Neither in size nor general form was there any appearance of the skull having been altered by hydrocephalus.

The very remarkable series of deviations from the normal construction of the cranium which this specimen exhibited, conferred on it characters which have not, I believe, been met with previously in conjunction in a single skull. Variations in the mode of arrangement of the bones are, however, occasionally met with in the parieto-occipital region of the cranium. Amongst the most familiar of these is the occurrence of Wormian or triquetral bones in the lambdoidal suture, which may vary in size, form, and number, and may sometimes be so arranged as to occasion a remarkable shelf or ledge-like projection in this part of the skull. A very characteristic

specimen of this description has been figured by Tarin;[1] others have been represented by Lucae in his large work on abnormal forms of skull,[2] and several specimens exhibiting the shelf in a more or less strongly marked form, have come under my own observation; and of these, two crania in the Anatomical Museum of the Edinburgh University, 109, 116, are very striking examples. Variations also occasionally occur in the mode of ossification of the cerebral part of the occipital bone itself. One of the best known of these consists in the presence of a transverse suture extending from one side of the lambdoidal suture to the opposite, and separating the whole or a great part of the supra-spinous from the cerebellar portion of the occipital bone. The interparietal bone thus formed was at one time supposed to be a characteristic feature of the Peruvian cranium, and was named by Von Tschudi in accordance with this view, os incœ. But it is now known that this opinion was based on imperfect observation. For not only is the interparietal bone of exceptional occurrence even in the Peruvian skull,[3] but it may exist as an individual peculiarity in the crania apparently of men of any race. Most large collections of skulls contain one or more specimens exhibiting an interparietal bone, and in some crania it may even be subdivided into smaller ossicles. Thus, it may be separated by a median vertical suture into two lateral halves, or it may be subdivided by two vertical sutures into a median and two lateral portions, and these may vary as regards size and shape; or one half, or only a portion of the cerebral part of the occipital bone, may be separated by a transverse suture, the other being perfectly continuous with the cerebellar division of the bone. Examples of one or other of these forms were known to and described by the older anatomists, as Vesalius, Eustachius, and Albinus, and by Meckel in his treatise on pathological anatomy. Tiedemann has also figured[4] several interesting specimens, Dr Humphry[5] has alluded to others, various forms have come under my own observation, and during the present year, Jacquart[6] has figured an additional series of skulls in which somewhat similar arrangements were observed.

But cases have also been recorded in which, quite independent of the presence of an interparietal bone, the occipital bone has possessed in the adult skull a median vertical suture, extending downwards from the superior lambdoidal angle. At the time of birth, indeed, one occasionally sees a fissure or depression in the supra-spinous part of this bone, extending for a greater or less dis-

---

[1] Osteographie. Pl. 2. Paris, 1753.

[2] Zur Architectur des Menschen Schädels. Frankfurt, 1857.

[3] See on this point an excellent paper by Dr M‘Bain, R.N., in Proc. Roy. Physical Society of Edinburgh, April 22, 1863. My own observations made on crania, not only in the Edinburgh Museums, but elsewhere, coincide with the conclusions of that gentleman.

[4] Zeitschrift. Vol. iii. p. 217. Pl. 16, e. s. 1829.

[5] Treatise on the Human Skeleton. Cambridge, 1858.

[6] Journal de l'Anatomie et de la Physiologie, May 1865, p. 244.

tance downwards to the spine. Meckel states that Vesalius and Reisel have seen the occipital bone divided into two parts by a suture extending vertically from the superior lambdoidal angle to the posterior margin of the foramen magnum; and I find that Peter Paaw,[1] in his Commentary on the eighth book of Celsus, page 8, figures an adult skull in which this arrangement is exhibited.

In the cranium now under consideration, a median vertical suture extended for a short distance downwards from the superior lamb-doidal angle, and ended inferiorly in an oval opening which reached as far as the occipital spine. Deficiencies in ossific formation so great as to occasion an absolute want of bony matter in this locality are evidently extremely rare, and but few cases occur in the records of Pathology. Dr John W. Ogle[2] has, indeed, described a case in which the cerebral part of the bone was perforated in the middle line by a foramen large enough to admit a probe, which communicated with a depression on the inner surface of the occipital bone in which an intra-cranial cyst was lodged; and he has briefly described[3] another specimen contained in the pathological depart-ment of the New Museum, Oxford, the particulars of which were communicated to him by Dr Acland, in which a foramen passed through the occipital tuberosity, and permitted a communication to take place between a venous capsule placed beneath the skin of the back of the scalp and the Torcular Herophili. It is interesting, also, to note that, both in Dr Ogle's first case and in the one now described, the opening in the bone occurred in conjunction with cystic forma-tion in the locality, though in the former the cyst was intra-, in the latter extra-cranial, and it does not appear from Dr Maclagan's description of the case that the cyst had any communication with the interior of the skull.

The supra-spinous part of the occipital bone in which the opening occurred differs from the cerebellar portion, in that it is developed in the primordial membranous and not in the cartilaginous cranium. Embryologists by no means agree in their statements as to the number of ossific centres from which the cerebral part of the bone takes its rise, which is probably due to the circumstance that they by no means correspond in number in different individuals. But there can be no question that in this particular specimen two dis-tinct centres at least must have originally existed, one for each lateral half, and that the process of ossific formation had failed as the spicula approached the middle line. The defect had occurred in a greater degree below than above, so that whilst in the former a large oval aperture (filled up by a membrane in the recent state)

[1] Succenturiatus Anatomicus. Leyden.
[2] Trans. Path. Soc. London, vol. vi. p. 12; and more fully, with a figure, in a very elaborate article in the Brit. and For. Med.-Chir. Review, July 1865, p. 208.
[3] Med.-Chir. Rev., op. cit., p. 212. In the same paper he also describes, p. 233, another case of deficiency of the occipital bone accompanied with extra-cranial cystic growth.

resulted, in the latter the two halves of the bone had come in contact along a sutural line. This malformation may be compared, therefore, with that which occasions, in the spinal column, the deformity known as spina bifida.

Of the mode of production of the two large openings in the parietal bones, it may at first sight seem to be more difficult to give a satisfactory explanation, for they are not, like the occipital opening, due to a congenital deficiency in the middle line, they do not occur in the substance of a bone midway between centres of ossification originally distinct, but they are placed laterally, and each is situated at no great distance from the eminence which seemed to be the centre of ossification for the parietal bone in which it was present—between it, indeed, and the middle line. Their position is not very far removed from the locality in which the parietal foramina are usually met with in ordinary well-formed skulls, and I think it not improbable that they may, though of course in an exaggerated form, be regarded as representing those venous foramina. Not that I wish it to be inferred that each opening transmitted a vein equal in size to itself, but that numerous small veins passed through the skull at this spot, and from their number had so far interfered with the ossification of the primordial membranous cranium in which the parietal bones are formed, that the osseous spicula had not advanced into it in these localities. I believe that this opinion as to their nature is borne out by the position of the openings, by the cribriform condition of the membranes which closed them in, and by the grooved state of the parietal bones internally, close to the inner ends of the openings, evidently for the reception of venous sinuses.

Variations in the size of the parietal foramina, within certain limits, are sufficiently familiar to all anatomists, and in passing through one's hands any large collection of skulls and skull-caps, one may find in some apertures so small as hardly to admit a bristle, whilst in others they will readily admit a good sized shot. But I am not aware that any case has been recorded in which a pair of openings, evidently congenital, of a size corresponding to those existing in this specimen had been met with.[1] Dr Humphry is, I think, the only anatomist who has described a parietal foramen approximating in size to either of these openings. In his treatise "On the Human Skeleton," p. 243, he states that in the Cambridge

---

[1] The openings which occasionally form in the parietal and frontal bones close to the groove for the superior longitudinal sinus in connexion with the growth of Pacchionian bodies must not be confounded with those treated of in the text. A little attention will at once serve to distinguish them, for in those occasioned by the enlarged "bodies," the inner table of the skull for some distance around the opening is atrophied and absorbed by the pressure of the hypertrophied glands. Hence the opening in the outer table is at the summit of a depression, often of considerable size, occasioned by the absorption of the inner table and diploe. A characteristic specimen of this form of perforation is exhibited by the skull-cap II-16, in the Anatomical Museum of the University of Edinburgh.

Museum is a skull which possesses a parietal venous aperture large enough to admit a finger.

Causes must have been in operation at a very early period in the life of this woman, which had led to various disturbances in the mode of ossific formation of those portions of the membranous cranium which corresponded to the areas of the future parietal bones. For in front of these enormously enlarged foramina the two parietal bones for some distance forward were so blended together that all trace of the sagittal suture had completely disappeared, and though vestiges of this suture might be seen for a short distance behind the coronal margin on the external surface, yet internally no trace of it was present. From the age of the woman the obliteration cannot be regarded as senile, but must have occurred at an early period of the growth of the bones.

Hence it would seem that the skull exhibits in its different parts two very opposite conditions of the ossific process. In the occipital and parietal apertures it presents gaps of considerable size in the bony vault in localities in which the osseous plates ought to be entirely, or almost entirely, continuous, whilst in the anterior part of the parietal region the coalescence of the two bones at so young an age indicates an exuberance in the osseous formation in that locality, and this excess of bony growth in the anterior part of the parietal region is still further exemplified by the beak which projected forward from the right parietal bone into the frontal, and which showed that the former bone had appropriated the whole or the greater part of the anterior half of the anterior fontanelle in the course of its formation.

In the description of the skull-cap, it was stated that a suture passed directly inwards from the inner end of the left parietal opening, which joined the unobliterated sagittal suture in the posterior third of the region. This suture, and the opening with which it was continuous, indicated a partial division of the left parietal bone into an anterior and posterior portion,—a mode of sub-division which, except perhaps in some hydrocephalic crania, has not been recorded, I believe, in the human skull. In enlarged hydrocephalic skulls, indeed, not only may there be an increase in the number of the osseous elements of the skull through unusual formation of Wormian bones in the membrane separating the proper cranial bones from each other, but these latter may in some exceptional cases be subdivided into smaller portions. A remarkable case, exhibiting a tendency to subdivision of a parietal bone into an anterior and posterior portion, was recorded many years ago by Ekmark, in the skull of a woman, æt. 45, who died from internal hydrocephalus.[1] The two parietal bones were blended together

[1] This case is alluded to by Meckel in his Handbuch der Pathologischen Anatomie, vol. i. pp. 332, 336. He ascribes it to Aurivillius; but it has been recorded at length by Ekmark, in his Inaugural Dissertation at Upsala, 1763. See Sandifort's Thesaurus Dissertationum, vol. ii. p. 325: Rotterdam, 1769.

anteriorly, but posteriorly they were separated by a sagittal suture, from which another suture ran obliquely backwards into the right bone.    From the lambdoidal suture a suture extended on each side into the corresponding parietal bone; that on the right side possessed the same direction as the one proceeding from the sagittal suture, and through their agency a portion of this bone was in a great measure separated from the rest.

In the skull-cap I have described it is not to be supposed that the portions of the left parietal bone lying anterior and posterior to the suture arose from distinct centres, but only that the spicula proceeding from the single parietal tuber had failed to be completely blended along this sutural line.    Hence it is to be distinguished from those cases in which a suture has been seen extending through the parietal bone on one or both sides in the antero-posterior direction from the coronal to the lambdoidal suture, and separating it completely into two parts, an upper and a lower.    As examples of this remarkable arrangement, I may refer to an adult skull figured by Tarin,[1] to which Winslow had directed his attention, in which the left parietal bone was so divided : to the skull of an infant described by Van Doeveren,[2] presenting a similar deformity : to the cranium of an adult male in the Gotthard Collection, described by Meckel[3] and Sömmering,[4] and figured also by the latter, in which both parietal bones were divided into an upper and a lower part : to an adult skull figured by Lucae,[5] in which the right parietal bone exhibited the separation : to a fœtal cranium described by Gruber,[6] in which the left parietal bone was divided : to an adult male skull described by Welcker,[7] in which the left parietal bone was separated into two parts : and to a fœtal skull figured and described by Dr Traquair,[8] in which the division occurred on the right side.    In none of these cases, which seem to be all that have as yet been recorded, did the skull appear to be hydrocephalic.    In all the cases in which a precise account of the arrangement is recorded, it is stated that the parietal eminence is situated in the upper of the two divisions, which is to be regarded therefore as the proper parietal bone, whilst the lower more scale-like portion is probably nothing more than an exaggerated representative of that scale-like triquetral bone which one sometimes finds occurring in the squamous suture.

In conclusion, it may be stated as a general result, arrived at after an examination of a large number of crania, that variations in the mode of construction of the bones of the skull-cap, i.e., of those

[1] Osteographie, Plate 5.    Paris, 1753.
[2] Specimen Observ. Academie., p. 195.
[3] Handbuch der Path. Anatomie, vol. i. p. 337.    1812.
[4] Zeitschrift für Physiologie, vol. ii. p. 1.    Pl. 1.    1826.
[5] Op. cit.    Taf. xi.
[6] Mem. de l'Acad. de St Petersburgh, vol. ii. No. 2, p. 2, 1859.
[7] Untersuch. über Menschlichen Schädels, p. 108.    1862.
[8] Natural History Review, January 1863, p. 132.

developed in membrane, are apparently of more frequent occurrence than in the bones of the basis cranii, which are developed in cartilage. This is probably due to the circumstance that the areas of the different bones are less precisely defined, and that the process of ossification is more liable to disturbance in the former than in the latter. The modifications in arrangement are especially apt to occur along the lines of apposition of adjacent osseous areas, i. e., along sutural lines, or along the margins of junction of the subdivisions of a bone proceeding from distinct centres; and it is in these localities that the anatomist so frequently meets with Wormian or triquetral bones, or occasionally with a beak projecting from one bone into an adjacent one, or with the not unfrequent blending of one bone with another along the sutural margins.

---

Art. V.—*On Paralysis of the Palate in Facial Palsy.* By William R. Sanders, M.D., F.R.C.P.E., Physician to the Royal Infirmary of Edinburgh, Lecturer on Physiology and on Clinical Medicine.

On the subject of unilateral paralysis of the velum palati, in connexion with paralysis of the portio dura, the statements of authors are curiously contradictory. Some writers maintain that the palate is never affected, or that its distortion is an accidental coincidence, independent of the lesion of the facial nerve. The majority, however, recognise the affection; but they describe different, or even opposite conditions of the paralyzed part. Thus, while the general opinion holds that the hemiplegic palate and uvula are, like the features of the face, drawn to the sound side, on the contrary, some of the best authorities record no lateral displacement of the palate, and describe the point of the uvula as directed not from, but to the paralyzed side. It further appears, that amidst these discrepancies, the real or principal deviation of the palate, that, namely, which occurs in the vertical direction, has been overlooked. It is not noticed, so far as I can find after considerable search, except by one author, whose statements have been unknown to, or disregarded by, succeeding writers. The mechanism of the distortion of the palate has also been left unexplained. Under these circumstances, I trust that some observations which I recently had an opportunity of making on cases of this kind, may not be without interest as contributing to the elucidation of the subject.

I shall give the first case, with the necessary details, and shall make it the subject of discussion; to the other cases I shall only refer briefly by way of supplement.

Case 1.[1]—William W., æt. 28, formerly a butcher, latterly a sol-

---

[1] Extracted from the report of the case by Dr James Carmichael, Resident Physician in the Royal Infirmary for the wards under my charge.

dier, was admitted into the Edinburgh Royal Infirmary, on 29th March 1865, affected with facial palsy of the right side. The attack had lasted seven weeks before admission; the predisposing causes were dissipated habits, syphilis in 1861, rheumatism in 1862; the exciting cause was, cold. The paralysis was nearly absolute, and was accompanied by deafness of the right ear. It had begun with ringing in the ear, and pain on that side of the head; and the paralysis was preceded by visible twitches of the facial muscles, before they lost their power. The usual one-sided deformity of the face,—the smooth half of the brow, the wide-open unwinking eye, the hanging cheek, the flattened nostril, the lowered angle of the mouth, dragging of the features of the opposite side, etc.,—was well marked, and was as usual greatly exaggerated by laughing, etc. There was total absence of voluntary motor power over all the affected muscles, including the buccinator, which was completely paralyzed, and the contractility of the palsied muscles under the stimulus of galvanism was also lost. The sensibility, and the movements of mastication, and the motions of the eyeball were unaffected. There could be no doubt that it was a case of peripheral paralysis of the right portio dura; the trunk of the nerve and not the brain being the seat of the paralyzing lesion.
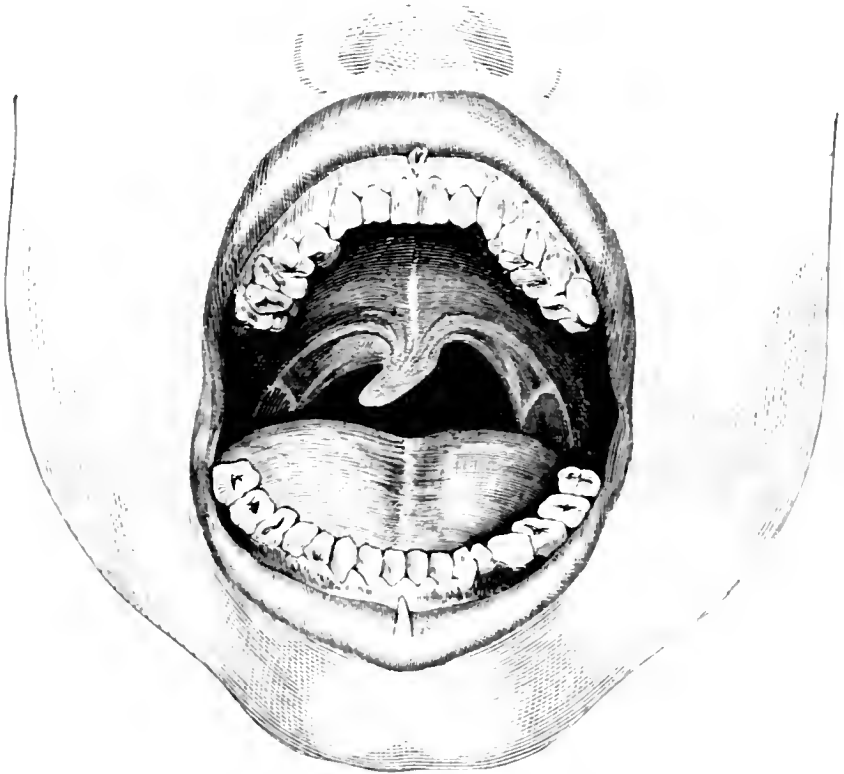


*Irregularity of the Soft Palate.*—In this patient the condition of the palate attracted particular attention, and a drawing of it, made by my friend Dr Banks at my request, is copied in the annexed woodcut.

When the mouth is opened wide, and the tongue depressed, the arches of the soft palate are seen to be unequal on the two sides. The soft palate hangs further down on the right or paralyzed side, so that the free inferior margin of the velum palati is on a lower level on the right side than the left. In consequence of this pendent condition of half the velum, the right palatine arch is lower, and it is also narrower than the left, and is less vaulted or arched. Accordingly, the free margin of the right posterior pillar (or arch), descends nearly straight, or with only a slight curve, downwards and outwards to the pharynx, while the left one, which retains the natural shape, turns upwards from the uvula, then curves downwards and outwards to the pharynx. The right arch may be compared to a narrow gothic and the left to a somewhat larger and higher roman arch.

On careful examination it is observed that this inequality affects the posterior palatine arches chiefly, almost exclusively ; the anterior palatine arches are nearly equal on the two sides ; the right is a little lower, but the difference is inconsiderable. Further, it is especially noticed that there is no decided lateral dragging or displacement of the velum palati (as has been generally described), for the raphe or middle line of the soft palate is straight and in the same continuous line with that of the hard palate.

The uvula presents a curious deviation ; it projects somewhat forwards, and is directed obliquely from left to right, the tip pointing to the right or paralyzed side ; its base is in the middle line, or perhaps a little drawn to the left or sound side. The uvula is also seen to be curved, especially near its root, the convexity being on the left side ; and owing to the velum palati being lower on the right side, the uvula appears unequal, presenting a long left side, and a short right side.

This irregularity of the velum, which is perceived when it is at rest, remains the same during the upward and downward movements of the palate, but it is not notably increased by them. The soft palate can be raised as a whole without difficulty either spontaneously when a full breath is drawn, or when it is irritated mechanically. It is also worth observing that, when the uvula contracts, it becomes straight and vertical, before it is withdrawn upwards within itself. The posterior arches of the palate, i.e., the palato-pharyngei muscles, are quite unimpaired in their action ; they contract vigorously and equally on both sides, and when the pharynx is touched or irritated they close inwards from the sides with the usual curtain-like movement. When the palato-pharyngei muscles begin to act, the inequality of the palate is at first slightly increased ; when they are in full action it ceases to be observed ; it reappears again when they relax.

It is important to note that this state of the palate gave rise to no symptoms, there was no impairment of speech (no nasal twang), nor of swallowing.

The general result of these observations may be thus stated : while the left side of the soft palate is natural in shape, the right

side, corresponding to the facial paralysis, presents a lowering or vertical relaxation of the soft palate, which reduces the height, diminishes the curve, and narrows the width of the posterior palatine arch of that side ; there is no lateral displacement of the velum palati ; the uvula is placed obliquely, and its point is turned to the paralyzed side.[1]

*Mechanism of the Deviation of the Palate.*—To understand how this irregularity is produced, it is necessary to call to recollection the principal muscles of the soft palate, and the change in shape which their respective actions produce. These muscles are, 1*st*, The elevators (levatores palati), which lift the palate upwards, and consequently heighten the palatine arches ; 2*d*, The tensors (circumflexi palati), which stretch the palate by pulling it outwards on each side, the opposite muscles antagonizing one another on either side of the middle line ; 3*d*, The depressors (palato-pharyngei),which, acting on the posterior pillars of the palate, draw the velum downwards, and lower the posterior palatine arches. The levator palati and palato-pharyngei are, therefore, antagonistic ; the former raise the velum and heighten the palatine arches,—the latter pull down the velum and lower and narrow the palatine arches. It is plain that, in the deviation of the palate in W.'s case above described, the right palato-pharyngeus muscle is not at fault, since the paralysis of a depressor muscle, by leaving unimpeded the action of the levator, would cause the palatine arch to be raised higher on the palsied side, the reverse of what is observed. Nor can the right tensor (circumflexus) muscle be paralyzed, for the inaction of one tensor would allow the soft palate to be dragged sideways by the opposite tensor ; the middle line of the palate would be displaced towards the sound side, while in W.'s case it was carefully observed that the vertical raphe of the palate remained in the mesial plane without any obliquity or lateral deviation. The effect, moreover, of paralysis of one of the tensors (circumflexus) would be to produce distortion of both palatine arches, since both of these, and the uvula which separates them would be drawn forcibly to the sound side. We arrive, therefore, by way of exclusion, at what anatomical considerations would suggest, that the deformity observed is due to paralysis of the right levator palati alone. This occasions the drooping or lowering of the velum on the paralyzed side by the relaxation of those fibres which should raise it, thus leaving without antagonism the action of the palato-pharyngeus muscle, which pulls it downwards. Inaction of the levator, excessive because unopposed, action of the depressor (palato-pharyngeus or posterior pillar), such is the mode of production of the lowering of the velum palati on the paralyzed side, while its normal level is maintained in the sound side.

It should be remarked, however, that this vertical relaxation,

[1] I had an opportunity of showing the palate of this patient to Professors Goodsir, Gairdner, and Struthers, Dr Warburton Begbie, Mr Turner, etc., and to the clinical class.

or vertical paralysis of the velum, seemed to be chiefly confined to its lower part, situated between the anterior palatine arches in front, and the posterior palatine arch, or lower free margin of the velum; for it was observed that the anterior arches were nearly of equal height, and that the elevation of the soft palate, as a whole, was not hindered. It follows, therefore, both that the palato-glossi muscles, which form the anterior pillars, were unaffected, and also that the paralysis of the levator palati was partial and incomplete. The paralysis appeared to affect especially the intrinsic fibres of the velum, if we may so term them; i.e., the fibres spread out in a vertical direction within the velum itself. The rest of the muscle (extrinsic) which raises the palate *en masse*, was not materially impaired in its action. It may be alleged that one levator palati alone would suffice to raise the whole velum if the other were paralyzed, but in that case we should expect the distortion of the palate to be greatly increased during its action; the palate being drawn diagonally upwards to the sound side. As this did not occur, it appears, as already stated, that on the paralyzed side the levator acted in raising the palate as a whole, but the paralysis of certain of its fibres, or perhaps an incomplete paralysis (paresis) of the whole, permitted the vertical descent or lowering of one half of the velum below the level of the opposite half. That a more decided paralysis might, have impaired, or even altogether prevented the upward movements of the right half of the palate is proved by cases to be afterwards mentioned.

The limitation of the paralysis to the levator palati, to the exclusion of the circumflexus and palato-pharyngei muscles, corresponds with our most accurate information in regard to the nervous supply of these muscles, and is calculated to throw additional light upon it. There has been very great difficulty in determining both anatomically and physiologically the particular distribution of the nerves in the palate, owing to the circumstance that it is supplied by branches from the fifth, the seventh, and the eighth pairs of cranial nerves. Great discrepancies will, accordingly, be found among writers on the subject. The best authorities are agreed, however, that the circumflexus palati or tensor muscle is supplied, not by the seventh or eighth, but by the motor branch of the fifth only. We should not, therefore, expect this muscle to be paralyzed in facial paralysis when the seventh alone is involved. This fact is specially interesting, because in the account usually given of this one-sided paralysis of the palate, the velum is described as being dragged to the sound side, i.e., a lateral paralysis and horizontal deviation of the palate are stated to occur. It throws some suspicion on the accuracy of this account, when we learn that such a distortion, if it really occurred, would indicate paralysis of the fifth and not of the seventh pair, to which it is attributed. I am disposed to think that there has been some haste and carelessness of observation or of description on this point. It seems as if observers,

having noticed the paralysis of the face to produce a lateral distortion, had taken for granted that the same kind of horizontal displacement would occur in the palate. However this may be, in W.'s case it was quite apparent that the distortion of the palate was vertical, not lateral; from above downwards, not from side to side; at right angles to the deviation of the face, not parallel with it. The absence of horizontal displacement in W.'s case, therefore, quite coincides with the anatomical and physiological statement, that the tensor of the palate (circumflexus) is supplied by the fifth, and not by the seventh, and is not involved in facial paralysis.

The palato-pharyngei muscles have been stated by authors to be supplied by both the seventh and the eighth pairs, the latter chiefly. So far as W.'s case may be allowed in evidence, it supports the view that these muscles derive their motor supply from the eighth pair, but it affords no indication that they receive any motor supply from the seventh, or suffer any paralysis from lesion of that nerve.

The levator palati, on the other hand, derives its chief and, according to some, its only motor innervation from the seventh pair; and it will be remarked that this is in accordance with the vertical form of paralysis of the palate met with in the case of W., and in other similar cases to which I shall afterwards allude. My own cases do not prove that the levator derives its only supply from the seventh, because in all of them the paralysis was incomplete; but they show, at least, that the levator derives a chief part of its motor influence from the facial, and suffers paralysis when that is affected.

*Obliquity of the Uvula.*—The position of the uvula in W.'s case seemed at first anomalous, and difficult or indeed impossible to account for on the theory of the unilateral paralysis of its own muscular fibres. For, in palsy of the right azygos uvulæ, we should expect the uvula to be drawn by the opposite azygos to the sound (left) side, and its tip pointed to the left instead of slanting in exactly the opposite direction, with its apex to the right or paralyzed side. The reason of this unexpected position is, I believe, to be sought in the circumstance, that the obliquity of the uvula is not due to its own muscles, but to the action of the right palato-pharyngeus muscle, which pulls the uvula to its own side. Leaving its own intrinsic (azygos) muscles out of account, the uvula may be considered as suspended vertically by its root from the middle of the anterior arches of the soft palate, and it is kept straight by the action of the different muscles of the palate (levators, tensors, depressors) but more especially by the palato-pharyngei muscles, which pull upon it on each side near its base. So long as the palato-pharyngei pull with equal force and opposite the same point the uvula will remain vertical and straight. In W.'s case, as already stated, the palato-pharyngei were unaffected by paralysis, and were therefore probably of equal power on each side. But in conse-

quence of paralysis of the right levator palati, it is evident that
the right palato-pharyngeus muscle, not being antagonized, would
act with greater effect on the uvula than the left palato-pharyngeus,
which is opposed by the upward traction of the left levator;
consequently the right palato-pharyngeus draws the uvula to its
own side, *i.e.*, the right, where the levator is paralyzed. Further,
since the inferior fibres of the palato-pharyngeus are inserted into
the uvula a little below the level of its base or origin from the
anterior pillars of the palate, their contraction would produce the
obliquity and curvature of the uvula observed in the woodcut.
And not only is the action of the right palato-pharyngeus muscle
stronger, because less antagonized than its fellow, but also from
the vertical relaxation of the velum on that side, its fibres may
be said to act in a more direct line (*i.e.*, with less vaulting or arch),
and at a point lower down on the right side of the uvula than the
corresponding palato-pharyngeus muscle does on the other side. On
comparing the long convex left side with the shorter right side of the
uvula (see woodcut), it is evident that the lower border of the right
posterior palatine arch is much nearer to the apex of the uvula
than that of the opposite arch. If it be admitted, then, that the
right palato-pharyngeus pulls on the side of the uvula more
directly and nearer the apex than the left palato-pharyngeus,
the oblique position, the curvature of the uvula, and the direction
of its tip to the right side, are readily understood. The peculiar
curvature of the uvula, of which the convexity is greatest opposite
the insertion of the lower border of the right palatine arch, corre-
sponds with the view just stated. There is therefore every reason
to suppose that the peculiar oblique position of the uvula is to be
explained by the greater action of the right palato-pharyngeus,
which drags and bends the uvula to its own side.

As a corollary to this explanation, it must be apparent that the
particular position of the uvula in paralysis of the levator palati
will depend on the greater or less power of its own intrinsic muscles
(azygos) to oppose the traction of the palato-pharyngeus; and it
will also depend on the length of the uvula, and especially on the
greater or less distance from its base or root at which the palato-
pharyngei muscles are inserted. In these respects, different
palates vary greatly. In W.'s case, the uvula was of moderate
length, and there was considerable extent of velum between the
anterior and posterior arches, so that the pull of the palato-
pharyngei muscles on the uvula on each side was considerable.
In other palates, where the extent of velum between the arches is
small, the traction of the palato-pharyngei on the uvula will be less.
It happens, consequently, that the position of the uvula is not the
same in all cases of hemiplegia of the palate as it was in W.'s case.
Sometimes it is vertical, sometimes it is oblique with the point
directed to the sound side. In the latter case, the sound azygos
probably draws it to its own side. Finally, there are congenital

or acquired obliquities of the uvula, to which I shall afterwards refer, in which the uvula of the normal palate is curved, and the apex is turned to one side.

In W.'s case, were the azygos muscles of the uvula unaffected? Being supplied, like the levator palati, by the seventh pair, we should have expected the right one to be paralyzed. The distortion due, as explained, to the palato-pharyngeus at first entirely concealed the condition of the azygos muscles; but on careful examination, it will be perceived that there is some probability of the paralysis of the right azygos, for it was observed that when the uvula contracted, either spontaneously or under irritation, it became straight, i.e., the obliquity was counteracted in the process of shortening. The change from oblique to straight could be produced only by the greater action of the azygos on the long or convex left side, and the comparatively feeble action, i.e., paralysis, of the opposite azygos on the right side. It will be remarked that here also the paralysis was not complete, for the uvula as a whole was capable of powerful contraction, and indeed was so completely shortened and withdrawn within itself during full contraction that only the apex remained visible as a round papilla.

*Recorded Observations of Irregular Palate in Facial Paralysis.*—I mentioned at the outset that some authors look upon the distortion of the palate in facial palsy as a very rare lesion, and probably a coincidence merely (Debrou, Todd, etc.). On this view, which deserves consideration, I shall make some observations before concluding, and pass it over at present. Our best medical text-books, so far as I know, omit all mention of the affection of the palate. On the other hand, all authors, since the time of Sir Charles Bell, who have specially treated of paralysis of the seventh pair, take notice of its effect on the palate. The account given by authors,[1] down to the most recent date, is with two notable exceptions always the same, in fact nearly a repetition in the same words. It may be assumed to represent the prevailing opinion, and was the notion that I had been accustomed to hold till the observation of W.'s case drew my attention specially to the subject. The description is, that the palate and uvula are distorted laterally, drawn transversely across towards the sound or unparalyzed side. Thus Todd says (although he immediately afterwards states that he looks upon the occurrence as probably a coincidence), " In some instances the velum of the palate participates in the paralysis" (of the facial nerve), " and when you look into the patient's throat you will find the uvula inclining

---

[1] See Longet, Syst. Nerveux (1842), ii. 450, 466, who quotes the observations of Montault, Diday, Seguin, and Debrou; also his Physiologie, ii. 556, 2d Edit. 1860. Montault is said first to have noticed the deviation of the uvula in 1831. Shaw observed two cases of partial paralysis of the soft palate, where " there was the peculiarity of *the uvula being drawn towards the paralyzed side* during inspiration,"—quoted in Baly's translation of Müller's Phys., 2d Ed., i. 709. On turning up the reference there given, I could not find Shaw's paper.

away from the paralyzed side, and the velum drawn to the sound side." Trousseau (Clin. Med., 2d ed., 1865, ii. pp. 258 and 265) is similar, but more detailed in his description; in a case of facial paralysis of the right side, he says: "En examinant l'arrière-bouche, il est évident que la double arcade formée par les piliers du voile du palais et la luette n'a pas de chaque côté la forme régulière qu'elle affecte normalement.  A gauche, cette arcade est plus étroite, ce qui indique une déviation de la luette de ce côté." And in another case, " la luette était entrainée à gauche (la para-lysie faciale occupait le côté droit), de telle sorte que la demi-arcade circonscrite entre elle et les piliers du voile du palais était sensible-ment beaucoup plus étroite, que la demi-arcade droite."  It will be remarked that Trousseau says nothing of the difference of height of the palatine arches, but describes the uvula as dragged to the sound side, and the palatine arch as being in consequence narrowed on that side (exactly the reverse of what existed in W.'s case). In the latest standard German work with which I am acquainted the description is similar.  Hasse (in Virchow's Handbuch der Speciel. Pathologie u. Therapie, band iv. p. 342,) says the velum palati and the palatine arches are drawn over, and the uvula turned, towards the sound side (" das Gaumen-segel, die Gaumen-bögen verzogen, das Zäpfen nach der gesunden seite hin gewendet;" and, p. 347, " die Verziehung der uvula nach der gesunden seite," etc.).  Such is the general description.

The two exceptional authors above alluded to, who give a different account, are Romberg and Davaine; and it is surprising, when we consider Romberg's reputation as one of the first clinical authorities on nervous diseases, that their statements have been unnoticed, not only in England, but by such eminent systematic writers of their own respective countries as Hasse and Trousseau.  Romberg is very precise in his statements, and they are very interesting: he appears, how-ever, to have observed the condition of the uvula only, and is silent regarding any lateral or vertical distortion of the velum or the palatine arches.  "The unilateral paralysis of the velum palati," he says, " seems to depend upon the loss of the conducting power of these nerve-fibres" (i.e., of the nervus petrosus superficialis major) " which gives the uvula a slanting direction, causing its point to be directed to the paralyzed side; it resumes its normal position after a cure has been effected."  He gives the particulars of four cases: in three the paralysis was on the left side, in which the uvula " had a slanting direction, was arched, and the tip pointed to the left:" in one case, the palsy was on the right side, and the " uvula was arched, and its tip turned to the right side."—(Rom-berg, Dis. of Nervous System, Syd. Soc.'s Transl., ii. 275–6.) Romberg, while explicitly stating the fact, offers no explanation of this peculiar position of the uvula,—so different from what might have been expected, and contrary to the direction usually described. Whether he considered the turning of the point towards the para-

lyzed side to be the effect of the base being drawn towards the opposite or sound side does not appear, since he makes no allusion to any lateral dragging of the velum or irregularity of the palatine arches.  His account, however, has the peculiar interest, that in all his cases a similar obliquity of the uvula existed to that noticed in W.'s case; and, further, that he ascertained the restoration of the uvula to its normal vertical position when the facial paralysis was cured.

The description by Davaine[1] deserves special notice, because it is the only one known to me which coincides entirely with what was observed in W.'s case.  He records only one case observed by himself of the unilateral paralysis of the palate; the remaining cases he quotes from Romberg and others.  In his special case, that of Édouard Arnoux, æt. 29, a patient in the Hôpital de la Charité, the subject of facial paralysis of the right side, the affection of the palate was so well marked as to give rise to the symptom of a slight nasal intonation in speaking.  His description is thus expressed:[2] " The velum palati is not regular; the arch formed by the right anterior pillar is less elevated than the left.  The posterior pillar of the same side (right) descends directly downwards, without being curved like that of the other side.  The uvula is bent like a bow; its point is directed forwards and towards the paralyzed side, while its base is carried a little towards the sound side.  The patient's voice is slightly nasal." (p. 170.)  After five weeks' treatment, the facial palsy having considerably diminished, the condition of the palate was as follows: " The uvula still presents a curve to the right, but the velum palati has recovered its regularity; the arches formed by the anterior pillars are equal on the two sides; the *right*[3] posterior pillar still falls somewhat.  The nasal voice (nasonnement) has very notably diminished."  In a case of facial paralysis on both sides, recorded by M. Davaine, the affection of the palate which he observed was more complete.  The velum was symmetrical (both sides being paralyzed), but hung down lower than usual; it was motionless and unable to raise itself, owing to the complete paralysis of both levators and of the azygos muscles.  There was in consequence a nasal sound of the voice, and some difficulty of deglutition.

Davaine directs special attention to the pendent condition of the velum, and ascribes it to paralysis of the levator palati, — an

---

[1] Mémoire sur la Paralysie générale ou partielle des deux nerfs de la Septième paire, in Mém. de la Soc. de Biologie for 1852, p. 164.  Paris, 1853. Also in Gaz. Méd.

[2] " Le voile du palais n'est pas régulier; l'arcade formée par le pilier antérieur droit est moins élevée que la gauche.  Le pilier postérieur du même côté droit tombe directement, sans s'incurver comme celui de l'autre côté.  La luette est recourbée en arc; sa pointe est dirigée en avant et vers le côté paralysé, tandisque sa base est un peu portée vers le côté sain.  .  .  .  La voix de ce malade est légèrement nasonnée."

[3] It is printed left (gauche) in the original, evidently *per incuriam*.

explanation which he confirms by the result of physiological experiments on animals. This part of his paper is of much interest and value. I do not find that he accounts for the position of the uvula; he merely mentions paralysis of the azygos (palato-staphylin); but this alone could not turn the uvula to the paralyzed side, but rather in the opposite direction. In noticing the varying accounts of observers, Davaine regrets that they had not paid so much attention to the state of the velum palati as they had done to the uvula; and in regard to the uvula, he supposes that the different statements of authors only appear contradictory because they have omitted to specify whether it was the base or the point of the uvula that was turned to or from the paralyzed side. But the expressions which authors have used, as well as the varying position of the uvula in different cases, render any such reconciliation of inconsistencies impossible.

I have referred to Davaine with some detail, 1st, because he is the only author known to me who discusses the deformities of the palate on accurate physiological grounds; 2d, because he appears to be the first or only author who has described the vertical relaxation of the velum (i.e., the paralysis of the levator palati only) in paralysis of the seventh pair; and, 3d, because he records the cure of this form of hemiplegia of the palate; the relaxed and lowered velum returning to its normal level, and the palatine arch recovering its proper height and curvature. I was not aware of Davaine's researches till very lately, sometime after my own observations had been made, and I have accordingly to correct the statement I made to the Medico-Chirurgical Society of Edinburgh, at their meeting of May last, when, in describing and remarking on W.'s case, I mentioned that, so far as I knew, this peculiar condition of the velum had not hitherto been described, and I referred to Romberg as the only author who had mentioned the position of the uvula as it appeared in W.'s case. My apology for unacquaintance with Davaine's views is, that, finding authors so accurate and well informed as Longet and Trousseau describe the deformity of the palate in the usual way, I did not think it probable that any important observations of their countrymen of a different purport had escaped notice.

The only other recorded case I shall allude to is one by Professor W. T. Gairdner (*Lancet*, i. 1861, p. 479) of paralysis of the portio dura on the left side. There does not appear to have been any irregularity of the soft palate in this case; but, that some degree of paralysis of the left levator palati and azygos existed is apparent from the description, that "the arches of the soft palate acted very differently under a reflex stimulus, the right or sound side being drawn upwards much more decidedly than the left, and the point of the uvula being always bent towards the sound side, while its base was maintained in the centre, or even inclined to the opposite direction."

*Recent Cases.*—Since W.'s case, I have had the opportunity of observing one, and obtaining from medical friends the account of two, recent cases of paralysis of the portio dura, in which the palate was affected, and presented the vertical relaxation or pendent condition on one side above noted.

CASE 2.—M. S., æt. 48, widow, out-patient at the Royal Infirmary:—Left facial palsy of three weeks' standing, due to cold. Facial distortion and affection of orbicularis palpebrarum as usual. The left posterior palatine arch was lower than the right, smaller and less arched. The uvula, which was very short, has its point directed rather towards the *sound* side. She is recovering rapidly, but the palate still presents some degree of the distortion above described.

CASE 3.—A female, æt. 50; paralysis of portio dura of the left side, commencing one week after fracture of the basis cranii. *Palate.*—Left side of arch lower and narrower than right. Tip of uvula directed towards the paralyzed side. Six weeks in hospital, dismissed with the paralysis persistent. (Communicated by Dr P. H. Watson, surgeon to the Edinburgh Royal Infirmary.)

CASE 4.—C. G., female, æt. 35, married; paralysis of portio dura of left side, of two weeks' standing. *Palate.*—Uvula oblique, the apex pointed to the left side; the posterior palatine arch was lower on the left side than on the right. There was no lateral displacement of the palate; the anterior arch was apparently normal. Patient got well of the facial palsy, and the palate and uvula returned to their natural position. (Communicated to me by Dr Coull Mackenzie, Resident Physician to the Chalmers Hospital, Edinburgh.)

*Natural Irregularities of the Palate and Uvula.*—The question again occurs, as it had occurred to the earlier observers, whether these deformities of the palate and uvula may not be congenital, and the coincidence with the facial palsy an accident. Soon after meeting with W.'s case, I examined the palates of patients of two wards, besides those of many students, without observing any marked anomaly. Since that time, now some months ago, my attention having been directed to the subject, I have occasionally, and latterly not unfrequently, fallen in with anomalies of the palate. I have found that curved uvulæ with the tip turned to one or other side, while the base remains in the middle line, or is slightly drawn to the opposite side, are by no means rare as a natural formation. Consequently, I should not attach any value to an oblique or curved position of the uvula, unless attended by the pendent half of the velum, or unless I observed it becoming straight as the facial palsy diminished. On the other hand, the vertical relaxation of one-half of the velum, with the consequent lowering and lessened curve of the palatine arch, appears to me to be distinctive of paralysis of the levator palate of that side.

The histories of cases in which the distortion of the palate has disappeared, in proportion as the facial palsy has approached a cure, are quite conclusive as to the real existence of the hemi-

plegia of the palate, and of· its dependence on paralysis of the portia dura. I regret that I cannot give the conclusion of W.'s case, as he absconded from hospital at the time of receiving his pension, for the purpose of indulging in dissipation, and I have not seen him since; the appearance of the face and of the palate had not materially changed when he left.[1]

*Prognosis and Treatment.*—The ·implication of the palate proves that the paralyzing lesion has affected the nerve trunk of the portio dura at or above the origin of the great superficial petrosal nerve, which goes to the palate : that is, in the first portion of the Fallopian canal. In consequence of the disease being seated so high upwards in the course of the nerve trunk, physicians who observed the affection of the palate were apprehensive that such cases would prove more dangerous and more untractable than the more ordinary forms of peripheral paralysis of the face, in which the palate is not affected. Experience has shown, however, that such fears are groundless. The cases in which the palate is touched are not more serious nor apparently more difficult of cure than others. They follow the general rule—that the paralysis itself is without danger, and that the whole risk is from the cause producing it. If it is due to cold, or syphilis, etc., a cure may be expected ; but if there is a tumour, or caries of the bone, etc., the prognosis is correspondingly grave. In a word, the affection of the palate does not necessarily add to the danger of a case or diminish its chance of cure. The treatment by blisters, galvanism, etc., is the most efficacious in this as in other forms of facial palsy.

*Conclusions.*—1st, That in paralysis of the palate due to lesion of the portio dura, the levator palati and azygos uvulæ are the only muscles affected ; the other muscles of the palate, viz., the circum-flexi, the palato-glossi, and palato-pharyngei are not paralyzed, nor impaired in their actions.

2d, That, consequently, the true form of hemiplegia of the palate, in lesion of the portio dura, is partial, and consists of a vertical relaxation or lowering of the corresponding half of the velum palati, with diminished height and curvature of the posterior palatine arch on the paralyzed side. This condition is due to the paralysis of the levator palati.

3d, That there is reason to doubt the accuracy of the description usually received of the horizontal displacement of the palate and uvula to the sound side, when affected in facial palsy. Such deviation would imply paralysis· of the circumflexus palati (and probably also of the palato-pharyngeus), which does not receive its nervous supply from the portio dura, and cannot be palsied by its lesion.

[1] I may mention that, in the course of these observations, I have met with an instance of bifid uvula. The uvula was rather long, about twice the natural breadth, and was divided in the middle from the free extremity upwards for a couple of lines, so as to present two distinct points each of the ordinary size and shape.

*4th*, That the lateral distortion of the palate due to paralysis of the portio dura, which may be expected sometimes to occur, would take place, not transversely, but in the diagonal direction upwards, and to the sound side, *i.e.*, in the line of action of the sound levator palati, and would be accompanied by the lowering of the palatine arch on the paralyzed side, as above described.

*5th*, It is probable that the absence of this oblique lateral distortion, both in a state of rest, and when the levator palati was in action, in the cases here recorded, was due to the minor degree of the paralysis, the balanced action of the unaffected muscles (circumflexi and palato-pharyngei) being sufficient to maintain the raphe of the velum in the mesial plane, in spite of the disturbance of equilibrium produced by the weakness (paresis) of one of the levators.

*6th*, The existence of this diagonal distortion upwards to the sound side, in certain cases, may have given rise to the common description of lateral deviation of the palate in facial palsy, although the phenomena presented do not correspond at all accurately with that description.[1]

*7th*, That the position of the uvula varies frequently, both in the natural and the hemiplegic palate, being twisted sometimes to the right, sometimes to the left, the point directed sometimes to the paralyzed, and sometimes to the sound side. Curvature of the uvula, taken by itself, is, therefore, an uncertain sign, and does not possess the diagnostic importance which has been ascribed to it.

*8th*, If the partial hemiplegia above described, due to paralysis of the levator, be looked for, instead of the lateral displacement from paralysis of all the muscles of one side of the palate, which the common description leads us to expect, but which I believe never occurs from lesion of the portio dura alone, it will be found that unilateral paralysis of the velum in facial palsy is by no means so rare as has been generally supposed.

*9th*, That the prognosis is not necessarily rendered more unfavourable in facial palsy when the palate is implicated.

---

[1] A case illustrative of the phenomena and conditions of this diagonal distortion has just occurred to me, too late for insertion in this paper. As the case is in several respects important, I shall take an early opportunity of drawing attention to it.

# Part Second.

## REVIEWS.

*Surgical Experiences: The Substance of Clinical Lectures.*   By
Samuel Solly, F.R.S., Senior Surgeon to St Thomas's Hospital,
Member of Council, and late Professor of Anatomy and Surgery
to the Royal College of Surgeons of England, etc.    8vo, pp.
656.   London: Robert Hardwicke: 1865.

The surgical experience of a Senior Surgeon to a great London
hospital must of necessity be large, and a faithful record of it should
convey much instruction.    Such a position, while giving great
opportunities for study and practice, carries with it the burden of
a great responsibility, swaying as it does with a very peculiar
influence the theory and practice of hundreds of old pupils of the
school.   To many such "St Thomas men," a book by "Old Solly,"
as we have often heard him called with affectionate irreverence,
will have the force of a Delphic oracle, while to all of us who have
studied "Solly on the Brain," the name on the title-page is a
guarantee that the book will prove a record of much faithful and
practical work for surgery.

As its name implies, this book consists of selections from the
clinical instruction delivered by Mr Solly at St Thomas's Hospital,
during a period of more than twenty years.   Two methods of
arranging such a selection present themselves,—either the chrono-
logical, in which each year would furnish its own cases and the
accompanying remarks,—or the systematic, in which, under their
different heads, the various cases and commentaries are grouped
into one chapter.   The first plan involves a risk of repetition and
complexity of arrangement, the latter has the disadvantage that it
is apt to condense clinical reports of cases into mere systematic
treatises on disease.   Mr Solly has chosen the latter, but has
succeeded in avoiding in great measure the risk alluded to.

The table of contents shows an attempt at arrangement of the
subjects.   The first division, pages 1–204 inclusive, embraces
Diseases of Spine, Injuries of Head, Amaurosis, Inflammation
of Dura Mater, Concussion, Delirium Tremens, Epilepsy, and
Paralysis,—most of which, though usually treated by the surgeon,
are fairly in the domain of what Mr Solly is fond of calling
"Medical Surgery."   Against these foes the phial and the lancet
—not the knife and saw—are the weapons used.   In the perusal
of these 200 pages, while finding much to admire in the diag-
nosis, and still more in the skilfulness of the treatment, one
great broad fact at once strikes a northern reader, *i.e.*, the enormous

and, in our eyes, fearfully dangerous use of mercury. Mercury as a purgative, mercury as an alterative, mercury to salivate, calomel, blue-pill, mercury and chalk, iodide and biniodide of mercury,—it seems the sheet-anchor, the summum remedium, the ne plus ultra.

This is no exaggeration. Of the thirty-three cases which are the texts for the clinical lecturers comprised in the 200 pages, fifteen were salivated once or oftener, thirteen others took mercury either as a purgative or alterative, while in the treatment of the remaining five we are not told that any mercury was given. It is a significant enough fact, that three out of the five who did not take it were treated after the year 1862, among the latest in date of the cases. Again, to quote verbatim from a lecture dated 1850:—"If you have to combat local inflammation, combat it by local depletion and mercury. Yes, my friends, mercury; this is indeed our sheet-anchor in all cases of inflammation and extravasation" (p. 131). We might quote other panegyrics quite as strong; thus, at p. 15, the purgative in cases of acute spine disease is to contain a greater or less quantity of mercury, according to the nature of the excreta. At p. 36, the use of mercury in small doses for periods of several months is recommended for chronic diseases of the retina.

Believing as we do that even for strong men a course of mercury is dangerous, that for strumous patients it is a slow but deadly poison, and that for many venereal cases the remedy is worse than the disease, and not unfrequently is the real cause of the tertiary symptoms of which the disease gets the credit, we cannot but enter a protest against such a very free use of this drug, which almost recalls the old days of foul wards, in which every patient must spit his daily pint or gallon, and where the seeds were laid which soon ripened into such fruit as mercurial ulcers, rodent ulcers of the face, sclerosis of the tibiæ, and corona veneris. That Mr Solly gives it with a careful cautious hand we do not doubt; but if fifteen out of thirty-three cases were salivated in his practice, though they may have been salivated wisely and moderately, who can tell the mischief which may be done by followers who imitate the therapeutics and wield the dangerous weapon which in a master's hand may be comparatively innocuous.

Between the lectures on Medical Surgery and those more specially connected with Operative Surgery are inserted three lectures on a more strictly medical subject,—Scrivener's Palsy, its clinical history, pathology, and treatment; Mr Solly supports the theory that it depends on local structural changes in the spinal cord.

Diseases of Bones and Joints, chiefly the latter, occupy the next large section of the book; in every chapter, almost in every case, there is some interesting observation, and the flavour of that ripeness that experience only can give. The knee-joint as usual receives the largest share of attention. Mr Solly's success in the treatment of strumous disease of this joint is very exceptional. He aims at anchylosis, and perseveres in trying for it without

reference to the length of time required, in preference to either excision or amputation of the thigh; and if the cases seen are numerous, the results are certainly very remarkable, for in 1854, after a surgeoncy of thirteen years, he was able to say regarding such a case,—

"I believe it is only the second time I have amputated a limb for disease of the knee-joint since I have been an officer of this hospital. In every other instance I have been able to procure anchylosis, though in one it required three years to accomplish it."

Again, in another clinical lecture, delivered in 1860, he speaks

"Of nine cases of diseased knee-joint, in which I believe I have succeeded in saving both limb and life without any operation, and one case where the same ultimate result has been obtained by the excision of the joint."

A very long period of treatment is found to be requisite in many of these cases. On this Mr Solly remarks,—

"There is one point which must of course strike you on listening to these details, viz., the length of time that some of them have been in the hospital. The power of retaining our patients in the hospital until a cure is effected is a privilege which can only be enjoyed in the old well-endowed hospitals,— and, for the sake of humanity, what a blessed privilege it is."

A very excellent chapter is devoted to the subject of injuries of the knee-joint without external wounds, such as, sprains with considerable laceration of ligaments; cases are given which illustrate the excessive danger both to life and limb of fractures into the knee-joint, from the frequency with which they pass on to suppuration. Along with these is recorded an interesting case of dislocation of the knee-joint with rupture of the popliteal artery, in which Mr Tyrrell tied the femoral soon after the accident, and the patient died of gangrene of the leg.

After discussing the diseases of, and operations on other joints without any great novelty, the subject of caries of the tarsal bones is discussed. For this disease Mr Solly advocates frequent and free applications of the gouge for the removal of diseased bone. Theoretically, and from the pathological point of view, such operations are unsuccessful, from the fact that once the tarsal bones are carious, the disease is so extremely apt to return; and we must add, that Mr Solly's cases do not in the least contradict this theoretical objection, though in one at least the portion of the tarsus removed had been very extensive.

A lecture is devoted to the subject of excision of the upper jaw, having for its text a case in which, in 1845, Mr Solly excised that bone for an "osteo-fibrous tumour." In this case he used the old plan of two incisions,—one from the inner angle of the eye to the upper lip near the middle line, and another from the zygoma to the outer angle of the mouth, and no notice is taken of any of the simpler methods. We cannot help feeling that it would have been more profitable, and at least as interesting, had Mr Solly given us more of his experience of this operation, and, instead of a long and

involved history of the invention of the operation, given a better account of how and for what it is now performed.

Mr Solly succeeds, however, in bringing thoroughly out the principles on which, and on which alone, this operation can either deserve or command success; these are, first, that suitable cases should be selected, the suitability depending in a very important degree on the possibility of removing *the whole* of the disease; and, second, that the operation should be performed through *healthy* parts, excising, if need be, the *whole* bone, not, as has been lately recommended, through diseased parts, chipping it away piecemeal by gouge or forceps. To imitate the practice of Acoluthus (1693) is at once to return to the dark ages, and to ignore one of the most cardinal principles of surgery.[1]

The lectures on hernia include a small but successful series of cases reported at considerable length. On the question of opening the sac, Mr Solly says :—

"Do not open the sac if you can avoid it, but divide the structure external to it. . . . The danger of the operation is much less if you do not expose the intestine. . . . After having divided the stricture, you gently press the sac between the finger and thumb, and if the contents all *pop* back, if I may so express it, leaving the sac quite empty in your fingers, you may be satisfied with the result; but if you do not empty the sac, feeling that something remains behind which you cannot get rid of, you should then open it to discover the cause. Most probably you will find an adherent intestine or portion of omentum; or if the contents of the sac suddenly give way, and the sac feels soft and not empty, you may apprehend a gangrenous intestine, and then you must open the sac."

The next chapter consists of the paper on the remedy of intestinal obstructions in certain cases, by colotomy in the left lumbar region. Three cases are given in which Mr Solly performed the operation. They lived two months and a half, thirty-nine days, and twenty-one days respectively; but in each case the operation seems to have so far at least prolonged life, and certainly given very considerable relief. They may be added as a supplement to the cases tabulated by Mr Cæsar Hawkins in the 35th volume of the Medico-Chirurgical Transactions, and the additional list given by Mr Curling in the third edition of his work on Diseases of the Rectum.

A case of ruptured bladder next affords Mr Solly a text for a disquisition on this painfully interesting subject. He is evidently by no means familiar with even the English literature of the subject, as he says he knows of only one case which recovered after this accident—that recorded by Mr Chaldecott of Dorking in 1846,—while two others at least have been recorded and frequently

[1] "My view is, that instead of attacking disease of the upper jaw at its circumference, as has been almost the invariable practice since Gensoul's proposal to excise the whole bone, it is better to get into the disease as it were, and cut from the centre to the circumference, making sure that in doing so that circumference shall be thoroughly encompassed."—*Professor Ferguson's Fourth Lecture on Progress of Surgery, Lancet,* 1st *July* 1865.

alluded to : 1st, Mr Syme's case, mentioned in his Contributions to the Principles and Practice of Surgery ; and, 2d, Porter's patient, recorded by Mr Rhynd in his Observations on Strictures, 1849. In the direction of treatment, Mr Solly advocates a proposal of Mr Harrison's, to tap the peritoneal cul-de-sac from the rectum, in order to draw off the effused urine, and thus limit the peritonitis. In preference to this very risky and, so far as we can discover, hitherto untried operation, we believe that in a distinctly marked case of rupture of the bladder into the peritoneal cavity, the best chance for the patient would be to cut boldly into the abdomen in the middle line, as in the operation of ovariotomy, sponge out the pelvis, search for the rent in the bladder, and stitch it up with metallic sutures, trusting to nature to pull the patient through ; while in cases of rupture of the anterior wall, free incisions into the cellular tissue, as practised in Mr Syme's case, seem to give the best chance.

Rupture of the anterior wall is by no means uncommon, as in thirty-seven cases reported by Houël, fifteen involved the posterior and twelve the anterior wall.

Mr Solly's lectures on stricture and its treatment are unfortunately separated from each other by very long intervals of time ; and partly from this reason, partly for an apparent indefiniteness in the pathology, they are certainly among the least satisfactory parts of the book.

The first lecture is chiefly taken up with an account of two cases, dated 1843 and 1848 respectively, in which, after failing to introduce even a catgut bougie through the stricture, Mr Solly cut the patients in the perinæum without any grooved staff. He tells us the operation is certainly one of the most difficult in surgery.

In the second lecture on cases dated 1842 and 1848, catgut bougies are recommended to be retained as long as possible after being passed, in order that they may dilate the urethra—and then the following advice : we quote verbatim :—

"If you have a case of complete retention, you try a catheter or bougie, but use no force, and if there be much difficulty, do not follow the practice of some who *will* get an instrument in somehow or somewhere, but order a warm bath at once ; and should this be of no avail, give a full dose of opium, which may be combined also with antimony.

"In a hearty strong man even bleeding might be advisable, but you will meet with few cases which will not yield to opium. When, however, such a case does occur, I should recommend you to adopt the operation of puncturing the bladder by the rectum, in preference to that of cutting down on and dividing the stricture."

In the third lecture, on cases dated 1855 and 1856, the treatment advocated for bad cases of stricture is a modification of that of Mr Wakley, gum-elastic tubes and bougies being substituted by Mr Solly for the metal ones of that gentleman. The elastic catheter or tube is left in for hours or days to cause " suppuration of the stricture," and by this means a very rapid patency of the canal

is attainable. The slightest acquaintance with pathology is sufficient to demonstrate the coarseness of this mode of cure, and the absolute certainty of the return in a worse form of such a stricture, whenever cicatrization of the suppurating portion of the canal takes place; yet at p. 538 we find the following sentence:— " I have scarcely anything to add as regards the surgical treatment of stricture (January 1865)." There is not a word to instruct the students regarding Mr Syme's operation for stricture, he never mentions his name; there is no allusion to Mr Holt's plan of splitting strictures. The clinical lectures may have contained more information, we hope for the sake of the students of St Thomas that they did; but that it is not for want of room it has been omitted in the book is evident, from the extreme diffuseness of the style, and the admission into the clinical reports of such mild pleasantries as the following:—

"*16th*.—Much the same. Ordered eight leeches every alternate night. These little black gentlemen have proved most valuable allies to the cupping-glasses."—P. 530.

The two lectures on lithotomy which follow are a valuable contribution to the literature of the subject, and specially to its statistics, as they include a detailed account of the sixty-three cases in which Mr Solly has performed the operation, and furnish in nearly all the age of the patient, the method of operating, and the results. In all there are 49 recoveries to 14 deaths, or a mortality of 22·2 per cent.; but the relation of mortality to age is shown very distinctly, in the following table, compiled from Mr Solly's data:—

|  | Recovered. | Died. | Total. |
|---|---|---|---|
| Under 10 years, . . | 21 | 1 | 22 or  4·5 per cent. |
| Between 10—30 years, | 12 | 3 | 15 „ 20   per cent. |
| Above 30 years, . | 16 | 10 | 26 „ 38·4 per cent. |
|  | 49 | 14 | 63 |

After lectures on purulent absorption, and on a case of gunshot wound, a very interesting chapter on some cases of aneurism concludes the work.

We would specially refer to an admirable account of a case of abdominal aneurism, the progress of which was so much retarded, and its size so much diminished by sensible constitutional treatment, that several surgeons of eminence denied its existence, but which ended fatally by rupture of the sac some time after the patient had returned to active exercise and ordinary habits of life.

The one feature which more than any other will strike the careful reader of this book is its great inequality. Both in style and material, some chapters are so much better than others that it is hardly possible to believe that all are the product of the same brain. This may in part be accounted for by the manner in which lectures on similar subjects are placed in juxtaposition with very little reference to the date of their composition.

The publisher's part is admirably done, the type and paper being luxuriously clear and pleasant.

# Part Third.

## MEDICAL NEWS.

### MEDICO-CHIRURGICAL SOCIETY OF EDINBURGH.

#### SESSION XLIV.—MEETING VIII.

*7th June* 1865.—Dr WARBURTON BEGBIE, *Vice-President* of the Society, in the Chair.

##### I. TUBERCLE OF THE FALLOPIAN TUBES.

*Dr Grainger Stewart* showed a case of abscess in the pelvis, with general peritonitis, dependent upon tubercular disease of the Fallopian tubes. The patient was unmarried, 24 years of age, and had been under the care of Dr Matthews Duncan. She had her first child in May 1864. She recovered imperfectly, and in August and September was in the Infirmary on account of pains and tenderness in the pelvis. The uterus was at that time enlarged, but had returned to its natural state in September, at which time she was dismissed. After dismission she was out of health, never menstruated, nor had any discharge. In December she returned to the hospital, and it was then found that there was great sensitiveness in the parts on vaginal examination, and a rounded fulness projected above the brim of the pelvis; the uterus was also found to be enlarged. In April a distinct swelling was found on the right side of the uterus. Some time before death, a quantity of bloody purulent matter was discharged along with the urine, and this continued up to the time of death. She died exhausted, May 24, 1865.

The body was not much emaciated. The uterus was somewhat enlarged, its mucous membrane thickened and tubercular. There were several tubercular ulcers in the vagina. Both Fallopian tubes had tubercle deposited in their substance; their walls, particularly the inner coat, thickened, and in some parts ulcerated. The tubes were bound down by inflammation, the right was doubled up over the fundus of the uterus, and lay in the centre of an abscess. The succession of changes was evidently the following : *first*, tubercular disease of the tubes, uterus, and vagina ; *second*, pelvic abscess, intraperitoneal, in connexion with the tubercular process. This abscess was bounded by adhesions, but had perforated into the intestine at several points, but had not made its way into the bladder. It had also led to or favoured the occurrence of general chronic peritonitis. But it also appeared not improbable that the peritonitis was quite an independent lesion, seeing that it was tubercular, and that a tubercular pericarditis also existed.

##### II. ABSCESS OF LIVER, WITH PERFORATION OF THE STOMACH.

*Dr Stewart* showed a specimen of abscess of the liver. The patient, a man aged 40, in the end of December was seized with cough, and pain in the right side. Five days afterwards, vomited bloody matter. He was for some time under observation in the Infirmary, during which he became affected with jaundice, and had diarrhœa. His general symptoms were those of adynamic fever. The liver was greatly enlarged; contained several abscesses. The stomach was adherent to the surface of the liver, and one of the abscesses communicated with it by three openings : one of the size of a crown-piece, and two smaller. There was no general peritonitis.

##### III. GANGRENE OF LUNG FROM EMBOLISM.

*Dr Stewart* showed a case of gangrene of the lung, in which a considerable portion of the right lung was completely disorganized. A clot was found in a branch of the pulmonary artery supplying the gangrenous part.

#### IV. ABSCESS OF LUNG, WITH PLEURISY AFTER OPERATION.

Dr Stewart showed a case of pleurisy of the right side, and a gangrenous abscess in the lower lobe of the right lung, removed from the body of a woman whose tongue had been excised. A clot occupied the branch of the pulmonary artery supplying the gangrenous district. There was cancer of the pleura and of both kidneys.

#### V. OCCLUSION OF LEFT COMMON CAROTID.

Dr Stewart showed the aorta and right carotid arteries of a woman who had died of an erysipelatous inflammation of the right leg and extensive bed-sores. The aorta was extremely atheromatous, and the left common carotid artery was completely occluded throughout its whole length by a partially-organized clot. The occlusion appeared to have arisen from extensive atheroma of the aorta at the point of origin of the carotid. The external and internal carotids were open, and the districts supplied by them presented no abnormality.

#### VI. AORTIC ANEURISM.

Dr Stewart showed the heart and aorta of a man, æt. 41. He had been under the care of Dr Sanders, and had exhibited the symptoms and signs of aortic obstruction and incompetence, with hypertrophy of the heart. Signs of aneurism were carefully sought for, but could not be found, the only peculiarity of the physical signs being the remarkable loudness and sharpness of the sounds at the base. The patient became suddenly affected with dyspnœa, and sank in a few hours.

The pericardium was natural. The heart hypertrophied and dilated. The aortic valves were thickened and incompetent. The inner and middle coats of the aorta were extensively diseased. Immediately above the aortic valves there was an aneurism larger than a walnut, with an orifice about one inch long by half-an-inch broad, and projecting into the atrium of the right ventricle and the commencement of the pulmonary artery. It thus greatly obstructed the flow of blood from the right ventricle. The segment of the valve of the pulmonary artery next to it was adherent to the surface of the aneurism, and there was an opening about the size of a crowquill leading from the aneurism to the right ventricle.

Several secondary aneurisms projected from the right cavity into the muscular substance of the heart. They had been formed by a burrowing of the blood among the muscular fibres, and projected from the surface at several points towards the base as reddish rounded elevations.

No important vein or nerve was compressed. The lungs were congested and somewhat œdematous.

#### VII. SYPHILITIC AND WAXY DISEASE OF LIVER.

Dr Stewart showed portions of the liver of a man æt. 32, who had died of waxy degeneration of the kidneys, liver, etc., following upon syphilis.

There were numerous cicatrices on the surface, and in the substance of the liver, of the kind ordinarily regarded as syphilitic, and associated with many of them were waxy masses, composed of hepatic tissue, in a state of waxy degeneration. The cicatrices were evidently formed after the atrophy of these waxy masses, as had been seen in a case previously exhibited to the Society, and described in the British and Foreign Medico-Chirurgical Review for October 1864.

#### VIII. CANCER OF THE ANTERIOR MEDIASTINUM, ETC.

Dr Stewart showed a case of cancer of the anterior mediastinum, etc. A patient had been under the care of Dr Dickson, affected with symptoms like those of aneurism of the aorta, with occasional severe laryngeal attacks. The examination revealed extensive cancer of the anterior mediastinum, involving a certain portion of each lung, compressing and involving the pneumogastric nerves on each side with the recurrent branches.

#### IX. "CULLEN AND GREGORY ON CHANGE OF TYPE IN INFLAMMATION."

Dr George W. Balfour read a paper on this subject, which will appear in the next number of this Journal.

## REPORT OF THE TRIAL OF DR PRITCHARD FOR MURDER.

*Tried at the High Court of Justiciary, Edinburgh, before the Lord Justice-Clerk, Lord Ardmillan, and Lord Jerviswoode, on 3d, 4th, 5th, 6th, and 7th July 1865.*

THE prisoner Edward William Pritchard was placed at the bar, charged with the crime of murder, in so far as, (1.) on one or more occasions between the 10th and 25th days of February 1865, he did wickedly and feloniously administer to, or cause to be taken by Jane Cowan or Taylor (his mother-in-law), tartarised antimony, and aconite, and opium, or other poison or poisons, in consequence of which the said Jane Cowan or Taylor died, and was thus murdered by the prisoner; (2.) on repeated occasions between the 22d day of December 1864 and the 18th day of March 1865 inclusive, he did wickedly and feloniously administer to, or cause to be taken by Mary Jane Taylor or Pritchard (his wife) tartarised antimony and aconite, or other poison or poisons, in consequence of which the said Mary Jane Taylor or Pritchard died, and was thus murdered by the prisoner.

The counsel for the Crown were the Solicitor-General (Young), Mr Gifford, and Mr Crichton. The counsel for the prisoner were Mr A. R. Clark, Mr Watson, and Mr Brand.

The pannel pled not guilty. The medical witnesses were allowed to be present during the evidence given of the facts; but were directed to retire during the giving of scientific or medical opinions.

The general evidence established that Mrs Pritchard began to suffer from sickness and vomiting in November 1864; that she went on a visit to her parents in Edinburgh and remained with them till near Christmas ; that on her return her health was considerably improved ; but that soon afterwards she begun to suffer from retching, vomiting, and cramps, and that though there were occasional interruptions of the symptoms she gradually got worse, and died on the 18th of March. It was also shown that Mrs Taylor, Mrs Pritchard's mother, came to Glasgow to nurse her daughter, arriving on the 10th of February ; that Mrs Taylor was at that time in good health, though subject to attacks of headache, for which she was in the habit of taking Battley's solution ; that on the evening of the 24th she was taken suddenly ill, and died early in the morning of the 25th. It was further proved that the prisoner had an illicit connexion with Mary M'Leod (a servant in his family), and that his wife was aware that he was unfaithful. It was also established that the prisoner had purchased large quantities of poisonous drugs, particularly of tartrate of antimony and Fleming's tincture of aconite ; and that from having been in the practice of giving his wife food and medicine he had had abundant opportunities of administering poison to her. Finally, it was shown that the prisoner was in embarrassed circumstances, and that he became entitled to the interest of a sum of money on the death of his mother-in-law.

We give the medical evidence *in extenso* as reported verbatim in the *Scotsman*.

*William Tennent Gairdner.*—Mr Gifford—You are Professor of Medicine in the University of Glasgow ? I am. Do you know the prisoner ? I do. Do you remember receiving a message requesting you to call upon the prisoner ? I do. When ? On the night between the 8th and 9th of February. It was during the night and between the 8th and 9th February ? Yes. At what o'clock ? I think between twelve and half-past one. I cannot come nearer the time. Had

you retired? I had not. I was making preparations for a lecture next morning. What was the message? It was to come and see Mrs Pritchard. Did you go immediately? Immediately. Had you ever seen her before? Never; so far as I know.

Lord Justice-Clerk—Never as a patient? No.

Mr Gifford—You met Dr Pritchard at the house? Yes. Did he take you to his wife's bed-room? Yes. Did he tell you before he introduced you what was the matter with her? In general terms. What did he say? He said she had been very sick, and that her stomach was not able to bear food. I think he said she had been some weeks so. Did he say anything more? Not just at that time, I think. I mean before he introduced you? I think not. When he had introduced you, did he still continue to speak to you about her symptoms? At intervals; but I cannot remember exactly. When he introduced you, how did you find Mrs Pritchard—was she in bed? I found her in bed, lying on her back, with a considerably flushed face, and in a state of pretty considerable excitement. She then, I think, told me herself she had been sick. You said that the prisoner went on to speak of her symptoms? Yes. Did he say anything about spasms? He did; but I cannot remember whether I got the first information of the spasms from him or from her. Was any opinion expressed by the doctor as to what was the matter with her? The only thing I recollect was after the spasms became known to me, and he then said that it was catalepsy. Did he mention that any other medical man had seen her? He mentioned that Dr Cowan, of Edinburgh, had seen her. Did he say if Dr Cowan had ordered anything, or what? I think afterwards he said that Dr Cowan had ordered stimulants, and he said that his wife had had chloroform, but whether by Dr Cowan's orders or not I do not know. Did he say his wife had had the stimulants? I think so. He said that she had had champagne. You spoke to Mrs Pritchard, I suppose? Oh, yes. Did she say anything about having sent for you? Yes. What did she say? She began by apologizing for not having sent for me sooner. She said that Dr Cowan was an old friend of the family, that though she had wished to send for me, she had sent for him on that account, and made a kind of apology to me for not sending for me before. I told her there was no necessity for apology, because all that she had done was perfectly natural and perfectly right. Did she say anything about her own brother? Yes, she said she was aware that I was a class-fellow of her brother. Who was her brother? Dr Michael Taylor of Penrith. Did she seem to know that her brother was a college friend of yours? Yes. You had some other conversation with her about her symptoms, and how she felt? Yes; we had a good deal of general conversation about her symptoms. What state did you find her in? She had been sick. I found her to a certain extent exhausted, but not by any means extremely so. She had a pretty good pulse. There was nothing in her symptoms indicating immediate danger; and the most remarkable thing about her symptoms was the violent state of mental excitement she was in, and the spasms of the hands.

Lord Justice-Clerk—There was no immediate danger from exhaustion? I thought not from the state of the pulse, and the general aspect of the patient. But the most striking symptom was the excitement and the spasms in her hands, of which she told you? Yes.

Mr Gifford—Did you yourself observe the spasms in the hands? I did. She held her hands outside the bed-clothes above her head, and I saw that the wrists were turned in, and the thumbs somewhat inverted towards the wrists— a very peculiar state of the hand. I think it was owing to her mentioning this that Dr Pritchard used the word catalepsy. Did you form any opinion as to the cause of her excitement? I thought that she was intoxicated. You attributed it to the stimulants? I did. I attributed it to the combination of champagne and chloroform. Did you make any further examination? Yes. What did you do next? I then withdrew to the fire in order to warm my hands, with the view of making an examination of her person, and I had no sooner moved towards the fire than she began to scream out at the top of her

voice, " Oh, you cruel, cruel man," or something like that, " you unfeeling man ; don't leave me ;" and I then returned to the bed and said I was not going to leave her.  I then returned to the fire, and was warming my hands ; and in the midst of this she was in a state of most violent hysterical excitement, screaming out various exclamations, which after a little while I ceased to take any notice of, because I thought she was not responsible for them at all, because I thought they were the exclamations of an intoxicated woman for the time being, and paid no attention to them ; but the general purport of them was a remark about my being extremely unfeeling in leaving her alone and going to the fire-side.  Then I returned to her, and examined her person : I took up the bed-clothes and examined the belly, and I asked particularly both at her and Dr Pritchard if there was any chance of her being pregnant—pregnancy being a frequent cause of vomiting.  I found there was none ; and then, after various other inquiries, and feeling her pulse, looking at the state of her skin, and so on, I came very soon to the conclusion that she was not in a state to give any evidence at all about her own previous history that night, and I just gave the orders I thought necessary, and left her.  Did you order that the stimulants should be discontinued ?  Yes.

Lord Justice-Clerk—To whom did you state that ?  To Dr Pritchard and to her ; but I repeated it more emphatically to Dr Pritchard than to her, because I told him very decidedly that I thought this was very bad practice, and that she was to get no stimulants whatever until I saw her again.

Mr Gifford—From what you observed, did you see any symptoms of catalepsy in her ?  No.  You formed a distinct opinion that there was no catalepsy ?  I may explain that I hardly know what catalepsy is.  It is not a disease of ordinary medical experience at all.  Most of what we know of it is from books ; and what is written about it is to a great extent apocryphal ; therefore, I don't presume to be an authority upon catalepsy.  Do you remember of her using any expression while you were there, to you or to any one, about hypocrites ? I cannot say.  She used a great deal of language in that hysterical state of which I did not take any notice, and in fact deliberately and intentionally ignored.

Lord Justice-Clerk—You intentionally paid no attention to it ?  Yes.  I thought it was as well to show her that I did not wish to give attention to every little expression.  Were any of the servants present ?  I have great difficulty in remembering that.  My attention was concentrated upon Mrs Pritchard.  I have some recollection of Lattimer, but none at all of the other servant, and Lattimer I think I saw chiefly at the following visit.  You left that night ? Yes.  Did you say anything to Dr Pritchard before you left ?  I spoke to him in strong terms about the impropriety of this stimulating practice, and said it was very bad practice.  He said it had been ordered by Dr Cowan.  He rather seemed to indicate that he concurred with me in disapproving of the champagne, but asked me if she was to get no more chloroform.  I said, " No, no stimulants and no medicine till I see her again."  Did you arrange when you were to see her again ?  Yes ; I was to see her the same day of which this was the morning. Did you call ?  I called between twelve and one o'clock.  That was the 9th February ?  Yes.  Did you see Dr Pritchard ?  I did.  He said Mrs Pritchard was better, and quite quiet.  Did you go to Mrs Pritchard's bed-room ?  We went to her bed-room, and I found her quiet.  Free from fever ?  Yes.  Had you any conversation with her ?  Yes.  In general terms I assured myself that she felt better, and that she had not vomited since I saw her ; but she still had the remains of the spasms in her hands.

Lord Justice-Clerk—That was about twelve hours after your former visit ? Yes.

Mr Gifford—Dr Pritchard was there the whole time ?  He was.  How long might you be there ?  About ten minutes.  What did you direct ?  I directed that she was still to get no stimulants and no medicine, and that when she required food she was to get a boiled egg plain, and milk and bread, but nothing else ; and I told her that my object was to make her diet as simple as it could

possibly be, in order that there might be no possibility of her taking anything that would disagree with her. That is, nothing that would produce sickness or sit heavy on her stomach? Yes. I think I told her that if her stomach had fair-play it would digest milk and the simple food I indicated. Did you say anything more to Dr Pritchard? I simply repeated generally what I had said to her.

Lord Justice-Clerk—You said if her stomach had fair-play it would digest milk? I do not wish it to be understood that I used these words. I do not remember the exact words; but I endeavoured to impress her with the idea that her stomach would digest a simple thing when it would not digest complicated things; and that she must not load it with medicine and with a variety of food, but that she must go back to perfectly simple food.

Mr Gifford—Did you form any opinion as to what was the matter with her? I was very much puzzled. You are confining yourself to the one visit, doctor? Yes. What did you think of her? I was very much puzzled. I thought she was intoxicated the evening before—drunk, in fact; but beyond that I formed no very decided opinion. Did you say, on leaving on the second visit, that you would return again? I do not think I fixed any time. Did you think her case required serious and constant attention? Yes. Just tell us what was your impression of it? My impression was, that if I had been a general practitioner, in attendance upon her, I should probably have seen her every day, or twice a-day; but there was a doctor in the house, and my habit is to act as a consulting physician, not as a general practitioner.

Lord Justice-Clerk—You considered that you had been called by the prisoner as a consulting physician? Yes.

Mr Gifford—This was upon the 9th. Did you return next day? No; I never saw her again.

Lord Justice-Clerk—Were you ever sent for again? No.

Mr Gifford—Had you to leave town? I had to leave town for a distant engagement on the Friday, and before leaving town I wrote a note or sent a message to ascertain how Mrs Pritchard was, and received for answer that she was better. I then left for my engagement, and returned on the Saturday afternoon. On my return there was a patient waiting for me; and while I was engaged with the patient, I believe Dr Pritchard called and left word that his wife was better, and that I need not call. Did you write to your friend Dr Taylor, in Penrith, about the case? Yes. I think it was on the 9th February, the day after my second visit. What was your reason for writing him? My reason was that I was puzzled, and that I thought the practice bad in so far as stimulants were concerned at least, and that I wished to be backed up and aided by his suggestion. Were there any symptoms of gastric fever upon M's Pritchard that you observed? I did not think there was any fever at all.

Cross-examined by Mr A. R. Clark—You said you did not understand what was the meaning of the word catalepsy which the prisoner used? It was not I that applied the word to the case. I do not say it was, but I thought you said you did not understand the meaning of the word as applied? No; it seemed to me to have no application to the case. Had you known the prisoner before? Yes. Long? More or less, I think, for about a year; but I don't remember how long? About how long? I think for one or two years. My connexion with him has been chiefly seeing a few cases with him in consultation.

Lord Justice-Clerk—You knew him as a medical man for a year or two previous? Yes.

Mr Clark—Was his nomenclature correct? Witness—In this case? Mr Clark—No; generally. Had he any peculiarity in the way in which he spoke of disease? Well, I can't answer that question. Did you not observe anything peculiar in his nomenclature of disease? Perhaps it was occasionally a little at random. What was it you observed in him? I have no very distinct impression. You say it was perhaps a little at random. What do you mean by that? I mean by that that I do not think he was a model of accuracy and wisdom and caution in applying names to things. Well, without being a model

of wisdom and accuracy and caution, what was the way in which he spoke of disease? Describe the way in which he spoke of disease. I have said before that there was nothing that caught my attention. At the same time, was there not something which did catch your attention? I think he was rather a careless man in his ideas; slip-slop a little. Was that through ignorance, do you suppose? I cannot tell. Was he a skilful man in his profession? I had not enough to do with him to tell you that. Tell me what symptoms you observed in Mrs Pritchard when you saw her on the second day? I think the chief symptom was the remains of the spasm. What was the state of her pulse? It was pretty quiet. What was the state of her tongue? I have no distinct impression about the state of her tongue. Her colour? Her colour was good, but rather high. Was she very prostrate? I think not. You said you wrote to her brother after you saw her the first night? Yes. Did you indicate to him that there had been anything more than improper treatment?—did you indicate to him that there had been any foul play? Witness—You mean poison? Mr Clark—Yes. Witness—Certainly not.

*Dr James Paterson.*—Solicitor-General—Are you a doctor of medicine in Glasgow? Yes. How long have you been in practice there? Upwards of thirty years. I believe you were at one time Professor of Midwifery in the Andersonian University? Yes. It is not, properly speaking, a University. You may call it the Andersonian Medical School. For how long were you Professor there? For twenty-two years. Did you resign? I did, about two years ago. Where is your house? No. 6 Windsor Place, Sauchiehall Street. That is another division of the street in which Dr Pritchard lived? It is. Do you remember being called to Dr Pritchard's house in February last? I do. Do you remember the day of the month? It was on Friday evening the 24th February. Was that the first time that you had been called there? The first time that I ever crossed his threshold. What time of night were you called? Between half-past ten and a quarter to eleven. Did you see Dr Pritchard? I met him in the lobby or hall of his own house. Tell us, if you please, what he said to you? He conducted me into his consulting-room on the first floor, and then he told me that his mother-in-law, while in the act of writing a letter, had suddenly been taken ill, had fallen off her chair upon the floor, and had been conveyed up stairs to the bed-room. Did he say how long before your visit this happened of the old lady tumbling off the chair? I think he said about half an hour or an hour before I came. I asked if he could assign any reason or cause for the suddenness of the attack. He said his mother-in-law and Mrs Pritchard had been partaking of some bitter beer, as I understood, for supper, soon after which they both became sick and vomited, and both complained that the beer was much more bitter to the taste than usual. You are telling us now what he said to you on your first arrival in the consulting-room, where he and you were alone together? I am. He said that they could not have taken more than a third part of a pint each, because there was still some remaining in the bottle. I said I could not think it possible that either Allsopp's or Bass's beer could produce such an effect, and that the attack must depend upon some other cause. Allow me to ask you, why did you mention Allsopp's and Bass's beer? These were the only two that struck my mind at the moment. Did he point to the beer bottle on the table? Afterwards, but not at this time. I then asked him in regard to the previous state of his mother-in-law's health, and I asked particularly with reference to her social habits, when, by a particular insinuation, he led me distinctly to understand that she was in the habit of taking a drop occasionally.

Lord Justice-Clerk—Drinking spirits, you mean? Yes.

Solicitor-General—What else did he say? He stated also that Mrs Pritchard had been very poorly for a long time past with gastric fever; and that some days previously he had telegraphed for his mother-in-law to come through to keep her or attend to her in her illness. We then went up stairs to the bed-room. On entering, I observed Mrs Taylor lying on the edge of the bed nearest

me. She was lying on her right side with all her clothes on, and on her head a half-dress cap with a small artificial flower. She had all the appearance of there having been a sudden seizure? Yes; and Mrs Pritchard, in her night-dress, with nothing on her head, and her hair very much dishevelled, was in the same bed, but underneath the bed-clothes, and sitting up immediately beyond her mother. Tell us the appearance of the old lady. On examining Mrs Taylor, my impression was that she had previously been in very good health.

Lord Justice-Clerk—But was she dead or living? She was living at this time. She gave you the indication of a person who had been in good health? So far as I could judge from appearance.

Solicitor General—Not touched by illness or emaciated? No. A healthy-looking old lady? I should say so. She seemed to me to be rather above the ordinary size, good-looking, well-formed, altogether I should say a very superior-looking person for her time of life, and certainly not having the slightest appearance of being addicted to the use of spirituous or intoxicating liquors. On examining her face it was rather pale, but the expression was calm or placid. The eyelids were partially closed, the lips were rather livid, the breathing slow and laborious. Her skin was cool, and covered with a clammy perspiration. The pulse was almost imperceptible, and she seemed to me to be perfectly unconscious. On my opening up the eyelids I found both pupils very much contracted. From those symptoms, and judging from her general appearance, my conviction was that she was under the influence of opium, or some other powerful narcotic, and I at once pronounced my opinion that she was dying. That was your opinion? Yes; decidedly. To Dr Pritchard, who was beside you all the while? Yes; on my doing so, Pritchard said something in an undertone of voice, apparently unwilling that my opinion should be heard by the ladies, which was quite natural and quite common. We retired a little from the bed-side, went near to the fire-place, and I then stated distinctly that she was dying. Pritchard said she had frequently had attacks of a similar kind before, but never one so severe. I said that nothing we could do would have the slightest effect, but that, as a last resource, we might try mustard poultices to the soles of the feet, the calves of the legs, and the inside of the thighs, and as quickly as possible administer a strong turpentine enema. That is an injection? Yes. Pritchard at once proceeded to prepare the enema, and he said he had a little before given her one in which he had administered a glass of brandy. The old lady lay apparently comatose, or unconscious; but on being roused a little, and the head and shoulders slightly elevated, there was a degree of consciousness came on, and the pulse became perceptible at the wrist. Was that rousing the first thing you had done to test whether she was really conscious or not? It was. And what you meant by saying that she was seemingly unconscious before was, that she was not manifesting consciousness before? Yes.

The Lord Justice-Clerk—All the symptoms manifested unconsciousness, did they not? Yes. The pulse was first perceptible at the wrist? Yes; I directed Pritchard's attention to the pulse, and he then clapped the old lady on the shoulder and said, "You are getting better, darling." I looked at him, and shook my head ominously, as much as to say, "Never in this world."

The Solicitor-General—She gave no promise to you of being better? None. A slight fit of retching now came on, and she put up a small quantity of a frothy kind of mucus, immediately after which the coma or insensibility returned—the breathing became more oppressed, more laboured, and the alvine evacua-tions were passed involuntarily. I then concluded that the case was utterly hopeless, but Pritchard administered the enema. What then? I left the room and went down stairs accompanied by Pritchard, and we went into the con-sulting-room. I repeated my opinion that she was in a state of narcotism.

The Lord Justice-Clerk—That is to say, under the influence of opium or some narcotic? Yes.

The Solicitor-General—Narcotism was the expression you used? Yes. Pritchard then said the old lady was in the habit of regularly using Battley's

sedative solution, and that she had a few days before purchased not less than a half-pound bottle of the medicine, and that he had no doubt, or it was very likely, that she might have taken a good swig of it. That was his expression? Yes. There was little more said at that time in regard to the state of Mrs Pritchard. You know Battley's solution? I know it, but I very seldom used it. Had Mrs Taylor anything of the appearance of an old lady who had been in the practice of using such a medicine? My impression was that she was not what is called an opium-eater, or one who used opium to any great extent. She presented no appearance of that? That was my opinion. Would you recall to your recollection the bed-room again, and tell us what you observed of Mrs Pritchard? While attending to Mrs Taylor, I was very much struck at the same time with the appearance of Mrs Pritchard. She seemed exceedingly weak and exhausted. Her features were sharp or thin, with a high hectic flush on her cheeks, and her voice was very weak and peculiar—in fact, very much resembling the voice of a person verging into the collapsed stage of cholera. The expression of her countenance conveyed to me the idea of a kind of silly or semi-imbecile person at the time. At first I was inclined to attribute her appearance to the recent severe attack of gastric fever, which I was told by the prisoner she had had, and her symptoms aggravated of course by the great consternation and grief not unnaturally caused by the sudden and alarming condition of her mother. At the same time I must say I could not banish from my mind the idea, or rather the conviction, that her symptoms betokened that she was under the depressing influence of antimony. You mean that that impression or conviction came upon you at the time while in her presence, and that you could not get quit of it? Certainly. I did not put a single question to Mrs Pritchard.

The Lord Justice-Clerk—The impression was created entirely by her appearance? Yes, and the general symptoms of the case. I then left, and went home about half-past eleven.

Solicitor-General—Were you sent for again in the course of that morning about one o'clock, and did you afterwards get another message not to come because she was dead? A little before one o'clock my door-bell was rung. I was in bed, but Mrs Paterson happened to be sitting up. She opened the door, and a girl asked me to come directly and see Mrs Taylor. I refused to go, because I was certain that I could be of no service; and as I was very much fatigued with the previous day's work, I was very unwilling to rise; but I sent my compliments to Dr Pritchard, saying that if he really thought I could be of use he was to send back word, and I would then rise and visit her. Your house was only a short distance from his in the same street? 195 yards. Did any message come back? No message came back, and I did not go; but about ten o'clock on Saturday morning, the 25th, an elderly gentleman called upon me. Was that Mr Taylor, the husband of the old lady? I afterwards learned that it was. Did he inform you that she was dead? He came for the death certificate. You refused to give a certificate? I said I was surprised that Dr Pritchard had sent for a certificate, and that as a medical practitioner he should have known that it was not given to the friends, but to the district registrar. Were you afterwards applied to by the registrar? On Friday, 3d March, I received through the Post-Office a schedule from the registrar, in which I was requested to fill in the cause of Mrs Taylor's death, and duration of her disease. You refused to do that? I did so, and sent it back with a note accompanying it, directing his attention to the circumstance. When did you see the prisoner after that? On Wednesday forenoon, 1st March. I met him accidentally in Sauchiehall Street, near my own house. On coming up to me, he said I had been very correct in my opinion with regard to his poor mother-in-law, and he added that he would feel obliged if I would visit Mrs Pritchard next day at eleven o'clock, as he required to be in Edinburgh at the funeral of his mother-in-law. I at once agreed to visit at his request. And did you go? Yes; on Thursday, 2d March, about eleven o'clock in the forenoon. Did you see Mrs Pritchard? Yes. She was in bed. How did you find her? She was still

very weak and prostrate, and in a weak voice she expressed her satisfaction and her gratitude at my calling. Then, in a very earnest manner she asked me if I really thought that her mother was dying when I saw her. I said most decidedly I did; and I had told Pritchard so. She then clasped her hands, looked up, and feebly exclaimed, " Good God, is it possible?" and burst into a flood of tears. I put some questions then as to the previous state of her mother's health, and especially if she was habitually addicted to the use of Battley's sedative solution. She told me that her mother's health generally speaking was good, but she suffered occasionally from what she called neuralgic head-aches, and for relief of these attacks she did take a little of Battley's sedative solution; but she added, that it could not be said that she was in the habitual use of that medicine. I then questioned her with regard to herself. She told me that for a considerable time past she had suffered very much from sickness, retching, and vomiting, with severe pains in the stomach and throughout the bowels, accompanied with purgings, great heat and uneasiness about the mouth and throat, and a constant urgent thirst. I examined her tongue. It was very foul, and of a lightish brown colour. Her features were still very sharp and deeply flushed. Her pulse was weak, contracted, and very rapid. Her skin was moist, but defective in animal heat, and altogether she presented an appear-ance of great general prostration. Her eyes were watery, but clear and intel-ligent. I prescribed for her small quantities, at short intervals, of champagne and brandy to recruit her strength; small pieces of ice occasionally to relieve the thirst and irritability of the stomach. If she tired of these, I said she should have recourse to granulated citrate of magnesia as a cooling effervescing drink, and have a sinapism or mustard-poultice applied over the pit of the stomach. So far they were verbal directions which you gave her? I also recommended small quantities, at short intervals, of easily-digested nutritious food, such as beef-tea, calf-foot jelly, chicken-soup, arrowroot, and so on. I then wrote a prescription for twelve grains of camomile, twenty-four of blue or grey powder, twelve of powdered ipecacuanha, and six grains of aromatic powder, the whole to be carefully mixed up, and divided into six equal parts, one powder to be taken every day. That prescription was with the view of relieving the biliary disturbance, and soothing the mucous lining of the alimen-tary canal. Did you give the prescription to herself? I did, and told her to show it to Pritchard when he came home in the evening, and to tell him what I had ordered. I never saw Mrs Pritchard again until within four or perhaps five hours of her death. Between the visit of which you have given us an account, and the last time you saw her before her death, did you see her husband, the prisoner? I did. When? On Sabbath evening, the 5th March. About nine o'clock he called at my house. What did he say? He told me his wife had been very much relieved by the medicines and treatment I had ordered; that she relished very much the small quantities of champagne and brandy, and felt refreshed by the cooling effervescing draught and the ice. What did he say further? He said that she was still very weakly, and the stomach still irritable. I recommended the continuance of the stimulants and nourishment, and to pay most particular attention to the state of the alimentary canal,—the stomach and bowels. Nothing more passed at that interview? No. Then I suppose the next occasion you have to speak of is that visit a few hours before her death? On the 17th of March—the Friday evening— Pritchard called upon me personally, I think about a quarter to eight o'clock in the evening, and requested me to go with him to see Mrs Pritchard. Did you go? I did. And went up to the bed-room and saw the lady? Yes. Mrs Pritchard was in bed, in a sitting position, supported by pillows. What appearance did she present? I was very much struck with her terribly altered appearance. She seemed quite conscious. I went up to her bedside, and she caught my hand, and I could see a half smile of recognition upon her counte-nance. She very soon began to mutter about her having been vomiting. Pritchard was standing behind me, and he volunteered to say that she had not been vomiting—that she was only raving. She complained of great thirst, and

Pritchard poured some water out of a caraffe into a tumbler, and gave it to her to drink. At the same time he said, "Here is some nice cold water, darling." Did she drink it? She drank it. I observed her countenance very much changed from what it had been when I last saw her. There was a peculiarly wild expression; the eyes were of a fiery red, and sunk-looking. Her cheeks were hollow, sharp, pinched-looking, and still very much flushed. Her pulse was very weak, and exceedingly rapid. Her tongue, how was it? It was of a darkish-brown colour, very foul; and she immediately began to grasp with her hand, as if to catch at some imaginary object on the bed-clothes. She muttered something about the clock; and Pritchard said he thought she referred to the clock or timepiece on the drawing-room mantelpiece. There was no clock in the room where she was. I expressed my surprise at the great change and alarming appearances, and I asked Pritchard how long she had been entirely confined to bed since I saw her. He said only since morning; that yesterday or yesterday afternoon, she was in the drawing-room amusing herself with the children. I again expressed surprise at her alarming condition. Her condition was alarming? Yes, certainly. Anything said about her sleep? He said she had not slept for four or five days or nights. I then said we must endeavour to do something to relieve her, and, if possible, procure some refreshing sleep. We left the bed-room and went down stairs, and I then prescribed thirty drops of solution of morphia, thirty drops of ipecacuanha wine, five or ten drops of chlorodyne, and an ounce of cinnamon-water. This was to be repeated in four hours, if the first draught did not give relief. That is, did not procure sleep? Quite so. Did you write the prescription? I did not. Pritchard wrote the prescription at my dictation. Did you ask him to write it? No; I said it was unnecessary to write it, it was so simple that he might make it up himself. I was anxious to save time, and give relief as soon as possible. What did he say to that? He said he kept no medicines in the house excepting chloroform and Battley's sedative solution. Did you say anything to that? I asked if he did not keep a small stock in order to meet any emergency, and particularly for night work; and he said he did not. Did that strike you as anything strange? It certainly did.

Lord Justice-Clerk—You mean that it is not a usual thing for a medical practitioner? Yes. Medical men in extensive practice must keep medicines in stock, especially for night work, if they have much night work to go through.

Solicitor-General—And he wrote it to your dictation? So far as I know. You did not look at it? No. You assumed he would write it correctly? Certainly. [Shown No. 13.] Is that in his handwriting? I think it is. I am sure it is. Does that conform to what you told him to write? Yes. What next occurred? I then left the house, and I heard no more of it till about one o'clock on the following morning, which was Saturday. And what did you hear then? At that time my door-bell was suddenly loudly rung, and on going to the door I found a young man, who requested me to go to Mrs Pritchard immediately, as she had become much worse, and was thought to be dying, if not dead. I proceeded to dress myself at once. In less than three minutes after that my door-bell was again rung, this time by a servant-girl; and as I opened the door she said, "You need not come; Mrs Pritchard is dead." And you did not go to the house again? No. And you have mentioned to us the only visit you ever paid to the house, and all you saw of those two ladies? Certainly; I never crossed the threshold of the house except on these occasions. Did you ever say to the prisoner that you thought his wife, Mrs Pritchard, had taken too much wine? I never did. And you have mentioned to us quite accurately everything you ever ordered for her? Yes. You are quite sure you never recommended Dublin stout for her? No; I never did.

Cross-examined by Mr Clark—You mentioned that Mrs Taylor had not the appearance of having been in the habit of using opium? That is my candid opinion. Have you had experience in cases of that kind? I have. And

Mrs Taylor did not resemble any such patient? I think not. Why did you judge that she was not addicted to the use of opium? If a person is in the habit of taking opium to a great extent, you generally find that they are not very good in colour. They are generally thin in features, and hollow about the eyes,—in fact, not of a healthy appearance generally. And Mrs Taylor, being stout and healthy-looking, as far as you could judge, you concluded that she was not addicted to the use of opium? That was my impression.—at least not to any great extent. I do not say that she never took opium at all, but merely that she was not an habitual consumer of opium. What do you mean by not being an habitual consumer of opium? do you mean that she did not take it constantly, though she might take it by way of medicine? That is what I meant. And when Dr Pritchard said to you that she was in use to take opium, you thought what he was saying was not true? That was my impression after I saw the individual; I took it for granted before I saw the individual. After you saw the individual, you thought the statement was not consistent with fact? I thought so. Now, when you were with Mrs Taylor that evening, did you examine attentively the condition of Mrs Pritchard? I only glanced at her.—I did not put a question to her. I formed a diagnosis from the symptoms that were present. By merely looking at her? Yes; just as I am in the habit of forming an opinion of any patient I see for the first time. But you did not examine her at that time as a patient, did you? Certainly not. But you formed the conviction that she was under the influence of antimony? Yes. Had you ever seen before a case of poisoning by antimony? Yes. How many? Perhaps two or three. What were they? Young children. Did you ever see a case of poisoning by antimony in the case of an adult? No. Now, I understand when you translate the words, "depressing influence of antimony," you mean that she was being poisoned by antimony? I was under the idea that she had been getting antimony for some time past. I had nothing to judge from but her appearance. Do you mean she was getting antimony medicinally, or for some other purpose? Of course, I could form no opinion as to how or by what means she was getting antimony. Was the condition you have described one to which a patient could be brought by the medicinal use of antimony? Not exactly the medicinal use, but a long-continued use; a judicious practitioner would not carry it to such an extent as to produce such debility and prostration. Did you mean to convey to us that she had been taking antimony medicinally, or that she was being poisoned by antimony? My impression was that she was being poisoned by antimony. And you formed that conviction by looking at her? Yes. Simply from looking at her? Yes; judging from symptomatology, the science of signs of disease. Now, as you thought Mrs Pritchard was suffering in that way from antimony, did you ever go back to see her again? I did not, and I believe that I never would have been called back again if I had not met Pritchard accidentally on the street. Why did you not go back? Because she was not my patient. I had nothing to do with her. Then, though you saw a person suffering from what you believed to be poisoning by antimony, you did not think it worth your while to go near her again? It was not my duty. I had no right to interfere in any family without being invited. Dr Paterson, is it not your duty to look after a fellow-creature whom you believe is being poisoned by antimony? There was another doctor in the house. I did the best I could by apprizing the registrar. Did you tell Dr Pritchard? No. You did not mention it to him? I did not. Had I been called in consultation with another medical man, I should certainly have considered it my duty to have stated distinctly my medical opinion. But you stood upon your dignity, and did not go back to see what you believed to be a case of poisoning? I had no right. No right? I had no power to do it. No right? I was under no obligation. You were under no obligation to go back to see a person whom you believed was being poisoned with antimony? I took what steps I could to prevent any further administration of antimony. By never going back to see her? No; I beg your pardon. What did you do? By refusing to certify

the death.   Had there been a post-mortem examination of Mrs Taylor's body,
I believe that in all probability the drugging with antimony would have gone
no further, at least at that time.   But still it comes to this, that, although you
had the impression, you never went near her again until you were called in
by Dr Pritchard?   Yes.   Well, did you find her labouring under the same
symptoms or similar symptoms to those you observed when you were with her
on the 2d March?   Yes.   You still believed her to be suffering under poison-
ing by antimony?   I did; and I prescribed accordingly.   Did you see her
alone on that occasion?   I did.   Did you give her any indication of what you
thought was her ailment?   I did not mention antimony to her in the slightest.
Did you mention poison to her?   I did not.   Did you give her any idea that
she was labouring under anything other than natural disease?   I did not con-
sider that she was labouring under natural disease.   Question repeated.   I did
not.   Why?   Because the treatment I prescribed for her, provided she got
nothing else, was quite sufficient, in my opinion, to have very soon brought
her round, taking it for granted that my prescriptions were carefully walked
up to, or rather my advice,—not prescriptions.   It was Dr Pritchard that
asked you to visit his wife upon that occasion?   Yes.   Did you mention to
him your opinion as to his wife being poisoned by antimony?   I did not.   It
would not have been a very safe matter to do that.   Why did you not visit
her the next day and see that your advice had been acted on?   I did not con-
sider at all, sir, that she was my patient, and I had no right or title to go back
and visit her.   I would have considered myself intruding upon the family had
I done so.   You had been asked to visit her by the prisoner himself on the
1st March?   I believe that if I had not met him accidentally, I would not have
been asked.   You have no right to say that, doctor.   Well, I understood that
visit more in the light of a friendly call of condolence under painful, trying
circumstances than as a medical visit.   Had you ever been intimate with Mrs
Pritchard before?   No.   Why did you call to pay a visit of condolence to a
person you never saw before?   It was at Dr Pritchard's request.   To condole
with her?   I could conceive of nothing else.   What was the use of calling on
a person whom you did not know to condole with her?   I had seen her at her
mother's deathbed.   Were you not called as a medical man?   I don't think
so.   I understood Dr Pritchard was attending her himself; that I was only to
call during the day, and when he came back in the evening I had nothing to
do with it.   But when you saw something so specially the case, why did you
not call back?   Simply because it was none of my business; I did not con-
sider it my duty.   She had her own husband there,—a medical man.   Having
been in a house where you thought there was poisoning going on, you did not
consider it your duty to go back?   I had discharged my duty as far as I
thought was incumbent upon me.   By describing certain things, and not know-
ing whether the prescription was followed?   In any case where a consultation
is held, the consulting physician has no right to go back to see the patient.
Then it was the dignity of your profession that prevented you from going
back?   It is the etiquette of our profession.   That was one reason why I did
not go back.   I did not say it was the only one.   In any case where I had
been called in for consultation, were I to go back, it would be a breach of the
etiquette of my profession.   You said you wrote to the registrar: did you
write first, or did you get a letter from the registrar before you wrote to him?
I got the schedule sent to me in the first place.   That was about Mrs Taylor?
Yes.   I got no notice with regard to Mrs Pritchard.

Solicitor-General—It was to visit Mrs Taylor, who was thought to be very
ill upon the 24th February, that you was called in?   Yes.   That was only
what you was called in for?   Only.   You were not consulted about Mrs
Pritchard at all?   No.   Was your meeting with Dr Pritchard accidental?
Purely accidental.   What time of day was it?   About eleven o'clock in the
forenoon; and he told me that he was going from home, and would be obliged
if I would call and see his wife next day.   You had no reason to suppose, and
do not suppose, that he was coming for you?   Certainly not.   And it was,

therefore, from your accidentally meeting him at eleven o'clock that one day, and his asking you to call at eleven o'clock the next, that you thought it was an accidental invitation? Purely. You said that it might not have been safe for you to communicate your suspicions to Dr Pritchard himself? It would not have been very natural, certainly. You mean that your suspicions concerned himself? I would rather not answer that question.

Mr Clark—You did not communicate that to any of the family,—that is, Dr Pritchard's family? No. Nor the Taylor family? I never saw any of the Taylor family, unless Mr Taylor himself, when he came for the certificate.

You told us that you wrote to the registrar, Mr Struthers? I did. That letter has been destroyed? I know the letter verbatim. I wrote it very guardedly.

Mr Clark—Is there a copy in existence?

Solicitor-General—I have a copy taken from the witness's dictation. State to us the terms of the letter, as well as your memory serves you, which you sent to the registrar.

Witness—The letter was dated No. 6 Windsor Place, 4th March 1865, and addressed:—" Dear Sir,—I am surprised that I am called on to certify the cause of death in this case. I only saw the person for a few minutes a very short period before her death. She seemed to be under some narcotic; but Dr Pritchard, who was present from the first moment of the illness until death occurred, and which happened in his own house, may certify the cause. The death was certainly sudden, unexpected, and to me mysterious." Then followed, " I am, dear Sir," &c., and I signed my name. I rendered emphatic the words, " the cause of death," by having them underlined.

Mr Clark—That was the whole letter? I believe so, *literatim et verbatim*. And there was no postscript? There was no postscript. Was that letter sent off the date it bore? I sent it off that day through the post-office, directed to James Struthers, registrar. When I was asking you whether you had taken any means for the protection of Mrs Pritchard, this was the communication you referred to? Yes. And the only communication you referred to? The only communication, and I had three motives for making it. Never mind the motives; but you say this was the only communication? The only communication. And Mrs Pritchard was not mentioned in it? No. You did not make any communication whatever to any one of Mrs Pritchard's family? No. Nor to any one? Nor to any one. I spoke of the matter in my own family; that was all.

Lord Justice-Clerk—In answer to a question from the prisoner's counsel, I think you stated that your impression when you first saw Mrs Pritchard, and afterwards when you saw her on the 2d March, was that she was being poisoned by antimony. That was what you said? Yes. Now I want to know exactly what you mean by that. Do you mean that you believed that some person was engaged in administering antimony to her for the purpose of procuring her death? But to me unknown. But was that your meaning? Yes, that was my meaning.

*Dr James Moffat Cowan.*—Mr Gifford—You are a Doctor of Medicine in Edinburgh? I am. Are you in practice? Not now, and I have not been for several years. I am a Doctor of the University of Edinburgh. Were you a relative of the late Mrs Pritchard? I was. What relation were you? Well it was rather distant; perhaps you will be able to trace it. Her grandfather and my grandfather were brothers; that is the relation. (A laugh.) You were second cousins, then? Yes. Do you remember getting a letter from the prisoner some time in February last? I do. Do you remember when it was? I think I saw Mrs Pritchard on the 11th, and I would get the letter on the 10th. You have not the letter now, I think? I have not got it; I unfortunately destroyed it. What did the letter say—what was its import? The import of it was, that Mrs Pritchard had been ailing for some time, and that he (Dr Pritchard) was becoming very anxious about her case. He wished to call in another medical man in Glasgow. Did he wish you to come through

and see her? He did. Well, did you go? Yes, I went. On the 11th? Yes,
on the 11th. Did you go to Dr Pritchard's house? Yes. When did you
reach there? About four o'clock, or between four and five o'clock—at dinner-
time. Where did you find Mrs Pritchard? She came down stairs from the
bed-room to the drawing-room to see me there. She met you in the drawing-
room? Yes. Was Mrs Taylor there at that time? No. You know that she
went afterwards? At my desire she went afterwards. Have you any reason
for saying that it was the 11th of February? Well, I have no particular
reason, except that, to the best of my recollection, that was the date. It was
a few days before Mrs Taylor went through? It was two days before that.

Lord Justice-Clerk—I was only there once in February, and that visit was
two days before Mrs Taylor went to Glasgow.

Mr Gifford—Did you see the prisoner before you saw Mrs Pritchard? He
met me in the lobby, and I inquired after Mrs Pritchard, and he said she
was very much better that day, and that she would be down to see me in the
course of a few minutes. And you went to the drawing-room? Yes. When
you saw Mrs Pritchard in the drawing-room, how did you find her? I found
her very much better than I expected to have found her. Did you put ques-
tions to her as to what her symptoms were? Yes; she said she had been
troubled with considerable irritability of the stomach, that she could not retain
food on her stomach, and had been vomiting for some time time back. Did
you put what questions you required in order to enable you to judge as a
medical man? Well, I did not go exactly as a medical man; I went more as
an old friend, but I did ask one or two questions. Then what did you say or
do? In the first place, I saw she had erred in coming down stairs, and I
ordered the application of a mustard-poultice to her stomach, and to take ice;
and if there were much prostration, I advised small quantities of champagne
with ice. The prisoner was present during the whole interview? Yes. Did
you remain over night? I did. You dined there, I suppose? I did. Did
anything occur in the evening about Mrs Pritchard? While I was sitting in
the dining-room with the children, Dr Pritchard came down from the bed-room
and told me that Mrs Pritchard had been vomiting again, and requested me to
accompany him to the bed-room to see her, which I did. To her own bed-
room? Yes. You saw her? Yes, and she told me she had been again vomit-
ing. What did you do? Nothing. She at that time complained, I remember,
much of feeling a desire for food, and yet she could not retain it, and I pro-
posed to administer beef-tea injections to see if that would do any good. Was
she in bed when you left her? She was in bed at that time. Did you see her
next morning? I did. Was she down at breakfast? No. In her own bed-
room? Yes. Was this before or after breakfast? It would be the first thing
in the morning. How did you find her? Much the same as on the previous
night. Did anything particular occur? No. Did you return to Edinburgh
that evening? Yes; I stayed in Glasgow during the day. Did you see her
that day again? Yes; I saw her when I left, but nothing particular occurred
that made any impression upon me.

Lord Justice-Clerk—You went back to Edinburgh on the day after you
went to Glasgow? I did.

Mr Gifford—Was it you that took the message to Mrs Taylor to go to Glas-
gow? I did. Who gave you the message? Well, it was Mrs Pritchard's
desire that her mother should come through and wait upon her. It was my
proposal partly, and she acceded to it. You suggested it? Yes. There was
a large family, and I thought she required undivided attention. And you saw
Mrs Taylor? Yes; and she went next day.

Cross-examined by Mr Clark—You knew Dr and Mrs Pritchard well? Very
intimately. During the whole time they were married? Yes. Did they live
happily together? Exceedingly so. Down to the time Mrs Pritchard died?
To the last moment—at least to the last moment I saw her. When was the
last time you saw her? I saw Mrs Pritchard at Mrs Taylor's death. You
never heard of any disagreement whatever between them? The very reverse.

And they appeared to you to be very affectionate as husband and wife? Exceedingly so. I never heard him speak a disrespectful or unkind word of her or to her, and I never heard her speak a disrespectful or unkind word to him or of him. On the contrary, they both spoke in the absence of each other very kindly? Exceedingly so. How did he and Mrs Taylor stand? Well, he was Mrs Taylor's idol. Do you remember of Mrs Pritchard's body being brought to Edinburgh? I do. It was taken to her father's house in Lauder Road? Yes; I accompanied it to the house. Did the prisoner accompany it? He did. It was in a coffin, of course? It was. When it was taken to the house, was the coffin opened? It was, at Dr Pritchard's desire. For what purpose? To gratify the servants. They were very much attached to her, and it was done that they might have a last look at the body. What day was that? It was on Monday the 20th. Now, just tell us what passed on this occasion? The coffin was opened, and I was in the bed-room at the time it was opened, and the servants were in the room, and Mr Taylor was in the room: Dr Pritchard exhibited a great deal of good feeling on the occasion, and kissed her; and after some time we retired.

Mr Gifford—Were you well acquainted with Mrs Taylor? Yes. You had known her for a great many years? All my life. You were intimate with her? Yes. You visited her frequently? Oh, very. She was a person of temperate habits? Very temperate habits.

Lord Justice-Clerk—Have you seen much of Dr and Mrs Pritchard during the last two years? A good deal. And visited them frequently at Glasgow? Well, I was not in the habit of visiting them very frequently, but occasionally, and Mrs Pritchard and Dr Pritchard were frequently through to Edinburgh. You saw more of them at that time? Yes.

*Dr Douglas Maclagan.*—Solicitor-General—Dr Maclagan, you are Professor of Medical Jurisprudence in the University of Edinburgh? I am. And have been long engaged as a medical practitioner in Edinburgh? Yes. For many years? Yes. And I believe you have also devoted considerable attention to chemistry? Yes: in its toxicological relations. In connexion with poisons? Yes. On the 21st March last, I believe, you made a post-mortem examination of a body that was submitted to your examination, represented to be the body of Mary Jane Taylor or Pritchard? Yes. And you prepared a report of that post-mortem examination? Yes.

Dr Maclagan read the following report :—

*Medical Report by Drs Maclagan and Littlejohn of Post-mortem Examination of Body of Mrs Pritchard.*

EDINBURGH, *March* 21, 1865.

We, the undersigned, in virtue of a warrant of the Sheriff of Lanarkshire of yesterday's date, concurred in, of this date, by the Sheriff-Substitute of Edinburgh, at No. 1 Lauder Road, Grange, examined a body identified in our presence as that of Mary Jane Taylor or Pritchard, by the following witnesses :—Mary Raynor or Taylor, sister-in-law of the deceased; Michael Taylor, father of the deceased; Catherine Lattimer, servant; and Margaret Dickson, servant.

The body appeared to be that of a healthy woman, of about the age stated on the coffin-plate—thirty-nine years. It was free from putrescency. There was moderate post-mortem lividity and *rigor mortis*. Nothing was observed externally, except a yellow stain on the right side of the abdomen, looking like the remains of a sinapism. The expression was placid. The pupils of the eyes natural.

*Head.*—The vessels of the scalp were not loaded with blood. The veins on the surface of the brain were moderately full, especially at the posterior part. There was considerable effusion of serum under the arachnoid membrane on the top of the brain, but not at the base. The brain itself was healthy, both as regards vascularity and consistence. The ventricles contained only a small quantity of serum.

*Organs of Respiration and Circulation.*—The windpipe was healthy. The right lung was quite healthy. The left lung was slightly adherent to the walls of the chest, at its apex, where there was a firm mass of old tubercular deposit, of the size of a hazel nut, of cheesy consistence, and unaccompanied by any traces of recent morbid action. There was a small amount of serum in the pericardium. The heart contained a little fluid blood in both cavities; rather more in the right than in the left ventricle. In the right ventricle there was a small fibrous clot. The heart and its valves were healthy.

*Organs of Digestion.*—The gums and mucous membrane of the mouth were exsanguine, the lips dry, the pharynx and gullet perfectly healthy. The walls of the abdomen were loaded with fat, and so were the omentum and mesentery. The viscera presented no morbid appearance externally. The liver was natural; the gall-bladder full of bile. The spleen was healthy. The kidneys slightly congested. The stomach contained about three drachms of pinkish-grey ropy fluid, with some small masses of tenacious mucus mixed with it. The mucous membrane was generally healthy, but on the posterior wall, near to the cardia, there was a patch of punctiform redness over a space of two inches square. The small intestines were lined with light grey mucus. The colon and rectum contained some yellow feculent matter, which nowhere was of solid consistence. The ileum, for about three inches of its length, at a part beginning about three inches above its termination in the colon, was closely contracted on itself. The mucous membrane of the rectum, through-out a good part of its extent, presented a superficial dark discoloration, as if some black pigment were embedded in its substance. There was slight ramiform injection of the greater part of the mucous membrane of the rectum. There were several small patches of the same appearance at various points throughout the colon, and a few spots of similar vascular injection in the small intestines.

*Urinary and Genital Apparatus.*—The urinary bladder contained about eight ounces of brownish yellow urine; the womb and its appendages presented no morbid appearance beyond a slight ulceration of the cervix uteri.

We have to report that this body presented no appearances of recent morbid action, beyond a certain amount of irritation of the alimentary canal, and nothing at all capable of accounting for death. We have therefore secured the alimentary canal and its contents, the heart and some of the blood, the liver, the spleen, the left kidney, and the urine, in order that these may be submitted to chemical analysis.

(Signed)     DOUGLAS MACLAGAN.
             HENRY D. LITTLEJOHN.

Solicitor-General—That is a true report? That is a true report.

Read your chemical report, Dr Maclagan, now. Dr Maclagan read a part of his report, dated 11th April 1865, but he was interrupted in the reading in order that Drs Gamgee and Littlejohn might speak to the parts of the report referring to what they had done during Dr Maclagan's absence in London.

[Dr Arthur Gamgee, assistant to Dr Maclagan, and Dr Littlejohn, were here called, and gave evidence that the statement in Dr Maclagan's report of what they had done in his absence in London was a correct statement.]

Dr Maclagan was again called, and read the remaining portions of the report. The following is a complete copy of the report :—

*Chemical Report by Dr Maclagan.    Death of Mrs Pritchard.*

EDINBURGH, 11*th April* 1865.

I have subjected to chemical examination the various organs, and contents of organs, re-moved by Dr Littlejohn and myself from the body of Mrs Mary Jane Taylor or Pritchard, at the post-mortem examination on 21st March, and I have to report the following as the results which I have obtained :—

It having been stated to me that antimony was suspected in this case, immediately on returning from the post-mortem examination, I made a trial experiment in presence of Dr Littlejohn, and my assistant, Dr Arthur Gamgee, with three drachms of the urine, and obtained from this unmistakable evidence of the presence of antimony. Being obliged, in consequence of the death of a relative, to go to London, and having, by the above experiment, ascertained that my researches must be directed towards the discovery of antimony, I requested Dr Gamgee, in conjunction with Dr Littlejohn, to carry on the following preliminary process in my absence. The whole contents of the intestines were evaporated to dryness on a water bath, so as to obtain a solid residue ; one-half of this residue was digested with water acidulated with tartaric acid, and filtered, by which a solution measuring two ounces and five drachms was obtained, in which any antimony present in the intestines would be found. One ounce of this fluid was subjected to a stream of sulphuretted hydrogen gas, and the orange-yellow precipitate which formed was collected on a filter and washed. This precipitate, and the remainder of the tartaric acid solution, were reserved for my examination on my return to Edinburgh on the 24th March. I then subjected these materials to the following examination. The orange-yellow precipitate was boiled in a tube with pure hydrochloric acid, and the solution thus obtained was mixed with water, when a white precipitate formed. The fluid containing this precipitate was again subjected to a stream of sulphuretted hydrogen gas, and again gave a deposit of an orange-yellow colour. One fluid drachm of the tartaric acid solu-tion was treated by Reinsch's method, and another fluid drachm was treated by Marsh's process. By each of these well-known methods, and thus operating upon a quantity of fluid corresponding to a forty-second part of the contents of the intestines, I obtained

unequivocal evidence of the presence of antimony.   By digesting a small quantity of the
dried residue of the intestinal contents with distilled water, filtering and subjecting the
filtrate to Reinsch's process, I readily ascertained that the antimony was here present in the
form of a compound soluble in water.   There are only two preparations of antimony
occurring in commerce which are soluble in water; the one of these, the chloride, is a strongly
acid, dark brown, corrosive fluid, totally unsuited for internal administration; the other is
what is known scientifically as tartarised antimony, and popularly as tartar emetic, a colour-
less substance, possessed of comparatively little taste, and in daily use as a medicinal agent.
I have no doubt, and shall assume in the following statements, that the antimony found in
Mrs Pritchard's body was taken in this form.   The remainder of the acid solution, amount-
ing to one ounce and three drachms, was subjected to a process intended to determine the
quantity of antimony present in the contents of the intestines; but though the presence of
this metal was determined with the greatest facility, I found that the amount yielded by the
materials which I used was too small to enable me to weigh it with sufficient accuracy.   I
also made an experiment with the contents of the intestines, directed towards the discovery
of vegetable poisons.   It is sufficient on this subject to say, that the result was entirely
negative.   I then subjected to analysis the following fluids and solids removed from the
body of Mrs Pritchard.

1. *Contents of Stomach.*—These amounted to little more than half-an ounce, and were free
from all odour of any poisonous drug.   They were subjected, in the first place, to what is
known as " Stas's process," for the separation of vegetable poisons, but not a trace of any of
these was detected.   The whole residues of this operation were preserved and subjected to
examination for antimony, but none was found.

2. *The Urine.*—The presence of antimony having been already ascertained in this secre-
tion, the remainder, amounting to seven ounces, was employed to determine its quantity.
The process followed here was a well-known one, by which the antimony is obtained in the
form of sulphuret, after destroying the organic matter by means of hydrochloric acid and
chlorate of potash.   The quantity of sulphuret was readily weighed, and found to be rather
more than one-tenth of a grain (0·1078 grain).   This corresponds to nearly one-fourth of a
grain (·218 grain) of tartar-emetic.

3. *The Bile.*—A little more than half-an-ounce of this fluid was obtained from the gall-
bladder.   By Reinsch's process fifty minims readily gave an antimonial deposit.   The
remainder of the bile, amounting to four drachms, was used to determine the amount of
antimony in it, and it yielded sulphuret of antimony, corresponding to more than one-tenth
of a grain (0·121 grain) of tartar-emetic.

4. *The Blood.*—The total quantity was six and a-half ounces.   One ounce was subjected
to Reinsch's process, and readily gave evidence of the presence of antimony.

5. *The Liver.*—The weight of this organ was found to be thirty-six ounces, a portion
weighing less than four ounces (1460 grains) was subjected to Reinsch's process, and a suf-
ficient amount of antimony was found to coat rather more than four square inches of
copper foil.   Although the existence in the liver of an abundance of antimony was to my
mind satisfactorily established by the appearance of the coated copper foil, I deemed it
right to employ a portion of the product thus obtained for confirming, by another test,
the presence of antimony in the body of Mrs Pritchard.   For this purpose a piece of the
copper foil, one inch long and half-an-inch broad, was boiled in a dilute solution of pure
caustic potash, the copper foil being from time to time freely exposed to the air.   The coat-
ing disappeared from the copper, and a solution was obtained, which, when acidulated with
hydrochloric acid, and subjected to a stream of sulphuretted hydrogen gas, gave an orange
precipitate, which again was dissolved in strong hydrochloric acid; this acid solution gave,
on being mixed with water, a white turbidity, which again was turned orange by sul-
phuretted hydrogen.   Another portion of the coated foil, measuring half-an-inch square,
was heated in a fine glass tube, with a view to ascertaining the presence or absence of arsenic,
which occasionally exists as an impurity in compounds of antimony.   No arsenic, however,
was found, nor had any been observed in the previous trial of the contents of the intestines
by Marsh's process.   Finding antimony thus abundantly in the liver, I made an experi-
ment to determine its actual quantity in that organ.   For this purpose I operated upon one
thousand grains, by the process described above for determining the presence of antimony,
and obtained an amount of antimony in the state of sulphuret (0·1231 grain) corresponding
to a quarter of a grain (0·25 grain) of tartar-emetic; the amount contained in the whole
liver being almost exactly four grains (3·93 grains).

I next examined the remainder of the solid organs removed from the body of Mrs Prit-
chard, and have to state, that I have found more or less of antimony in the whole of them.
I operated in no instance upon more than 350 grains, in every case following Reinsch's
process.   I thus obtained the evidence of the presence of antimony in the spleen, kidney,
muscular substance of the heart, coats of the stomach, coats of the rectum, brain, and
uterus.   On the 29th of March I received from the hands of John Murray, sheriff officer,
Glasgow, two parcels of clothes, with sealed labels attached to them, with a view to my
examining some stains upon them.   One of these labels bore, " Police Office, Glasgow,
Central District, 23d March 1865.   Found in the house of Dr Pritchard, 131 Sauchiehall
Street, and referred to in the case of himself.   (Signed) A. M'CALL, AUDLEY THOMSON."
The label was signed by John Murray in my presence, and initialed by me.   On the back

of the label was the following list of the articles attached to it :—"One night-dress, 1 chemise, 1 night-cap, 3 handkerchiefs, 1 knitted woollen semet, a pair of worsted stockings, 1 woollen polka." The other label was similarly dated and signed, the list on the back being—" 2 sheets, 2 pillow cases, 2 towels, 1 toilet cover." I examined such of the stains on these articles as appeared of importance, confining my experiments to a search for anti- mony, and I have to state, that whilst with many of the stains the result was entirely nega- tive, I found antimony on the following :—1st, On the chemise, from a stain obviously of discharge from the bowels, and which had been marked by me A. 2d, On one of the sheets, distinguished by me as No. 1, in a stain marked by me B. 3d, On the other sheet, distinguished by me as No. 2, in a stain obviously of urine, marked by me A. 4th, On a toilet cover, in a stain of a reddish colour, looking like a wine stain. It is hardly necessary to state that the materials employed in all these chemical operations had been ascertained to be entirely free from all metallic impurity. From the experiments, the details of which are given above, I have been led to the following conclusions :—1st, That Mrs Pritchard had taken a large quantity of antimony in the form of tartar emetic. 2d, That having regard to the absence in her case of any morbid appearance sufficient to account for death, and to the presence in it of a large quantity of a substance known to be capable of destroy- ing life, her death must be ascribed to the action of antimony. 3d, That it is most unlikely that this poison was taken in a single large dose. Had this been the case, I should have expected to have found some more decided evidence of irritant action in the mouth, throat, or alimentary canal. 4th, That from the extent to which the whole organs and fluids of the body were impregnated with it, it must have been taken in repeated doses, the aggre- gate of which must have amounted to a large quantity. 5th, That from the large amount found in the liver, from its ready detection in the blood, and from its being found passing so copiously out of the body by the bile and urine, it is probable that some of the poison had been taken at no greater interval than a period of a few days previous to death. 6th, That I am inclined to believe that it had not been administered, at all events in any great quantity, within a few hours of her death. Had this been the case, I would have expected to have found at least some traces of it in the contents of the stomach, and more in the contents of the intestines; whereas none was found in the former, and the amount found in the latter seems to be amply accounted for by the bile impregnated with the poison discharged into them from the liver. 7th, That the period over which the administration had extended cannot be determined by mere chemical investigation, but must be deduced from the history of the case, with which I am unacquainted.          DOUGLAS MACLAGAN.

The Solicitor-General—That is a true report? Yes.

Dr Maclagan then read the following report, giving the results of a post- mortem examination of the body of Mrs Taylor made by himself and Dr Littlejohn :—

*Medical Report by Drs Maclagan and Littlejohn of Post-mortem Examination of Body of Mrs Taylor.*

EDINBURGH, *30th March* 1865.

In virtue of a warrant of the Sheriff of Lanarkshire, dated 28th March 1865, and con- curred in, on 29th March, by the Sheriff-Substitute of Mid-Lothian, we this day, at the Grange Cemetery, examined the body of Mrs Jane Taylor, who was buried there at the beginning of the present month. The coffin was exhumed in our presence, and was found to bear on the plate " Jane Taylor, died 25th February 1865, aged 71 years." A portion of the earth from above the coffin was secured for chemical examination. The coffin, and subsequently the features of the deceased, were identified in our presence by the following witnesses :—Mr Michael Taylor ; Dr M. W. Taylor ; Margaret Dickson ; James Thomson ; John Moffat ; David Glen ; and Robert Grant. The coffin was entire. The following were the appearances observed by us in the body of Mrs Taylor :—

*Externally,* it presented the appearance of great freshness. There was some red post- mortem coloration of the shoulders and back. The abdomen was slightly green over a space of not more than four inches by three. There was a little mouldiness on the face, but there was no putrefactive disfigurement of the countenance. The expression was placid, and a little florid colour was visible on the cheeks.

*Head.*—The scalp was not congested. The dura mater was firmly adherent to the skull at several points, especially at the frontal bone, and in the right temporal fossa, at which places the inner table of the skull exhibited rough elevations and depressions, to which the dura mater was attached. These were of old standing. A small quantity of fluid blood, which had exuded from a vein torn in removing the skull-cap, was found on the upper part of a posterior lobe of the left hemisphere. It was entirely a post-mortem oc- currence. The blood was at once washed away by a little water poured gently upon it, and the brain and membrane beneath it were found quite in a natural state. There was a small amount of sub-arachnoid effusion, obviously also a post-mortem phenomenon, as it was found only at the back part of the brain, and was unaccompanied by any appearance of inflammatory action. The blood-vessels of the brain were not congested. The ventri- cles contained less than a teaspoonful of clear serum. The brain throughout was remark-

ably fresh. Every part of it was most carefully scrutinized, but at all points it was found perfectly healthy, both externally and internally, equally as regards consistence, colour, and structure. There was a trifling amount of atheromatous deposit on the coats of the vessels at the base of the brain, but much less than might have been expected in a person seventy-one years of age.

*Organs of Respiration and Circulation.*—The mucous membrane of the trachea was little, if at all, altered by putrefaction, being only slightly reddened, and lined by a little colourless mucus. The lungs were remarkably healthy, there being no trace of anything noteworthy about them, except some old adhesions of the left pleura. The pericardium was healthy, and contained no serum. The heart was large, and weighed sixteen ounces. It had a considerable layer of fat over its surface, was slightly dilated, particularly on the right side, but all its valves were quite healthy. There was about one ounce and a-half of fluid blood, along with a fibrinous coagulum in the right ventricle. The left ventricle was almost empty. The venæ cavæ contained half coagulated blood. The aorta was quite free from atheromatous deposit.

*Organs of Digestion.*—The gums and mucous membrane of the mouth, the pharynx, and gullet, were perfectly healthy. The walls of the abdomen were loaded with fat, and so were the omentum and mesentery. The stomach contained five ounces of turbid yellow fluid, and some small masses of undigested food. The mucous membrane was free from disease, and presented only some post-mortem blackening at several points, and a yellow coloration from contact with the contents. The intestines presented diffuse post-mortem redness externally at several points, but nowhere exhibited any distinct morbid appearances. A portion of the ileum, about four inches in length, and about three feet above the cæcum, was closely contracted upon itself. The small intestines contained only a lining of pinkish-gray mucus. There was a small amount of yellow fluid fæces in the cæcum and rectum. The large intestines elsewhere contained only a lining of pinkish-grey mucus. The mucous membrane of the intestines everywhere was perfectly healthy. The rectum at one or two points, especially close to the anus, presented slightly the appearance of a black pigment matter embedded in its mucous membrane. The other organs of the abdomen were healthy.

*Urinary and Genital Apparatus.*—The bladder was contracted, and contained only a little mucus. The uterus and its appendages were healthy.

We have to report that we have not been able to discover in the body of Mrs Taylor any morbid appearance capable of accounting for her death, and are of opinion that the cause of her death cannot be determined without chemical analysis. We have therefore secured, for this purpose, the alimentary canal and its contents, the heart and some of the blood, the liver, the spleen, the kidneys, the bladder and uterus, and a portion of the brain, which have been left in the custody of Dr Maclagan.

<div align="right">DOUGLAS MACLAGAN.<br>HENRY D. LITTLEJOHN.</div>

Lord Justice-Clerk—That report is signed by yourself and Dr Littlejohn? It is.

Solicitor-General—And it is a true report? Yes.

Dr Maclagan next read the following report of the chemical analysis of the various things mentioned at the end of last report :—

<div align="center">*Chemical Report by Dr Maclagan. Death of Mrs Taylor.*</div>

<div align="right">EDINBURGH, 13th April 1865.</div>

I have subjected to chemical examination the various organs and fluids removed by Dr Littlejohn and myself from the body of Mrs Jane Taylor at our post-mortem examination on 30th March, and have to report on them as follows :—

*Contents of the Stomach.*—These, which amounted to five ounces, were, in the first place, subjected to the process known as that of Stas, for the detection of the active principles of vegetable poisons. The result, however, was that no trace of any of these was detected. A special test was also applied, with the view of discovering in the stomach meconic acid, one of the characteristic constituents of opium, but in this also I was unsuccessful. The residues of the above process were reserved to be tested for metallic poisons, and a pre-liminary trial, by Reinsch's method, having revealed in the contents of the stomach the presence of antimony, I subjected the whole to a process by which I was enabled to determine the amount of this metal. The process was as follows :—The materials were boiled with pure hydrochloric acid and copper foil, so long as the latter continued to receive on its polished surface a deposit of antimony. The foil thus coated was boiled with a weak solution of pure potash, the foil being from time to time exposed to the air, and the antimony was thus dissolved. The fluid, after being acidulated with hydrochloric acid, was subjected to a current of sulphuretted hydrogen gas, and yielded an orange-coloured deposit of sulphuret of antimony. This was further purified by dissolving it in a weak solution of sulphide of sodium, from which it was again precipitated by hydrochloric acid and weighed. Assuming, for reasons to be afterwards given, that the antimony existed in the form of

tartar emetic, the amount of this represented by the sulphuret which I obtained from the stomach was a little more than a quarter of a grain (0·279).

*Contents of Intestines.*—The whole contents were evaporated at a gentle heat on the water bath, and a residue obtained, which weighed four hundred and thirty grains.   Ten grains of this residue, on being subjected to Reinsch's process, yielded a characteristic deposit of antimony.   To determine in what form this antimony existed, other ten grains were treated with distilled water, the solution filtered, and fluid subjected to Reinsch's process.   A characteristic antimonial deposit was obtained, thus proving that this metal was present in a soluble form.   There are only two soluble forms of antimony met with in commerce.   One of these, the chloride, is a dark-coloured, acid, corrosive fluid, totally unsuited for internal administration.   The other is what is known scientifically as tartarised antimony, and popularly as tartar emetic, a colourless substance possessed of comparatively little taste, and in daily use as a medicinal agent.   I have no doubt that it was in this last form that the antimony had been taken which I found in the alimentary canal of Mrs Taylor.   I endeavoured to determine, by the process formerly mentioned, the amount of the antimony in the contents of the intestines, and for this purpose one hundred grains of the dried residue were boiled with hydrochloric acid and copper foil.   The amount of foil coated was one and a-half square inch, but the deposit was too small to enable me with confidence to make it the subject of a quantitative determination.

A piece of the coated copper, half-an-inch square, was heated in a tube to ascertain the presence or absence of arsenic, which occasionally occurs as an impurity in tartar emetic, but none was found.

*The Blood.*—Of this, six and a-half ounces were obtained at the post-mortem examination. One ounce was subjected to Reinsch's process, and a characteristic antimonial deposit was obtained.

*The Liver.*—This organ weighed two pounds six and a-half ounces.   Two hundred and twenty grains were subjected to Reinsch's process, and two pieces of copper foil were coated with a characteristic deposit of antimony.   One of these was made use of to confirm, though this was not necessary, the fact, that the deposit on it was antimony.   For this purpose it was, by the process already described, converted into sulphuret, which again was dissolved in strong hydrochloric acid.   The solution thus obtained became milky on the addition of water, and on being a second time exposed to sulphuretted hydrogen gas again yielded the orange-coloured sulphuret.   These reactions are conclusive as to the deposit on the foil being antimony.   I determined the amount of antimony in the liver.   For this purpose I operated upon a thousand grains by the method already described, and obtained a quantity of sulphuret, indicating that the liver contained rather more than one grain and a tenth (1·151 grain) of tartar emetic.   I also examined the other solid organs and tissues removed from Mrs Taylor's body, in each case following Reinsch's method, and in each case obtaining on the copper a characteristic antimonial deposit.   I thus found that there was more or less of antimony present in the muscular substance of the heart, the spleen, the kidney, the coats of the stomach, the coats of the rectum, the uterus, and the brain.

*Lastly*, As Mrs Taylor's body had been exhumed, I thought it my duty to examine some of the earth in which it had been interred, although this was superfluous, from the facts that the soil of the cemetery was dry and the coffin entire.   For this purpose I boiled eight ounces of the earth in water, filtered and concentrated the decoction, and subjected it to Reinsch's process, but it was found not to contain a trace of soluble antimony, and was therefore incapable of impregnating with this metal any body buried in it.

Cross-examined by Mr A. R. Clark—I understand that the first experiment you made was the experiment made upon the urine?   Yes.   When you obtained unmistakable evidence of the presence of antimony, by what process did you arrive at that conclusion?   By performing Reinsch's process, and getting the characteristic violet deposit upon the copper.   You did not carry it further? No.   That is the way you obtained unmistakable evidence of the presence of antimony?   Yes.   In your opinion as a chemist is that conclusive proof of the presence of antimony?   I should not consider a case thoroughly worked out on that alone, but as a trial experiment, to my mind it was quite unmistakable. Is it unmistakable?   I think so.   Being unmistakable, is there any necessity of going further?   It is better, I think, in every case to carry assurance to the minds of other people by adding a further corroborative test.   I understand that in your opinion the characteristic deposit upon the copper is conclusive of the presence of antimony?   Yes; quite satisfactory to my mind.   I understand that Reinsch's process consists in producing upon the copper foil a certain coloured deposit?   Yes.   That is the beginning and the end of the process?   Yes; properly speaking.   That deposit which you procured upon the copper may be subsequently tested in other ways, but that is not an essential part of Reinsch's process?   No.   But I understand you proceeded so far as to get this deposit on the copper by Reinsch's test, which you held

to afford unmistakable evidence of antimony? Yes, in the urine. After you had done so you had to leave for London, and the preparatory work was done by Drs Gamgee and Littlejohn. Were the rest of the experiments conducted by yourself? Yes. The whole of them? Yes. From the beginning to the end? Yes. You performed the experiments upon the contents of the intestines with a view to enable you to determine the quantity of antimony? Yes. The result was that you found a quantity so small that you could not determine it by weight? Yes; by that particular process. The exact quantity in the intestines was so small that you could not make it out? I could not make it out as a quantity. I could not weigh it satisfactorily. In these intestines what did you operate upon? Upon the remains of the fluid that had been prepared in my absence by Dr Gamgee and Dr Littlejohn. Upon nothing else? Nothing else. Only upon the solution which Dr Gamgee gave you? Yes. Upon a portion of it? Yes. Now, I should like you to tell me whether you handed any portion of the solution to Dr Penny? None of the solution. Did you hand any part of the intestines to Dr Penny? Yes; some of the dried residue. You yourself did not know how the solution was prepared, or in what way the previous preparatory process had been carried through of preparing it? I was merely informed that they had followed the instructions which I had given when I went away. Would you tell me, referring to your report, what was the amount of antimony that you found in the liver? (After referring to report.) A quarter of a grain to the thousand grains, corresponding to tartar of emetic. But of sulphuret of antimony? ·1234 to a thousand grains. In making these experiments you did not find any traces of mercury? I did not; not at the time.

Solicitor-General—You gave to Dr Penny a variety of articles that were taken from the body of Mrs Pritchard, and also from the body of Mrs Taylor? Yes. Just be kind enough to tell us what you handed to him from Mrs Pritchard's body. Yes; a note made at the time by myself contains a short record of the proceedings. I delivered to Dr Penny at the University, from the body of Mrs Pritchard—(1) a portion of the rectum, (2) the pyloric half of the stomach, (3) about half a kidney, (4) a portion (half) of the spleen, (5) a portion of the heart, (6) a portion of the brain, (7) 255 grains of dried contents of intestines. 225 or 255? Well, I am not very distinct about that. You gave him upwards of 200 grains? Yes. Then a portion of liver, and a portion of blood. In glass bottles? Yes; all the things were either in jars or bottles. You handed them over to Dr Penny in your laboratory at the University? Yes. Of Mrs Taylor's body you delivered to Dr Penny what articles? First, a portion of liver; second, a portion of heart; then, one kidney; then, 100 grains of dried contents of intestines; about one-half of the stomach; a portion of the rectum; and a portion of the blood. I believe you found no mercury in your examination of the contents of the intestines? No. You were requested subsequently to make an examination of a part of the residue of the contents of the intestines of Mrs Pritchard? Yes. When was that? Last week. What I operated upon were the remains after the process that had been conducted by Dr Gamgee and Dr Littlejohn in my absence, and which had remained locked up.

Lord Justice-Clerk—Tell me what you made an examination of? It was the solid residue that had remained after the tartaric acid fluid had been filtered through.

Solicitor-General—And with what result? I determined the presence of mercury, and found a considerable quantity of antimony remaining in it. Just give us as accurately as you can the result, and state how much antimony was found? I got a clear fluid by operating upon that residue with chlorate of potash and hydrochloric acid; and then passing sulphuretted hydrogen, I got a precipitate of a dirty orange colour, which was collected, washed, and boiled in strong hydrochloric acid. The yellow colour disappeared, and the precipitate became black. The hydrochloric solution was then mixed with water and tartaric acid, and it gave an orange precipitate which, when collected and weighed, amounted to 0·082, equal on the whole to 1·265 of sulphuret of anti-

mony.  Is that one grain and 265 decimal parts of a grain you mean?  Yes.
That is about a grain and a quarter is it not?  Yes; rather more.

Lord Justice-Clerk—In what quantity of solid residue?  In the whole that
remained.

Solicitor-General—What would be the weight of it?  It would be impossible
to estimate the weight, because it had been in water, and had then been kept
in a jar.  It was not a thing to be weighed.  In short, it was more antimony
than you found in the contents of the intestines after the precipitate obtained
by Dr Littlejohn and Dr Gamgee?  Quite so.  Dr Littlejohn and Dr Gamgee
treated the solid residue of one-half of the contents of the intestines, in my
absence.  They filtered the clear tartaric acid, and the result is given in my
first report.  The solid matter of that not dissolved by the tartaric acid was
kept on the filter.  It was that that was operated upon, and therefore it was
the solid residue of the one-half of the intestines, minus, of course, what had
been dissolved by the tartaric acid.  Then it was so much antimony which
their process had not extracted?  Yes.

Lord Justice-Clerk—It was about a grain and a quarter?  Yes; a grain and
a quarter of sulphuret.  And what is that in tartar emetic?  It is equal to
2·56 of tartar emetic.

Solicitor-General—That is rather more than two and a-half of tartar emetic?
Yes, rather more than two and a-half.  Now, you have spoken of the precipi-
tate you obtained becoming black?  Yes.  What did that indicate?  It indi-
cated the presence of sulphuret of mercury.  Did you make a quantitative
analysis to determine the amount?  Yes.  How much mercury did you find?
It was 0·0509 grain—5-100dth parts of a grain—the twentieth of a grain.

Lord Justice-Clerk—That was mercury—in what form?  I cannot tell in
what form.

Solicitor-General—Did you estimate the total quantity of tartar emetic con-
tained in the whole of the intestines from what you recovered?  I made a
corroborative experiment along with that which I have just narrated on a little
fresh portion of the dried contents.  I took fifty grains of matter that had never
been operated upon by any person before—what had been got by simple
evaporating to dryness.  I worked by the process of chlorate of potsah as
before, and I got 0·138 of sulphuret of antimony, corresponding to 0·280 of
tartar emetic.  And what was the weight of the whole dried contents of the
intestines?  1020 grains generally.

Lord Justice-Clerk—So that the whole tartar emetic was?  In the whole of
the contents of the intestines it would be 5·712.

Solicitor-General—What do you mean by the contents of the intestines?
That which had originally been got out of the intestinal canal from the stomach
down to the rectum, and which had been evaporated to dryness as the first
stage of the proceedings.

Cross-examined by Mr Clark—Did you find any mercury in making that
last experiment?  I did.  What was the amount?  The amount in the experi-
ment with the fifty grains of sulphuret of mercury was 0·0308 grain—3-100dth
parts of a grain.  In conducting your original experiments, did you carry any of
them further than the mere obtaining the deposit on the foil?  Yes; I boiled
the copper foil in potash, so as to get the sulphuret of antimony.  In all cases?
No.  Speaking of Mrs Pritchard's body, in what cases?  It was partly on the
tai aric acid solution.  What do you mean by partly?  That process was fol-
lowed out of testing the antimony by means of the solution in potash, both
with the contents of the intestines and with the liver.  In the other cases you
rested satisfied with obtaining the deposit on the copper?  I think in all the
other cases.  In making these experiments which you have referred to upon
the bed-clothes, and so on, did you carry your test further than the coloured
deposit?  No.  In the case of the examination of Mrs Taylor, did you proceed
to the close of the experiments, or did you rest satisfied with the coloured
deposit?  I carried out the experiments in regard to the contents of the intes-
tines and the liver.  In the other cases you did not?  No.

*Dr Frederick Penny.*—Solicitor-General—You are Professor of Chemistry in the Andersonian University of Glasgow? Yes. You have, I believe, given much attention to chemistry for many years? I have. Including great attention to the subject of poisons? Yes. You received from Professor Maclagan of Edinburgh the things which you heard mentioned in the witness-box a little ago? I did; on the 10th April last. You made a chemical analysis of these? I did. Dr Penny then read the following report :—

*Report of Analysis in the Case of the Death of Mrs Pritchard.*

ANDERSONIAN UNIVERSITY, GLASGOW, *9th May* 1865.

On Monday, the 10th of April last, I received from Dr Douglas Maclagan, at his laboratory in Edinburgh, the following articles, all of which were certified to have been taken from the body of Mrs Pritchard :—

> No. 1. Pyloric half of stomach. ⎫
> „  2. Nearly half of kidney.  ⎬ These four articles were contained
> „  3. Portion of rectum.   ⎪  in a stoneware jar.
> „  4. Portion of spleen.   ⎭
> „  5. Portion of liver in a glass jar.
> „  6. Portion of brain in a glass jar.
> „  7. Portion of heart in a glass bottle.
> „  8. Portion of blood in a glass bottle.
> „  9. 225 grains of dried contents of intestines in a glass bottle.

The several vessels containing these articles were securely closed, and duly labelled. I brought them direct to Glasgow on the day referred to, and, in accordance with instructions from the Crown-Agent, Edinburgh, I have, at my own laboratory, carefully analyzed and chemically examined each and all of the said articles, for the purpose of ascertaining whether they contained any poisonous substance.

*Dried Contents of Intestines.*—The investigation was commenced with the contents of the intestines. From the information which I received, my attention was particularly directed to the detection of antimony ; but, deeming it desirable to search for the presence of other metallic poisons, I subjected a portion of the said contents to the usual course of qualitative analysis for the detection of various metals of a poisonous nature. The results of this exhaustive examination gave distinct indications of the presence of antimony and mercury. For the purpose of establishing unequivocally the presence of these metals, and at the same time of estimating their quantities respectively, the following experiments were then carried out :—A known quantity of the said contents was dissolved with the usual precautions in hydrochloric acid, with the addition of chlorate of potash, and the solution being properly diluted with water, was subjected to the action of sulphuretted hydrogen gas. An abundant black precipitate was obtained, which, by proper treatment, was separated into sulphide of antimony and sulphide of mercury. The sulphide of antimony, which was obtained of a fine orange-red colour, was washed, dried, and weighed. Its weight corresponded to a quantity of metallic antimony equal to 2·1 grains in one thousand parts of the dried contents of the intestines. The same sulphide was found to be readily soluble in sulphide of ammonium, and also in hydrochloric acid, and the acid solution, when poured into water, gave a white precipitate, and when boiled with copper-ribbon, deposited a violet-coloured coating on the surface of the copper. The coated copper, on being heated in a glass tube, gave no distinct crystalline sublimate. All these results are eminently characteristic of sulphide of antimony when thus treated. The sulphide of mercury was black ; it was dissolved in nitric and hydrochloric acids, and the solution being appropriately prepared, was treated with chloride of tin. A precipitate of metallic mercury was obtained, which, after being suitably washed and dried, was found to correspond to three grains in one thousand grains of the dried contents. A portion of this precipitate, on being heated in a dry glass tube, gave a sublimate of mercury in brilliant and mirror-like globules. Another portion was dissolved in nitric and hydrochloric acids, and the solution, after the removal of the excess of acid, was tested with caustic potash, ammonia, and iodide of potassium, and with other re-agents and methods for the detection of mercury. In every case the peculiar reaction of that metal was satisfactorily produced. In order to corroborate the results of the foregoing experiments, another portion of the said contents of the intestines was subjected to Reinsch's process, and this was supplemented by Marsh's process. By the former process copper foil was coated with a deposit which presented the peculiar violet colour and the general appearance of metallic antimony ; and, by continuing the process till the copper foil ceased to be coated and the liquid was exhausted of separable matter, pieces of the copper foil were obtained with a grey coating, which, on being rubbed, became silvery and lustrous, like metallic mercury when similarly deposited. The coated copper was then digested in an aqueous solution of pure potash, and after being well washed and dried, it was cautiously heated in a small tube. A sublimate of metallic mercury in minute lustrous globules was obtained ; and this sublimate, when dissolved in the proper acids, yielded with the well-known tests—the chemical reactions of metallic mercury. The potash solution from the coated copper was then treated in the usual manner for the separation of antimony in the form of the orange-

red sulphide, which, when collected and weighed, was found to correspond very closely with the proportion obtained by the process previously described. The sulphide of antimony was soluble in sulphide of ammonium and in hydrochloric acid. The solution in hydrochloric acid gave a white precipitate when poured into water, and on being subjected to Marsh's process, deposited on a porcelain slab the characteristic stains of metallic antimony. In another experiment, a portion of the said contents was distilled with concentrated hydrochloric acid, and antimony was detected in the distillate. With a view of ascertaining whether the antimony and mercury existed in a form soluble in water, in the said contents of the intestines, a portion of these was macerated in distilled water, and the solution carefully tested for both metals. The presence of antimony was distinctly detected, but no mercury. The said contents were also examined by Stas's method for aconite, morphia, and other organic poisons, but not the slightest evidence of the presence of such poisons was obtained.

*Stomach.*—The stomach was analyzed by the same methods as those applied to the dried contents of the intestines. It yielded antimony in appreciable proportions, but no mercury. The quantity of antimony obtained was equal to ·05 of a grain in one thousand parts. The stomach was also minutely examined for morphia and aconite, but not a trace of these substances was obtained.

*Liver.*—The liver was found to contain antimony, but no mercury. The proportion of antimony amounted to one-tenth of a grain in one thousand grains.

*Spleen.*—The spleen yielded antimony in about the same proportion as that found in the liver, and it also contained mercury in well-marked quantity.

*Kidney.*—The kidney yielded about the same proportion of antimony as the liver, and it was also found to contain an extremely minute trace of mercury.

*Heart.*—The heart yielded antimony in a proportion rather larger than that found in the liver. It also contained mercury in smaller quantity than the spleen.

*Brain.*—The brain contained antimony in less quantity than the liver, but it yielded no mercury.

*Blood.*—The blood contained a small quantity of antimony, and also a faint trace of mercury.

*Rectum.*—The rectum yielded antimony, but in less quantity than the liver. It afforded no indications of mercury.

Having deliberately considered the results of my experiments upon the articles subjected to analysis, I have arrived at the following conclusions :—

1st, That all the parts of the body examined by me contained antimony. 2d, That in the dried contents of the intestines the antimony was partly in a form soluble in water, and most likely in the state of tartar emetic or tartarised antimony. In the liver, kidney, and the other viscera, the antimony was deposited in a state insoluble in water. 3d, That the contents of the intestines contained the largest proportion of antimony, next the heart, then the liver, kidney, and spleen ; less in the stomach ; and the smallest quantity in the rectum, brain, and blood. Not knowing the total weight either of the contents of the intestines, or of the several organs here enumerated, I was unable to calculate the total quantity of antimony in these matters, either separately or conjoined. 4th, That the contents of the intestines, the spleen, the heart, the blood, and the kidney, contained mercury ; but that none of this metal was present in the liver, stomach, rectum, and brain. That, in all these matters, the mercury was in a state insoluble in water ; and this result is quite consistent with the known property of mercury to form insoluble combinations with animal substances, even though it had been taken or administered in a soluble form during life. 5th, That the largest quantity of mercury was contained in the contents of the intestines, next in the spleen and heart, and extremely minute traces in the blood and kidney. 6th, That the presence of antimony and mercury in the contents of the intestines, indicates that these metals were being passed from the deceased up to the time of death. 7th, That no other metallic poison was contained in the matter examined. 8th, That no aconite, morphia, or other vegetable poison, discoverable by chemical process, was contained either in the contents of the intestines, or in the stomach. 9th, Not having detected any organic poison, either in the said contents of the intestines or in the stomach, it was not necessary to examine the other articles for such poisons, and more especially as the quantities of these matters received for analysis were too small to hold out any prospect of a successful result. All this I certify on soul and conscience.                    FREDERICK PENNY.

GLASGOW, 9th May 1865.

Solicitor-General—Is that a true report ? It is. You also at the same time received from Dr Maclagan portions of the body of Mrs Taylor ? Yes. And you made a similar analysis of these ? Yes.

Dr Penny then read the following report :—

*Report of Analysis in the Case of the Death of Mrs Taylor.*

ANDERSONIAN UNIVERSITY, GLASGOW, 9th May 1865.

On the same day and occasion that I received the articles in the case of the death of Mrs Pritchard, Dr Douglas Maclagan delivered to me the following articles, certified to have

been taken from the body of Mrs Taylor:—1. Portion of liver in stoneware jar; 2. Portion of stomach in glass bottle; 3. Portion of heart in glass bottle; 4. One kidney in glass bottle; 5. Portion of rectum in glass bottle; 6. Portion of blood in glass bottle; 7.100 grains of dried contents of intestines.

The vessels containing these articles were securely closed and duly labelled, and were, on the day referred to, brought by me direct to Glasgow.

I have subjected all the articles above enumerated to a course of analysis and chemical examination similar to that applied to the articles in the case of Mrs Pritchard. The following were the results obtained:—

*Liver.*— In the liver the presence of antimony was unequivocally detected, and a quantitative estimation gave ·017 of a grain in 1000 grains of this organ. A careful analysis was also made for the presence of mercury, but not the slightest trace was detected.

*Stomach.*—The stomach yielded about the same proportion of antimony as that found in the liver. No mercury was detected. The stomach was also minutely examined by Stas's process for aconite and morphia, but not a trace of these poisonous alkaloids was obtained.

*Heart.*—The heart was found to contain antimony in less proportion than the liver. It yielded no mercury.

*Kidney.*—The kidney yielded about the same quantity of antimony as the heart. It gave a marked quantity of mercury.

*Rectum.*—The rectum gave antimony, but no mercury.

*Blood.*—In the blood, antimony was detected in rather larger proportion than in the heart. No mercury was detected.

*Dried Contents of Intestines.*—In the dried contents of the intestines, antimony was found to the extent of ·583 parts in 1000 parts by weight. It was partly present in a form soluble in water. No mercury was detected. The said contents were also carefully analyzed for aconite and morphia, but no evidence of the presence of these poisons was obtained.

From a careful consideration of the results of the analysis and examination of the above-named articles, I am clearly of opinion that they are conclusive in showing:— 1st, That all the articles subjected to analysis contained antimony. 2d, That the dried contents of the intestines contained the largest proportion of antimony; next, the liver and stomach; then, the blood, and in less quantity in the heart, kidney, and rectum. 3d, That part of the antimony in the contents of the intestines is in a form soluble in water. 4th, That the kidney was the only article in which mercury was detected. 5th, That neither the stomach nor the contents of the intestines contained aconite or morphia in quantity sufficient to be detected by known chemical process. 6th, That the articles subjected to analysis contained no other metallic poison than antimony and mercury as reported above.

To the truth of this report I hereby certify on soul and conscience.

GLASGOW, *9th May* 1865.　　　　　　　　　　　　　　　　FREDERICK PENNY.

Solicitor-General—Now, that is a true report? It is. You also made a report on certain articles which were delivered to you by Mr M'Call, Superintendent of Police? Yes.

Dr Penny then read the following report :—

*Report of Analysis of certain Articles referred to in the Case of Dr Pritchard.*

ANDERSONIAN UNIVERSITY, GLASGOW, 17*th May* 1865.

On Thursday, the 13th of April last, Alexander M'Call, Superintendent of Police, delivered to me, at my laboratory, the following productions, having sealed labels attached, referring to the case of Dr Pritchard:—A glass bottle, labelled "Battley's Sedative Solution," [B.] A bundle of seven small paper packages, [C.] A quart wine bottle, containing ginger wine, [D.] A small glass vial, containing a white powder, [E.] Three small vials, two corks, and one stopper securely tied together, [F.] Six small vials and six corks, attached with string, [I.] On the same day and occasion, John Murray delivered to me a paper package, having labels attached, marked A, and containing tapioca. On Friday the 21st April last, Alexander M'Call delivered to me a small glass phial, with label attached, marked G, and also a piece of cheese, marked H. On Thursday the 11th inst., John Murray delivered to me a paper package, with label attached, marked K, and containing tapioca. In accordance with instructions received from John Gemmel, Esq., Procurator-Fiscal, I have made a careful analysis and chemical examination of the contents of the several productions above enumerated. My experiments and investigations gave the following results, which, for the facility of reference, are reported in alphabetical order :—This paper package [A] contained 2850 grains of tapioca. The presence of antimony, in the form of tartarised antimony, was unequivocally detected. Its amount was found to be equal to 1·62 grains in the pound of tapioca. Not a trace of mercury was detected. This bottle [B] contained one ounce and five drachms of a dark brown liquid, having the odour and general appearance of Battley's solution of opium. It was found to contain an appreciable quantity of antimony in a

soluble form. The amount was equal to 1·5 grain per fluid ounce of the liquid. It contained no mercury. (I am at present engaged in examining it for other substances.) The seven paper packages [C] comprised in this production were marked No. 1 to No. 7 inclusive. No. 1 contained a small lump of crystallized nitrate of silver, weighing 16·5 grains. It contained no antimony. No. 2 contained 132 grains of cummin seed in powder. Neither antimony nor mercury was found in it. No. 3 contained 143 grains of sugar of lead. Nothing extraneous was detected. No. 4. The contents of this package consisted of a mixture of mercury and chalk, weighing together 6·5 grains, and it was evidently the medicinal preparation called " Hydrargyrum c. Creta." No antimony was found in it. No. 5 contained a lump of opium, weighing 110 grains. No. 6 contained 13·5 grains of morphia, contaminated with a small quantity of nitrate of silver, which, from the appearance of the paper package, had manifestly enfiltered accidentally from without. No. 7 contained 1350 grains of a white gritty, crystalline powder, which was found to have all the physical and chemical properties of sugar of milk. It was carefully tested for mercury, antimony, and other substances, but the results were entirely negative. This bottle [D] contained 18 fluid ounces of ginger wine. No antimony or mercury was detected. This phial [E] contained 3·5 grains of a white powder, which was found by analysis to be tartarised antimony. The three phials [F] included in this production were labelled respectively 1, 2, and 3. No. 1 contained one ounce and three drachms of tincture of conium. No. 2 contained five drops of the same tincture. No. 3 contained two and one-half drachms of the same preparation. This phial [G] contained nine drachms and a-half of a light yellow-coloured liquid, having the taste and odour of cinnamon, and consisting of a mixture of medicinal substances. It contained no antimony and no mercury. This cheese [H] was tested for antimony and mercury, but no evidence of the presence of these metals was obtained. This production [I] included six small phials, which were found to contain as follows :—No. 1. Four drops of tincture of aconite. No. 2. Twelve drops of the same tincture. No. 3. Thirty drops of the tincture of conium. No 4. Fourteen drops of the tincture of conium. No. 5. Empty. No. 6. Nine drops of the tincture of digitalis. This paper package [K] contained 1695 grains of tapioca. Not the least trace of either antimony or mercury was detected in this tapioca.

All this I certify on soul and conscience.                          FREDERICK PENNY.
    17th May 1865.

Solicitor-General—On the 15th May you received some other articles from Mr M'Call ? I did. And you prepared a report regarding them ? I did. The following was the report :—

*Report of Analysis of certain Articles referring to the Case of Dr Pritchard.*

ANDERSONIAN UNIVERSITY, GLASGOW, *May* 19, 1865.
    This is to certify that I have subjected to careful analysis and chemical examination the following articles, which were delivered to me on the 15th inst., by Alexander M'Call :—No. 1. A brownish coloured and turbid liquid, measuring three fluid ounces, contained in a glass bottle, labelled chloroform. It was tested for antimony and mercury, but not a trace of either metal was detected. It contained no aconite. No. 2. A white crystalline powder, contained in a small cylindrical wooden box, with screw cover. It weighed 15·5 grains, and was found to consist of a mixture of tartarised antimony and arsenious acid (that is, the common poison of arsenic), in nearly equal proportions by weight. No. 3. About ten drops of colourless liquid, contained in a quart wine bottle. It was found to be an aqueous solution of corrosive sublimate. No. 4. (A.) A white powder, contained in a circular red pasteboard box. It weighed 5 grains, and was found to be calomel. No. 4. (B.) A white powder, weighing 35 grains, contained in a green pasteboard box. It was found to be tartarised antimony. All the productions containing the articles subjected to analysis were securely closed, and had sealed labels attached.          FREDERICK PENNY.

Solicitor-General—That is a true report ? It is. In your report about the bottle containing a dark brown liquid having the odour and general appearance of Battley's solution of opium, you found an appreciable quantity of antimony in a soluble form ? I did. And you say in that report that you were at that time engaged in examining it for other substances ? Yes. Did you, in fact, complete your examination for other substances to the best of your judgment and ability ? I did. What did you look for in particular ? I looked for mercury and other metals. I searched for aconite, and also for conium. Did you find any of these ? I found aconite. How do you proceed in order to search for aconite in another substance ; is it by chemical or other processes ? Chiefly by the taste of the extract obtained by evaporation, and by its physiological action upon small animals. Just explain to us as distinctly as you can how you proceeded with this fluid, in order to determine whether aconite was present in it or not ? A portion of it was evaporated to dryness, and the extract thus

obtained was very carefully tasted, or its effects upon the tongue and upon the lips ascertained by applying it to them. And what were the effects? Tingling and a benumbing sensation. Characteristic of aconite? Yes. Another process was also carried out with the extract which remained after the evaporation. To another portion of the extract dissolved in water ammonia was added, and a precipitate was separated and examined in the same way, after being dissolved in diluted hydrochloric acid. The benumbing and tingling sensation produced by that precipitate was very slight. But the ammoniacal liquid, after the separation of the precipitate, was treated with hydrochloric acid and evaporated, and the sensation produced by this residue was very strong and distinct. With a view to ascertain the character of aconite when mixed with Battley, I purposely mixed known quantities of tincture of aconite with Battley's solution, treating the mixtures in the same way. I took mixtures from 5 per cent. to 40 per cent. What tincture did you mix? Fleming's tincture. That is a strong tincture? Yes; a strong tincture. The results were precisely similar, but when the proportion was equal to 10 per cent., the sensation was by no means so strong. The addition of Fleming's tincture of aconite to genuine Battley to the extent of 10 per cent. of the mixture gave a sensation very much stronger than the liquid in this bottle. But the sensations were the same, although that one produced by Battley, with 10 per cent. of Fleming's tincture in it, was the stronger of the two? Precisely so. I draw the conclusion that in this solution there was more than 5 per cent., but less than 10 per cent. The sensation of benumbing and tingling is peculiarly characteristic of aconite? Yes. And well known to be so? Yes. You are acquainted with Battley's solution? I am. I believe you procured some pure specimens of it, and treated it without mixture of any kind in the same way as the contents of that bottle? I purchased Battley's solution at several establishments in Glasgow, and also in London. I examined all these samples, and I found that in no case were such sensations produced by the extract obtained as described. Did you buy some of it—I mean of the genuine Battley—of Murdoch Brothers of Union Street, Glasgow? Sauchiehall Street. Did you find any trace of the presence of antimony in the genuine Battley? None. Your examination, I suppose, satisfied you that it contained neither antimony nor aconite? It did. [Bottle produced.] And your examination of the contents of that bottle satisfied you that it contained both antimony and aconite? Yes. You made some further experiments with the contents of the bottle upon rabbits, I believe? I made in all about twenty-five experiments upon rabbits.

Lord Justice-Clerk—With the extract obtained from the bottle? And from genuine Battley and various mixtures.

Solicitor-General—Just take genuine Battley first. Did it kill any rabbits or not? Genuine Battley did not kill the rabbits with a dose even equal to fifty grains. The contents of the bottle—what effect did they produce? According to the dose. What dose killed? 40 grain drops. How did you administer it? By injection under the skin of the back, between the skin and muscles. You experimented with the contents of that bottle on the rabbits in the precise way that you did with the genuine Battley? Precisely. Did genuine Battley, when injected to any extent into the rabbit, deprive it of life? I have already said that genuine Battley did not kill in any case.

Lord Justice-Clerk—To what extent did you try it? To the extent of fifty grains.

Solicitor-General—You did not try it any higher than that? No. Did you experiment in the same way with genuine Battley to which you had added Fleming's tincture of aconite? I did. Tell us the result of that. I made in all about ten experiments with the genuine Battley mixed with Fleming's tincture. In different proportions? Yes. And what extent of Fleming's tincture produced the same effect as the contents of the bottle? I will tell you the result of two sets of experiments. In one set I injected a mixture of Battley into three young rabbits, and in a third into full-grown rabbits. In the first set of experiments with young rabbits, I injected ten grains of genuine

Battley's solution; in the second experiment with a young rabbit, I injected ten grains of this Battley; and in the third experiment, I injected a mixture composed of nine grains of genuine Battley, and one grain of Fleming's tincture of aconite. With the old rabbits, I proceeded in a similar manner, using forty grains instead of ten grains as with the young rabbits,—first, with genuine Battley; and, second, with the mixture; and, third, with genuine Battley mixed with aconite. The general result was, that genuine Battley did not kill? It did not kill. Well, and the contents of the bottle? The following were the results:—The symptoms manifested by the rabbits, both old and young, subjected to the action of genuine Battley, were simple in character and few in number, and were not materially altered by variation of dose. The animal soon assumed a prone position, resting on belly and chest, and the head invariably resting on the ground. The fore-legs were either sprawling or gathered under the body, the hind-legs always extended side-ways; the eyes remained open, and the pupils were natural and not contracted; the breathing was invariably gentle; no cries were uttered, and no convulsions or spasms of the body were apparent. There was a complete condition of inanity, and, with the exception of the open state of the eyes, the animal seemed to be in a state of profound sleep. There was no indication of spasmodic movement; but when aroused or urged to motion, the movements were performed in a crawling tortoise-like manner. In this state the animal remained for several hours, and then gradually recovered. The effects produced by the mixture of genuine Battley with aconite were as follow, and presented a striking contrast to the symptoms resulting from pure Battley:—Very soon after the injection, the animal became restless and uneasy, and then began to crouch, resting on its flank, with the hind-legs extended laterally, and keeping its head erect. It next assumed a sitting posture in an attitude of watchful expectancy, and commenced to twitch its lips and move its jaws as if chewing. Suddenly, it staggers, rolls over, and quickly regains its feet. Saliva begins to flow from the mouth, and soon after piteous and peculiar choking cries are emitted. The head is retracted, and the breathing is painfully laborious. Convulsions now set in, followed by intervals during which the limbs are quite relaxed, and the animal lies helpless on its side. Frantic leaps are now frequently taken, accompanied by movements of a paralytic character. A state of utter prostration also occurs, variable in duration; and then a strong convulsion comes on, during which, or immediately after, the animal expires, the limbs becoming instantly relaxed. Then as to the results produced by this Battley, the symptoms exhibited by the rabbits subjected to this liquid corresponded in every important respect with the effects produced by the above mixture, and were so clearly similar that it was impossible to detect any essential difference in them. In the case of the small rabbits, the experiments were made at the same time, but, without knowing beforehand, it would not have been possible to distinguish the animal under the influence of this Battley from the one under the influence of the mixture of Battley and aconite. These results leave no doubt on my mind— joined with the taste and sensation—that that Battley contained aconite. All the other experiments, which were numerous and varied, confirmed these results.

The Solicitor-General—That bottle was about half-full, I think, when you got it? It contained 1 oz. 5 drachms. What, according to your judgment, as the result of your examination, was the whole quantity of aconite in that amount of liquid? Between five and ten per cent.

Cross-examined by Mr Clark—You said the bottle was half-full when you got it? It contained 1 oz. 5 drachms. Let me see how high that would come upon the label? I could not tell that. Would it go up half-way to the label? No; it would not come to it.

Lord Justice-Clerk—Aconite is a vegetable poison, I understand? Yes. What is the popular name of it? Monkshood. Were the experiments with the rabbits you spoke of made with the Battley's solution purchased by yourself from Murdoch Brothers? They were made with the Battley purchased by myself from the Apothecaries' Hall, or from other places. That is, not from

Murdoch Brothers? No. Battley's sedative solution is a preparation of opium, I believe? It is. If you pursue Reinsch's test for the detection of antimony, and obtain, in the course of that process, a deposit upon copper foil, is that deposit conclusive of the presence of antimony? It would not be to my mind. Whatever the deposit might be? Whatever the deposit might be. Why is that so? Because other matters are liable to give a deposit similar in appearance to the eye. The only test that you have for antimony, when you pursue Reinsch's process to the extent of getting a deposit on the wire, is the colour that is seen on the foil? Yes; the violet colour on the copper. But that is not conclusive of antimony, because the same colour may be produced by other substances? It may. What are these substances—give us an example of them? Oily matters. Animal oils? Or vegetable oils; animal oils particularly. Anything else? No; nothing occurs to me at this moment.

Solicitor-General—[Shown No. 142.] That is a small phial containing a dark-coloured liquid? It is. When was that delivered to you? On the 16th May 1865. By whom? By John M'Millan, assistant to Murdoch Brothers. And did you make an analysis of that? I did. And was it similar to the Battley's which you purchased yourself? In every respect.

Lord Justice-Clerk—You analyzed it, did you? I did.

Solicitor-General—That contained no antimony and no aconite? None. [Shown No. 143.] That is a bottle containing Battley's solution which you yourself purchased at Murdoch Brothers? Yes; it was purchased by myself. And have you analyzed that? I did. And it was in no respect different from that which was in No. 142, brought to you by Mr Macmillan? It corresponded in every respect.

Lord Justice-Clerk—And with the genuine Battley which you purchased elsewhere? Yes. And with which you made experiments on the rabbits? Yes.

Solicitor-General—From whom did you get that bottle, No. 85, of which we have heard so much? On Thursday, 13th April, Alexander M'Call, Superintendent of Police, delivered it to me at my laboratory. Can you tell me in a general way how much liquid there was in it at the time? It was much below the lower edge of the label. Of course you mean the original label? Yes. Can you tell me how much the bottle will contain when full? The entire capacity of the bottle was 5½ oz. The top red line on the left side marked at the time by myself, as shown me by one woman, indicates 2¾ oz.; and the lower red line on the right side marked at the time by myself, as shown by the other woman, is 2½ oz. Do you know who these women were? No; I have seen them in Court. When I received it, it contained between 1½ and 1¾ oz. Did you send some of the contents of that bottle to Dr Maclagan? I did. Or give them? I did not give them; I sent them.

Lord Justice-Clerk—Did you put it into his own hands, or send them? I give up possession of the bottle to the officer Murray.

Solicitor-General—That was after you were done with it? After I had completed all my experiments—last week. I have marked on this label the date when I gave it up, which was on the 20th of June.

Lord Justice-Clerk—Was there anything in the bottle then? Yes. How much? There must have been about a drachm.

Solicitor-General—Was it sealed up? Yes; and bore my seal. Did Dr Maclagan get it with the seal unbroken? Yes. There was nothing in the contents which you sent to him that was not in it when it was handed to him originally? No; it was precisely in the same condition. Did you happen to be present when Dr Maclagan broke the seal? No; I was present at the experiments made in the University here upon rabbits by Dr Maclagan, in presence of Dr Christison, Dr Littlejohn, and Dr Gamgee. These experiments were precisely similar to mine, and were made with the same result, except that death was more speedy from the larger dose given. These experiments were made with the mixture in the bottle, the genuine Battley, and the Battley to which the tincture of aconite had been added? Yes. And these experiments which you witnessed, being exactly the same as your own, confirmed the opinion

which you expressed, that aconite was present in the bottle? Entirely so. With the exception of the antimony and the aconite which you detected, the contents of the bottle were, I presume, similar to the genuine Battley? I examined it for the leading constituents of opium, and I found them there. In fact, it would be correct to say that it differed from genuine Battley, so far as you could see, only in the presence of the antimony and the aconite? It did.

The medical witnesses were here asked to leave the court, as the examination of Dr Penny was now to be directed to matter of opinion.

Dr Penny was then asked to read the latter part of his report upon the cause of Mrs Pritchard's death, which embodied the conclusions at which he had arrived. Having done so, his examination was resumed by the Solicitor-General. You heard read by Dr Maclagan the report of the post-mortem examination of this lady's body? I did. And I believe you have had previously an opportunity of studying it? It had been put into my hands by the agent for the defence. But you had read it before? Yes. The result of that report is, that the post-mortem appearances exhibited nothing to account for death? That is the result of the report. You heard the evidence as to the symptoms exhibited by Mrs Pritchard from the time that she was taken ill after the New Year down to the time of her death? I did. Are these symptoms suggestive to you of the action of any poison with which you are acquainted? Witness—From study only? I mean from study. I understand you are a chemist, not a medical man? Witness—Purely a chemist. And you have studied the action of poison? I have. Do these symptoms indicate the action of any poison to you? They correspond with those of tartar emetic. That is the other name for tartarised antimony? Yes. Tartarised antimony is one of the forms, and the common form of antimony soluble in water? The best known form. You detected the presence of mercury by your chemical examination? I did. Did you hear anything in the evidence which accounted for that? Yes, I did. What was that? Those powders prescribed by Dr Paterson. Containing calomel? Yes, and *hydrargium cum creta*. Assuming that such powders had been administered shortly before death, that would correspond with the traces of mercury which you found afterwards? It would—to the extent given. Supposing that antimony had been applied to Mrs Pritchard's neck in the month of October—externally, I mean—when she was complaining of the swelling of a gland in her neck, would that account for any of the results of your chemical analysis? I have no experience in that direction; I am not qualified to answer that question. You confined yourself to the fact that these substances were detected by chemical analysis, and to the opinion, as the result of your study on the subject of the symptoms attending the administration of such a poison? Yes. And I understand you to say that the symptoms throughout Mrs Pritchard's illness—I speak only of the time after Christmas down to her death —corresponded with the symptoms produced by the administration of antimony? So far as the scope of my experience goes from study, they did. You also heard the account of Mrs Taylor's illness. Do the symptoms spoken of by the witnesses who gave that account suggest the operation and action of any poison to your mind? Merely the vomiting—from antimony. There were other symptoms in the case of the old lady—the comatose state in which she was? I am not prepared to speak to that. In regard to Mrs Taylor's case, what were your conclusions as stated in your report? From a careful consideration of the results of the analysis of examination of the above-named articles, I am clearly of opinion that they are conclusive in showing,—1st, That all the articles subjected to analysis contained antimony. 2d, That the dried contents of the intestines contained the largest proportion of antimony; next the liver and stomach; then the blood; and in less quantity in the heart, kidney, and rectum. 3d, That part of the antimony in the contents of the intestines is in a form soluble in water. 4th, That the kidney was the only article in which mercury was detected. 5th, That neither the stomach nor the contents of the intestines contained aconite or morphia in quantity sufficient to be detected by known chemical processes. 6th, That the articles subjected to analysis con-

tained no other metallic poison than antimony and mercury, as reported above.
Is that the opinion truly entertained by you as the result of your examination?
It is. You say the same in the concluding portion of your report regarding
Mrs Pritchard? These are my conclusions. Is there any other matter on
which, as a chemist, you can give any other information from the evidence you
have heard? You have stated all you are able to state in respect to either Mrs
Pritchard or Mrs Taylor? To the best of my belief I have stated all I know.
Nothing occurs to my mind beyond what I have stated.

Cross-examined by Mr Clark—Has aconite a bitter taste? No. Has antimony
a burning taste? It has, after a time, a metallic taste. When it enters the mouth
has it a burning taste? Not so far as I have tasted it. What it may be when it
goes down the throat in sufficient quantity to poison I don't know. (Laughter.)

*Dr Douglas Maclagan recalled.*—Examined by the Solicitor-General—Dr
Maclagan read the portion of his chemical report embracing his conclusions as
to the death of Mrs Pritchard. That is a true report? Yes. You are now
better acquainted with the history of the case? Yes. You heard the account
given in this place of the illness of Mrs Pritchard, the first time she was taken
ill after returning from her father's at Christmas? Yes. I am referring to the
part of the evidence which relates to the account of the illness after her return
to Glasgow up to the time of her death. Does that account of the history of
her illness suggest to you as a medical man the cause of her death? It suggests
a confirmation of the opinion I had formed from my chemical and *post-mortem*
examination. Do the symptoms which she exhibited indicate the administration
of antimony at an early period of the illness? I think so. If I remember right,
there was vomiting at a very early period of the illness—that was a charac-
teristic symptom—and muscular depression. Sickness and vomiting, and
muscular depression, are symptoms of the action of antimony. Were there
any other symptoms exhibiting the action of antimony which struck you?
There was irritation of the bowels and cramp of the extremities, which are
symptoms of poisoning by antimony. These are all characteristic of that
poison? Yes. Then is it according to your opinion, judging from these
symptoms, that the administration of antimony commenced with the commence-
ment of the illness after Christmas, and continued down to the time of her
death? I think it is most probable.

The Lord Justice-Clerk—You mean the administration of antimony was
going on? Yes; from time to time. Then you think the symptoms she
exhibited were such in all respects as you would have expected on the sup-
position that antimony was administered to her all along? I think so. Does
the history of the case, as you have heard it in the evidence, and particularly
of the symptoms which were manifested, suggest to your mind, as a medical
man, any other cause of death? I don't know any natural disease that I could
very well say I think it was due to. There is no natural disease to which you
can ascribe the death? No, my lord, there is not.

Solicitor-General—The *post-mortem* examination did not indicate any natural
disease whatever? No; no natural disease that could account for her death.
There was an arrested pulmonary disease that had existed years before; but
that had nothing to do with her death obviously. The result of the chemical
examination accounted for her death in a manner entirely in accordance with the
symptoms? I think so. Suppose that antimony had been externally applied
to her neck in October last, when she was plagued with the swelling of a gland
in the neck, would that in any way account for her illness? If it was rubbed
in to the extent of producing pustules on the skin—assume that? Oh, no!
I never saw antimony rubbed into the skin produce any of the constitutional
effects of antimony. And that would not account for the result of your
chemical examination, finding it in the stomach and other organs? Oh, no!

Lord Justice-Clerk—It would not account for the results of the chemical
examination, nor, I suppose, for the symptoms exhibited between Christmas
and the death? No!

Solicitor-General—Suppose that years ago—I cannot give you any time more nearly, but just take the statement as I have given it to you now—Mrs Pritchard applied antimony internally on one occasion, when she had a tendency to inflammation of the eyelids—suppose that this was years ago, and that she had not used it internally except on that one occasion—would that be in any way connected with the symptoms of her illness, or with her illness at all? No. It would have nothing to do with it? Oh, no. You heard Dr Paterson mention the powders which he had prescribed for her? Yes. That was in the beginning of March? Yes. And the powder contained mercury? Calomel and grey powder. Suppose these powders to have been administered, would they account for the mercury which was found by your chemical analysis? Certainly. Had that mercury, in your opinion, anything to do with causing death? I do not think that there was any evidence of its having caused death. Or having had any concern with the death? Not that I can think of. But the traces of mercury which you found were such as you would expect in a patient who died while such powders were in the course of being administered? Yes. Were these proper powders to administer? Well it is quite a usual prescription—calomel and grey powder. I presume you mean were they a safe prescription, generally speaking. I am not asking your opinion on the particular case at which you were not present; but they are a safe and common prescription? Yes. Then nothing in the history of the case as you have heard it in the evidence occurs to throw any doubt upon the conclusion at which you arrived by your chemical analysis? I cannot say that anything has occurred. But everything therein tends to confirm it? Rather so. Not entirely so? Oh, yes; I should say decidedly so;—that is the proper answer. The symptoms during the whole of her illness, and the result of the chemical analysis, are in harmony with each other, and both concur in pointing to antimony as the cause of death? I think so. Was there anything in the case to indicate to a medical man that she was labouring under gastric fever? No. Do you mean that a medical man of ordinary intelligence attending her during the illness which you have heard described would not have concluded that she was labouring under fever? I should think not. Is there anything in the account of her illness to suggest gastric fever or any other fever to your mind at all? No. Now, will you read the concluding part of your report respecting the case of Mrs Taylor? Dr Maclagan then read the conclusions stated in his report, as given above.

"From the above experiments I am led to the following conclusions:—1. That Mrs Taylor had taken a considerable quantity of antimony in the form of tartar emetic. 2. That, having regard to the absence of any morbid appearances sufficient to account for death, and to the presence in the body of a considerable quantity of a substance known to be capable of destroying life, her death must be ascribed to the action of antimony. 3. That it is most likely that this was not taken in a single large dose. Had this been the case, I should have expected to have found some morbid appearances indicative of the irritant nature of the drug. It appears to me more probable, from the amount found in the body, that it must have been taken in a succession of doses, not great enough individually to produce local irritant effects, but amounting in the aggregate to a large quantity. It is right, however, to add that a single copious dose, not large enough to produce marked local effects, might give rise to fatal depression of the system in a woman aged seventy-one, whose heart was enlarged and somewhat dilated. 4. That, from the fact that antimony was found copiously in the liver, was readily detected in the blood, and existed to the amount of a quarter of a grain in the stomach, some at least of the tartar emetic had been taken, probably within a few hours before death. 5. That, from mere chemical investigations, I am unable to say over what length of time the administration of the antimony had extended, supposing it, as I believe, to have been taken in a succession of doses. This can be learned only from a consideration of the history of the case, with which I am unacquainted.—Douglas Maclagan."

That is your conscientious opinion? Yes. You heard and attended to the evidence respecting Mrs Taylor also? Yes. What cause of death does that indicate to your mind? I am inclined to think, from the account we have heard of the case, that there was something more than antimony at the last. Antimony there must have been, for you found it? Yes. You were inclined to think that there must have been something more than antimony. By " inclined to think" do you mean to say that you have doubt or hesitation, or do you actually think it? I do think it. What else? I think some powerful depressing poison besides antimony. Such as? The symptoms might be produced by aconite. Aconite is narcotic? It is a sedative rather; but it is commonly described as a narcotic in books. It does not always affect the brain by any means, which is the proper meaning of the word narcotic.

Lord Justice-Clerk—What are its effects? Its effects are to lower the circulation especially, and produce a paralyzed condition of the muscles. The fatal result, I think, is generally due to its effect upon the heart as a muscular organ.

Solicitor-General—Just mention the symptoms in Mrs Taylor's case which you think were such as aconite would have produced. I think her being found with her head fallen on her neck, and hardly observed to breathe, and her pulse almost if not absolutely imperceptible, and in the dozing torpid state in which she was——

Lord Justice-Clerk—Coma, I suppose? I am not sure if it was coma; I rather think it was the torpid condition of the brain from the lowered circulation. All these you say are what? Are such as would have resulted from aconite.

Solicitor-General—Are they such as you would expect to result from aconite? Yes. In short, they are the symptoms produced by the action of aconite? Yes; but aconite, like most poisons, varies a little in the symptoms it produces in different individuals. You heard the result of the analysis of the liquid in the Battley bottle by Dr Penny, and you yourself experimented with it upon rabbits? Yes; we made one series of experiments. Do you corroborate what Dr Penny said about the result of his experiments? Yes. Would the aconite and the antimony existing in the liquid account for the symptoms under which Mrs Taylor appeared to be labouring on the evening of the 24th and the morning of the 25th February? That must depend upon the quantity which she took of the liquid. But taking what quantity, would you say? What would represent over five or ten grains of the tincture of aconite contained in it would do it. I say over five, because that has been indicated as a safe quantity of Fleming's tincture to be given, though I do not think it safe. Assuming aconite to have been taken, you would expect to have found it upon the chemical analysis which you made? I might not; these organic poisons are very often not found, though they are known to have been taken. You mean in cases where they are certainly known to have been taken? Yes. You are referring to aconite? Yes; but the major includes the minor. I refer to the whole class of alkaloids. Antimony passes pretty rapidly out of the system? Yes, a good part of it passes pretty rapidly out of the system. In vomiting and purging? In vomiting and purging, and by the urine. And in that way the patient is weakened, and ultimately destroyed? Yes. Would the administration of opium in any way interfere with the symptoms exhibited by the person who had taken antimony? So far that I think it is possible it might make the tendency to vomit less. And also interfere with its effects upon the bowels, I suppose? Yes. But would it, even in conjunction with opium, exercise a pernicious influence on the patient? Yes; the depressing effect upon the muscular tissue would remain. Have you ever known a patient under the influence of opium, aconite, and antimony at the same time? No. But if these poisons—opium, aconite, and antimony—were administered so as to be operating at the same time, are the symptoms which Mrs Taylor exhibited such as your science would lead you to anticipate? I think so, because the aconite, being the more powerful, would probably predominate. I infer from your *post-mortem* report—but I wish to know whether I am

certainly right—that the *post-mortem* appearances were not such as to indicate apoplexy? No. When a patient dies of apoplexy, will a *post-mortem* examination indicate that disease? In most cases, but not invariably. And all you can say, therefore is, that they were not indicated by the appearances you saw. That is all. But that is not conclusive? Not absolutely conclusive. By most cases you mean the large majority of cases? Yes. Are the exceptions very rare? I have not met in my own practice, where I had an opportunity of making a *post-mortem* examination, any case where I did not find indications of apoplexy. But there are accounts of such cases? There are. Was there anything in the symptoms which she manifested during life shortly before her death or at any time before her death, which indicated apoplexy? Certainly not. Then taking these appearances before death and the *post-mortem* examination together, is the idea of apoplexy satisfactorily excluded in your judgment? Yes.

Lord Justice-Clerk—That is to say, you are satisfied she did not die of apoplexy? Yes. Was there anything in her symptoms which, in your judgment, would have led any medical man to think of apoplexy? Not if he heard the account given by Dr Paterson. I am not asking your opinion upon the accuracy of the evidence; but, assuming it to be correct, would any man, seeing what Dr Paterson described, think of apoplexy as the cause? If I had seen what Dr Paterson saw, I would not have concluded that it was apoplexy, and I do not think any other man would. That is what I mean. The purport of the question is, whether it is a thing about which there could have been any reasonable difference of opinion? Doctors do differ; and I wanted to know whether this was a matter about which there could be a difference of opinion among intelligent men? I don't think it. Did you taste the aconite in the bottle? I did. Did it produce the sensation which aconite produces? It did. You are acquainted with the tingling and benumbing sensation? Yes. You are not likely to mistake it? ' I think not. Did that, irrespective of the experiments on the rabbits, satisfy you that there was aconite there? I should certainly have inferred that without any experiment upon the rabbits. Without any doubt? Yes. And the experiments upon the rabbits only went to confirm that? Yes. Did you get that bottle brought to you by the officer with the seal unbroken? Yes, by J. Murray. It is broken now? I endeavoured to keep the seal as entire as possible. Has aconite any effect in paralyzing a patient? It does produce paralysis of the muscles, and sometimes convulsions. You are acquainted with Battley's solution? Yes. It is a very well-known medicine? Very well-known. I presume it is taken for all the purposes for which opium is used? It is a form of opium. What is a common dose of it? Well, I believe it is stated by those who prepare it as being a third stronger than laudanum, but in practice I have not found it to be so. What would be a good dose for an old lady of seventy? I would not give so much, perhaps, to an old lady as to a strong man. The medium dose of laudanum is commonly stated to be twenty-five drops.

The Lord Justice-Clerk—That depends upon habit entirely, and upon the circumstances of the case? Yes. You would give the patient a little less of Battley than of laudanum—about twenty drops or so.

Solicitor-General—Generally speaking, it has the same effects as laudanum? Yes.

Cross-examined by Mr A. R. Clark—You saw no indications of poisoning by opium in Mrs Taylor's case? No precise indications. It did not appear as if she had taken any opium? I cannot say that she had not taken any, but I did not observe any symptoms which specially pointed to opium. Were the symptoms which you saw exclusively the symptoms of aconite, as you thought? Well, it is very difficult to say; but I think aconite was the leading feature in the final part of the case. Did the symptoms in the course of the illness, as described by Dr Paterson, not indicate poison by opium or laudanum at all? I think not.

Lord Justice-Clerk—You mean that you now think that they don't indicate poisoning by opium? Yes.

Mr Clark—Are they inconsistent with poisoning by opium? I do not know that they are inconsistent with her having had opium; but they are not consistent certainly with poisoning by opium, and with the ordinary symptoms. Then you could not say that she had not taken opium? Oh, certainly not. Only that the symptoms of aconite predominated? That is what I think. If she had taken opium alone, what would you have expected to find different from what you heard? I would have expected to have found the pulse slow and full, and probably the breathing laborious and stertorous. But though these were absent, you cannot say that opium was not taken? No; particularly if the person was accustomed to the use of opium. I think Dr Paterson said her breathing was laborious? I think not; my impression was that he stated her breathing was barely perceptible.

The Lord Justice-Clerk here read from the notes that he had taken of Dr Paterson's evidence, from which it appeared that that gentleman had used the expression that her breathing was laborious.

Mr Clark—It seems, then, that her breathing was laborious. What did that indicate? It indicated some narcotic poison. Keeping that symptom in view, what modification does that make upon your opinion? Not much, because of the condition of the pulse, which shows the action of aconite upon the heart. You say it does not make much modification; does it make any? I do not think it does. You indicated first that you understood it was easy, light breathing? I indicated that the breathing was very feeble, but there I was wrong. Therefore not laborious? Not in the common case. Is laborious breathing an indication of opium? It is an indication of many things besides opium. Did Dr Paterson not also say that the breathing became stertorous? I do not think so.

The Lord Justice-Clerk here read from his notes again, from which it appeared that Dr Paterson had used the word coma.

Mr Clark—You observed that Dr Paterson makes use of the word coma? Yes. Does that indicate opium? Yes. Not aconite? Not generally; but here it was more oppression than true coma. Then you think that Dr Paterson was not right when he described it as coma? Coma is used by many persons to describe insensibility. Did Dr Paterson use it scientifically? Probably. But you pointed to the absence of coma as indicative of poisoning by aconite? I spoke of her being in a torpid condition, which I think was connected with the weakened state of the circulation, not from fulness of the brain. Now, in regard to the detection of organic poisons, you say that aconite is one? Yes. And is not easily detected by chemical analysis? It can be detected. By chemical analysis? Not by chemical tests. Opium is another vegetable poison? Yes. When it is given, it is absorbed into the system? Yes. And so a person may be poisoned by opium without any traces of it remaining in the system? In the stomach. But in the system? I have not been able to find any in the system, though I have made experiments for that purpose. A person may be poisoned by opium without any trace remaining in the stomach or system capable of being detected by chemical analysis? Certainly. Antimony of course is a mineral poison? Yes. And it is more easily detected? Yes. And known to be so? All mineral poisons are known to be so. A person cannot be poisoned with antimony without the antimony being capable of being detected in the system? I am not quite sure that I would be prepared to say that. I can only reason analogically, and I am not prepared to give in to that statement broadly; because I know that a person may be poisoned with arsenic without its being detected. Do you know any case in which there was poisoning by antimony without the antimony being found? I cannot recollect of any such case. There was a very large quantity of antimony found here in both cases? The quantity was considerable. But the expectation is that, if a person is poisoned by antimony, chemical analysis will detect the antimony? Yes. It is possible there may be an exception, but you have not yet known it? It is possible. The pupils of Mrs Taylor's eyes are mentioned as being contracted? Yes. Is that an indication of poison by

opium? Yes; but it occurs in aconite too. Is it a characteristic of poisoning by aconite? It seems to have been observed in a considerable number of cases, but they vary a good deal in that respect. In short, I understand there is a question as to whether aconite does contract the pupils of the eyes? There is a question; but that seems to arise from people having observed the symptoms at different stages, and the probability is that contraction had been produced first, and then relaxation at the time all the muscular parts become relaxed,—viz., at the time of death.

Lord Justice-Clerk—You said that anything over five grains of Fleming's tincture of aconite would satisfactorily account for all the symptoms exhibited by Mrs Taylor? I mean the fatal symptoms at the end of the case. Now, referring to bottle 85, how much of that liquid must she have taken in order to take equal to five grains of Fleming's tincture? If Dr Penny's estimate be correct—and as I only made one experiment myself, I am not entitled to speak from my own knowledge—if his estimate be correct, that it contained from 5 to 10—say 7 per cent., she would require to take 7, which is over 5 a little, and that would give her 100 drops. I used the word "grains," though we don't measure such fluids by grains, because Dr Penny used it in his estimate. It would be more correctly minims, which are measured drops. Must it be all taken at once to produce these effects? Aconite might be given in divided doses, and it might not prove fatal, though the same quantity was taken, because the depressing effect of one dose might have gone off before the second dose was given. Then you are speaking of a single dose? I am speaking of single doses.

Mr Clark—Aconite is applied externally in some cases, such as neuralgia? Yes. There is a liniment in the Pharmacopœia which is of about the same strength as Fleming's tincture.

Lord Justice-Clerk—Are 100 drops of Battley's solution an unusual quantity for a person to take who has been in the habit of using it for a long time? No, my lord; there are many opium-eaters who would not thank you for 100 drops. (A laugh.) I am speaking of a person who has been in the habit of taking it in moderation. Would 100 drops be too much for such a person? Oh! he could take 100 drops quite well, a person who was in the habit of using it. Would 100 drops be a large quantity? 100 drops would rather more than fill an average-sized tea-spoon.

*Dr Henry Duncan Littlejohn.*—Solicitor-General—You are Surgeon of the Edinburgh Police? I am. And, in addition to your general practice, you have had considerable practice there? I have. You acted along with Professor Maclagan in making the post-mortem examination of these two ladies, Mrs Pritchard and Mrs Taylor? I did. And you signed the reports along with him? I did. And you concur in these reports as being true? I do. Now, take the case of Mrs Pritchard first. Does that report indicate in any way that the lady had been ill of gastric fever at the time of her death? It does not. Nothing to suggest that? Nothing to suggest it. You took no part in making the chemical analysis? I did not. But you have seen the report of that analysis? I have. And heard it read? I have. You have also heard the whole evidence in this trial? I have. Now, attending to the evidence respecting Mrs Pritchard's symptoms during her illness, from her return to Glasgow after Christmas, down to the time of her death, what in your opinion was the cause of her death? Antimony administered in small quantities, and continuously. Do you mean from the commencement of the illness down to the time of the death? Yes; down to the time of the death.

Lord Justice-Clerk—That is from about Christmas-time till her death? Yes.

Solicitor-General—Supposing that to have been so, and that the poison was administered occasionally during all that time, the symptoms are exactly such as you should have expected? They are exactly such. Does any other way of accounting for these symptoms during that period occur to your mind as

a medical man? None other. You cannot account for them in any other way? I cannot. And that way entirely accounts for them? Entirely. And the chemical analysis is, of course, such as, upon the same supposition, you should have expected? Quite. You also heard the evidence regarding Mrs Taylor's death—her illness before death, and such an account of her death as we have had here? I did. What opinion did you, as a medical man, form from the symptoms in her case as to the cause of her death? I had greater difficulty. It seemed to me that she might possibly have died from a dose of antimony administered shortly before death, or else from some of the sedative narcotic poisons. Have you any difficulty in her case in arriving at the opinion that she died from poison? None whatever. I so understood you that the difficulty you alluded to is as to the particular poison which killed her? Clearly so. Do you think the symptoms were mixed in her case to some extent like the symptoms of narcotic poison, and to some extent like the symptoms of antimony? Well, I am inclined to believe they were. Was there anything in her case to make you think that she died of apoplexy? There was not. Nothing to suggest that idea? Nothing. Was there any of the distinctive characteristics of apoplexy present at all? Not to my knowledge. And the *post-mortem* examination did not indicate any such disease? The *post-mortem* appearances did not. Do you recognise in the symptoms which Mrs Taylor exhibited prior to her death—do you recognise the action of antimony? In the failure of circulation I certainly do, and great depression and spasms. And in the state of insensibility in which she was? Yes; in the later stages of antimonial poisoning we have generally a state of insensibility. You heard one of the servants describe the sensations she felt on taking a bit of cheese on one occasion—a hot taste in her mouth, I think she said like pepper—and a burning sensation in the throat. What does that indicate? Do you form any opinion as to what might be in the cheese? Not very decidedly. What does it suggest? It suggests a large quantity of antimony; and it also suggests a strong dose of narcotic poison.

Lord Justice-Clerk—It suggests many things? Yes, my lord. Many things besides cheese? Yes.

Solicitor-General—And in one of the servants it produced violent sickness, lasting a considerable time—some hours, I think. That also is consistent with antimony? Yes; quite consistent with antimonial poisoning. Would antimony produce a burning sensation in the throat? It would in large quantities. I suppose you do not say that from actual experiment? I do. I have tried in it pretty large quantity. But in the throat? The secondary effect is always felt in the throat. And it did produce a burning sensation? It did. You also heard the account another servant gave of the effects following from some egg-flip she had swallowed. What do these symptoms convey to your mind? They point to some substance resembling antimony, if not antimony itself. Antimony would account for them Does anything else occur to you at this moment that would do it? Various other emetics. Can tartar emetic be readily beaten up with egg-flip? With great facility. Rather a convenient medium for administering it? Yes. It dissolves readily? It does. Is it possible to convey antimony into the egg-flip in loaf-sugar? Antimony itself can be obtained in lump. But could you put tartar emetic into the sugar in sufficient quantity to produce sickness? It is quite possible by dusting it on. The sugar, being porous, would take up a quantity. It is a white powder? It is; resembling powdered sugar. If it was proposed not to kill by a dose, but to keep up the illness, a sufficient dose could be given in a lump of sugar? Quite easily.

Cross-examined by Mr Clark—Do I understand you to say that if the sugar—the two pieces of sugar—were put into a cup of egg-flip, enough of antimony can be conveyed by dusting over the sugar with tartar emetic—so that a teaspoonful of the egg-flip could produce the effects mentioned? It is quite possible. Egg-flip being a thick mucous substance, it would sustain mechanically a considerable quantity. You observe I am not speaking of the

egg alone, but of the beat-up egg with hot water upon it. Suppose egg-flip is made in the ordinary manner, can you convey into the cup as much antimony upon two pieces of sugar as, taking a spoonful of the liquid, would produce the effect you have said? I think it is quite possible. Have you made any experiments to try it? I have made no direct experiments to try it? Have you made any indirect experiments to try it? I have not. You have made no experiments at all? We doctors are continually making experiments. But I mean experiments for this purpose? I have made no experiments with direct reference to this question. You have made no experiments at all? Not with reference to this case. Then this is mere theory? Grounded on my experience of this drug. Without knowing the quantity of hot water that was poured upon the egg-flip, or the extent to which the egg and water were mixed together, you say that it is possible that as much tartar emetic could be put upon two lumps of sugar as would produce the effects referred to? I think it is quite possible. I may say that I am intimately acquainted with tartar emetic, and I hold I am entitled to answer the question the way I have done. You say that Mrs Taylor indicated poisoning by antimony and some other narcotic poison? I said " or " some other. Is opium included in sedative narcotic poisons? It is not. Do you mean to say that opium is not a sedative narcotic poison? I do not regard it as such. Is it not a narcotic? It is. And sedative? It is not. I regard aconite as a sedative narcotic. There was nothing impossible in the symptoms with her having taken opium? Nothing. You are inclined to the opinion that, besides opium, there was some other poison? Yes.

The Lord Justice-Clerk—I understood you to say that it was not impossible for Mrs Taylor to take opium, and the opium to contribute to produce those symptoms? It was not impossible.

*Dr Paterson* recalled.—Solicitor-General—Dr Paterson, you heard the evidence in this case, and I hope you paid particular attention to that regarding the illness of the two ladies, and the symptoms? I have. Are you able to say, from the evidence, whether the impression you formed regarding Mrs Pritchard was confirmed or contradicted? In my opinion it has been very well confirmed so far as regards poisoning from antimony. Are you well acquainted, from your professional experience, with the action of antimony? I have seen a good deal of it from the thirty years' experience I have had, both in external and internal use. Have you seen it kill? I have. More than once? I have seen two cases of children that were poisoned by having it accidentally administered to them by their parents. These were the only cases of death? Yes; from antimony. Are all the symptoms which you heard described in the case of Mrs Pritchard such as would be produced by antimony? Yes; in what is called chronic poisoning by small and repeated doses. You include all the symptoms from the beginning of the illness, soon after Christmas, down to the time of her death? As far as I can judge from the general description, I certainly consider that there was antimony administered during the greater part of that time. Her appearance when you saw her in February entirely accorded with that? Yes. Her appearance was just such as you would have expected in a person who had been the victim of chronic poisoning by antimony? Yes. And the symptoms down to her death were also such as would be so produced? I certainly think so. That is my decided conviction. As a medical man, from the evidence as to the illness, the post-mortem examination, and the chemical analysis, do you think that she was killed by poisoning by antimony? That is my decided impression. What is your impression as to Mrs Taylor's death? My impression was that her death was caused by opium; but there might have been some other narcotic combined with that unknown to me. Would aconite in combination with opium well account for the symptoms you saw? It would certainly contribute to the effect, and hurry the termination. Would these two in combination well account for the symptoms you saw? It never entered into my brain to

suppose such a combination. Well, but let it enter your brain now. I suggest it to you now. Well, I believe it would. You have not in your practice seen any person poisoned by a combination of aconite and opium? Not to my knowledge. I have seen them poisoned by opium. Do you know experimentally, or only from study, the action of aconite? I tasted aconite just last week to determine its qualities. You mean the effect of it? Yes; of course I was very cautious in regard to the dose; I applied it to my tongue. And what sensation did it produce? In less than a minute there was an increased flow of saliva. This was immediately followed by a strong tingling sensation, very soon accompanied by numbness, and I felt the effects of it for at least four hours afterwards. You made such an acquaintance with the taste of it as you would know it again? Certainly; I will never forget it while I live. Attending now to the history of Mrs Taylor's case, along with what you saw of it yourself, do you think that any of the effects of antimony were exhibited by her? I had not the slightest suspicion of antimony in any shape or form when I saw her. Was what you saw inconsistent with antimony? The narcotic effect was such by the time I saw her that I do not believe I could recognise the effect of antimony. The narcotic effect would overpower the other? Yes. Would such a poison as laudanum interfere with the emetic effect of antimony? I believe it would. And also with its effect upon the bowels? I think it would.

Cross-examined by Mr Clark—Was there stertorous breathing in Mrs Taylor? Literally, there was what is called stertorous breathing, but I would call it oppressed breathing. You call it oppressed, but it is also called stertorous breathing or snoring? Yes. And snoring and stertorous breathing mean the same thing? The same thing.

Solicitor-General—In describing Mrs Taylor's condition when you were examined before, I believe you used the word *coma?* Yes; I did. What did you mean by the word? It signifies insensibility—insensibility especially under opium. And you were under the impression that it was opium alone? My impression was that it was opium alone, or some of its preparations; it might be morphia.

Lord Justice-Clerk—I would like to know before you go what your opinion is now, after hearing the whole evidence as to the cause of Mrs Taylor's death? It strikes me that she died from the effects of the narcotic. You mean the opium? Yes. That is your opinion? That is my opinion. You think, then, that she had taken so much opium as to kill her? I think so. Without the presence of any other poison? That is my own impression.

Solicitor-General—Do you mean that she had no antimony or aconite? I understand that now, but I did not understand that at the time.

Lord Justice-Clerk—I hope you understand me. You have heard all the evidence which discloses the presence of antimony in Mrs Taylor's body, and after having heard all the evidence, I want you to tell me what you now think was the cause of Mrs Taylor's death? I believe her death was occasioned by a combination of those two medicines—the antimony and the opium. A less dose of opium would have a greater effect, seeing that the body was previously under the influence of antimony. A smaller dose of opium would have a fatal effect in consequence of the condition of the body, produced by antimony? I certainly think so. Suppose that the opium which Mrs Taylor took had upwards of five per cent. of Fleming's tincture of aconite combined with it, what do you say then? The effect would be much more rapid, certainly. And more likely to be fatal? Certainly.

The Jury unanimously found the prisoner guilty on both charges, and he was sentenced to be executed at Glasgow on the 28th of July.

# Part First.

## ORIGINAL COMMUNICATIONS.

ARTICLE I.—*On the Mortality of Childbed as affected by the Number of the Labour.* By J. MATTHEWS DUNCAN, M.D., F.R.S.E., Lecturer on Midwifery, etc., etc.

THERE are two important questions regarding the mortality of lying-in women which certainly have not received the amount of attention which they deserve. They are interesting and important, not only in themselves, but also, in a high degree, on account of their bearing upon topics which are constantly discussed without taking into account the great light and influence which the answers to them might bring upon such topics.

The questions I allude to are :—Does the number of a woman's pregnancy regulate in any degree the mortality to be expected from lying in? Does the age of the childbearing woman regulate in any degree the mortality accompanying this function?

Analogous questions in regard to some surgical operations and to some diseases have been discussed, and not without good results. This circumstance renders it the more extraordinary that the questions I have proposed regarding parturition have stimulated so little inquiry. The topics of childbearing and of its mortality, and particularly the indefinite disease puerperal fever, are among the most interesting and carefully studied in the whole range of medicine, and the neglect of the two questions named I can attribute only to the want of materials for their settlement. Yet I do not hesitate to say, that had the profession set these questions before them in their simplicity and importance, materials would ere now have been found or accumulated, and their most desiderated solution satisfactorily effected ere this time.

I regret that, at present, I know of no data sufficient to decide the questions raised. Yet I shall lay before the profession such as I have collected. They are deficient in point of number and of precision. Had the numbers been much larger the results would have had value in spite of the want of precision in the data as arranged for comparison. The element of want of precision consists mainly in the comparison of different pregnancies not being confined to women of the same age. This condition is of course neces-

sary to ensure against a probable source of error, the amount of
which is unknown, consisting in the disturbing influence of age
upon mortality.

It is well known that a large amount of puerperal mortality is
produced by that indefinite class of diseases unphilosophically and
injuriously combined under the name of puerperal fever.   So
important is this class of diseases that it appears to me worth while
to discuss separately the influence of age on the mortality from
puerperal fever.   The same should be done for all causes of death
in or after labour as soon as data are collected.   In the case of
puerperal fever some data are at hand.   It is not my object, at
present, to enter on the vexed questions in reference to this erroneously
so-called fever.   No doubt many grave and cardinal errors prevail
regarding this class of diseases, and statistics may contribute aid to
demolish some of them.   The invasion of this disease is well known
to be described by a class of obstetricians as an "accident."   To
remove it from this category is a just object of ambition.   To some
extent this has already been effected by Dr Simpson, who has shown
that it is subject to the law of the duration of labour.[1]   The object
will be further promoted if it can be shown to be under a law of the
number of the pregnancy, or of the age of the mother, or of both.

## I. *The Relation of the Number of the Labour to the Mortality from Puerperal Fever.*

It may at first sight appear unnatural to enter upon a special
kind of mortality in childbed, before describing the whole mortality
in childbed.   And it is worth while to consider this point briefly,
to show that, at least with the means at my disposal, the influence
of age is better and more securely observed in this special kind of
mortality than in the whole mortality of childbed.   For, in propor-
tion as puerperal fever has the quality of an accident, as many
obstetricians believe, so will the unalloyed influence of the number
of the pregnancy upon its occurrence be observable.   Were it
evidently not an accident, but due to this cause or the other cause,
it would be more and more difficult to eliminate the influence of
such causes upon the mortality with a view to arriving at the results
produced by the number of the pregnancy.   There are, specially in
many first labours, such evident and direct causes of death in many
cases that the influence of the number of pregnancy can make no
alteration in the fate of the mother.   Such cases, in however great
numbers, can throw no light on the influence we are studying.   In
proportion as such cases are intermingled with others fitted to throw
light on the subject, so will they obscure that light.   Deaths in
childbed from puerperal fever are, to some small extent, truly
described as accidental : no cause for the supervention of the disease
may be detected ; just as this is the case, so will be the value of the

[1] Obstetric Works, vol. i. p. 530.   See also Edinburgh Medical Journal for
July 1857.

testimony of such accidents to the influence of the number of the pregnancy.

Before discussing generally the influence of the number of the different successive pregnancies, I shall compare, first of all, the influence of primiparity as compared with that of births after all subsequent pregnancies. It is well known that first pregnancies are, as a whole, attended by a much greater mortality than subsequent pregnancies, and this is a circumstance which scarcely demands explanation, for the primiparous woman has a longer and more difficult labour than others; many primiparæ are delivered under the influence of depressing mental emotions; in primiparous women all the arrangements, mechanical and other, for delivery are tested, and subsequent deliveries occur only in those who have so far successfully endured the trial as to survive. But it is particularly illustrative of the topic of this paper to inquire what effect primiparity has in labours that are natural, in women against whose chances of recovery nothing is known, who have easily passed through their trials.

Drs Johnston and Sinclair, in their valuable work on " Practical Midwifery," describe 11,874 cases in which the labours were " purely natural." Of these 3699 were examples of primiparity. There were, therefore, 8175 births after the first. Of the 3699 primiparæ, whose labours were purely natural, 20 died of puerperal fever. Of the 8175 natural deliveries in women who had already passed safely through the dangers of parturition, 21 were followed by puerperal fever and death. To compare these proportionally, among purely natural deliveries in primiparæ, every 185th woman died of puerperal fever, or ·57 per cent.; while, among similar deliveries in multiparæ, only every 389th woman died, or ·25 per cent. Puerperal fever deaths are described by Messrs Johnston and Sinclair as " considered accidental." Their interesting data show that primiparæ are very greatly more liable to this awful accident than others.

In order to illustrate this particular point I have no other collection of cases of natural labour to refer to, and therefore nothing so valuable and directly applicable. But I shall adduce evidence derived from more general collections of cases, including all kinds of labour.

Professor Hugenberger, of St Petersburg, has published some observations on this point made in hospital practice.[1] Of 2253 primiparæ 97 died of puerperal fever. Of 5783 multiparæ 141 died of the same class of diseases. Among the primiparæ puerperal fever death seized every 23d woman, or 4·35 per cent.; while among the multiparæ death in the same form seized only every 40th woman, or 2·44 per cent.

[1] Das Puerperalfieber im St Petersburger Hebammen-Institute von 1845–1859. S. 24. Separat-Abdruck aus der " St Petersburger Medicinischen Zeitschrift."

Dr Collins, in his "Practical Treatise," describes 56 deaths from puerperal fever. Of these, 30 occurred among 4969 primiparæ, and 26 occurred among 11,445 multiparæ. Among the primiparæ every 165th woman died of this disease, or ·6 per cent. Among the multiparæ every 440th woman died of it, or ·23 per cent.

Among the married women whose deliveries were registered in Edinburgh and Glasgow in 1855, there were 58 puerperal fever deaths. Of these 26 occurred in 3722 primiparæ, and 32 in 12,671 multiparæ. Of the primiparæ puerperal fever carried off every 143d, or ·7 per cent. Of the multiparæ every 396th, or ·25 per cent.

Having shown by the statistics already brought forward that deaths from puerperal fever are among primiparæ at least twice as numerous as among multiparæ, I proceed now to inquire into the comparative mortality from this cause in labours following succeeding pregnancies.

Hugenberger devotes a short paragraph to this topic, and gives interesting data, which I here produce in a tabular form :—

TABLE 1.—*Showing the Mortality from Puerperal Fever in Different Pregnancies. From Hugenberger.*

| No. of Pregnancy. | No. of Mothers. | No. of Deaths. | Per-centage of Deaths. | Or One in |
|---|---|---|---|---|
| 1st | 2253 | 97 | 4·30 | 23 |
| 2d to 4th | 4031 | 85 | 2·11 | 47 |
| 5th to 9th | 1563 | 47 | 3·01 | 33 |
| 10th to 19th | 189 | 9 | 4·76 | 21 |

This table of Hugenberger's data justifies his remarks. He says that the greater or less frequency of previous pregnancies appears to be not without influence upon the lying-in, for while those pregnant from the second to the fourth time show the most favourable results, the first increment of mortality begins with those in the fifth to the ninth pregnancies ; and a greater mortality still is observed in women in the tenth to the nineteenth pregnancies. If we could dare to adopt as demonstrated what Hugenberger's data seem to show, and as yet I have adduced nothing calculated to shake their evidence, we should have an extremely interesting addition to our knowledge of the influence of the number of the pregnancy upon the danger of confinement. It would appear that from the very great danger of a first confinement, woman passed into a period of comparative safety in the next succeeding confinements, till she came to about the fifth lying-in, when danger began to increase ; and as pregnancy succeeded pregnancy, danger still farther increased, until it reached a degree as great as that of a first confinement.

An interesting contrast of these results with what is known of

the fecundity of women at different ages may be made. The average age of wives in Edinburgh and Glasgow bearing first children is 24 years. The average age of wives bearing fifth children is 31 years. From the 25th to the 30th year, women are more fertile than at any other time. It is within the ages of 25 to 30 that are included the average ages of women bearing second, third, and fourth children, those produced with least danger to life. Hence, if the data are good and sufficient, there is a coincidence between the time of the greatest amount of safety and that of the greatest fecundity; and diminished fecundity, or likelihood of having children occurs when danger is great; that is, in first pregnancies and in fifth and subsequent pregnancies, or in pregnancies of women below 25 years of age and above 30. But this point will be better and more directly demonstrated when the influence of age is itself discussed.

I shall now bring forward other data similar to Hugenberger's, with a view to observing whether they confirm his results or not.

In Edinburgh and Glasgow, in 1855, there were 58 puerperal fever deaths of wives, all occurring before the ninth pregnancy. There were in that year delivered 15,384 wives pregnant for the eighth time or less. Arranging these according to the number of the pregnancy, we have the following:—

TABLE II.—*Showing the Puerperal Fever Deaths of Wives delivered in Edinburgh and Glasgow in 1855.*

| No. of Pregnancy. | No. of Mothers. | No. of Deaths. | Per-centage of Deaths. | Or one in |
|---|---|---|---|---|
| 1 | 3722 | 26 | ·698 | 143 |
| 2 | 2893 | 8 | ·276 | 361 |
| 3 | 2534 | 11 | ·434 | 230 |
| 4 | 1982 | 6 | ·303 | 330 |
| 5 | 1543 | 2 | ·129 | 721 |
| 6 | 1221 | 2 | ·164 | 610 |
| 7 | 848 | 1 | ·118 | 848 |
| 8 | 641 | 2 | ·312 | 320 |

This table is scarcely a fit object of comparison with Hugenberger's, for it will be observed, that while his table has cases of death in women even in the nineteenth pregnancy, no wife died after delivery in Edinburgh and Glasgow in 1855 whose pregnancy was above the eighth. So far as this imperfect table goes, however, it is somewhat in opposition to the general tenor of the results published by Hugenberger.

In the work on practical midwifery by Johnston and Sinclair, I find a table of 75 puerperal fever deaths, in 74 of which the number of the pregnancy is given. Unfortunately, I can discover in the work no data regarding the number of the pregnancies of the whole women delivered. Unwilling, however, to lose any advantage that

may be gained from the table of pregnancies of 74 puerperal fever deaths, I have in the following table arrranged them for comparison with the whole women delivered in Edinburgh and Glasgow in 1855. Of course the per-centages derived from this comparison are

TABLE III.—*Showing a Comparison of Puerperal Fever Deaths in the Dublin Hospital with the Number of Parturient Wives in Edinburgh and Glasgow in 1855.*

| No. of Pregnancy. | No. of Mothers. | No. of Deaths. | Per-Centage of Deaths. | Or One in |
|---|---|---|---|---|
| 1 | 3722 | 40 | 1·07 | 93 |
| 2 | 2893 | 6 | ·27 | 482 |
| 3 | 2534 | 11 | ·43 | 230 |
| 4 | 1982 | 3 | ·15 | 661 |
| 5 | 1543 | 3 | ·19 | 514 |
| 6 | 1221 | 4 | ·32 | 307 |
| 7 | 848 | 2 | ·23 | 424 |
| 8 | 641 | 0 | | |
| 9 | 425 | 3 | ·70 | 141 |
| 10 | 222 | 1 | ·45 | 222 |
| 11 | 152 | 1 | ·65 | 152 |

not figures of actual value, but only of value for comparison with one another; and it is interesting to observe that they roughly confirm the results of Hugenberger. After a great mortality in first pregnancies, there is a great improvement in second, third, and fourth pregnancies, and then, again, as the fifth pregnancy is passed the mortality rises as the number of the pregnancy increases. It must be admitted that this accordance is not very exact, the regularity of the results being disturbed by the great mortality in third pregnancies and the absence of mortality in eighth pregnancies. There can be no doubt that the value of the table is not very great; yet it evidently points towards confirmation of Hugenberger. Larger and better data are required to produce a satisfactory assurance.

## II.—*The Relation of the Number of the Labour to the Mortality accompanying Parturition.*

In pursuing this topic I shall follow the same course as I observed in describing the mortality from puerperal fever, beginning by a comparison of the mortality of first labours with that of all subsequent labours.

The first data which I adduce are Johnston and Sinclair's 11,874 cases of purely natural labour. These are specially valuable for the purpose, for nearly the same reasons as enhanced their value when puerperal fever was the only cause of mortality under consideration, reasons which, therefore, need not be repeated here. Of 11,874 purely natural cases, 3699 were first labours, and 8175

subsequent labours.  Of the primiparæ 33 died, and of the multi-
paræ 34 ; or 1 in 112 of the former, or ·89 per cent., and only 1 in
240 of the latter, or ·41 per cent.

These purely natural cases form part of a total of 13,748 labours
described by Drs Johnston and Sinclair.  Of these, 4535 were
primiparæ, and 9213 multiparæ.  Among the former 83 died, or 1 in
54, or ·18 per cent., and among the latter 80 died, or 1 in 115, or
·86 per cent., a mortality, it is to be remarked, relatively almost the
same as among the purely natural labours.

We may now take those labours alone which were not purely
natural.  Of these, 836 were in primiparæ, and of these 50 died,
being 1 in 17, or 5·98 per cent. ; while 1037 were among multiparæ,
and of these 46 died, being 1 in 22, or 4·43 per cent.  Here it is at
once observed that the relative mortalities are nearly alike, forming
a striking contrast to the relative mortalities under any other
circumstance adduced.  It is unfortunate that this striking observa-
tion is founded on so few data.  It cannot fail to excite reflections
in the practical obstetrician.  These would be out of place in this
article, and I shall only diverge so far as to remark that here the
primiparæ evidently hold a very greatly improved position, that
while in natural labour puerperal fever carries off proportionally
twice as many primiparæ as multiparæ, that while in labours
generally, twice as many of primiparæ die as of multiparæ ; yet
in unnatural labours the balance is nearly straight, the primi-
paræ enduring the cause of danger conjoined to all the evils con-
nected with primiparity, nearly as well as multiparæ endure the
cause of danger alone, without the evils which all the statistics
hitherto adduced show to attend primiparity.

In Dr Collins' " Practical Treatise," the deliveries of 16,414
women are described.  Of these, 4969 were primiparæ, and 11,445
were multiparæ.  Among the whole there occurred 164 deaths, but
the number of the pregnancy is given only in 160 cases.  Of these
160 deaths, 80 occurred among the primiparæ, being 1 in 62, or 1·61
per cent. ; and 74 occurred among the multiparæ, being 1 in 155, or
·64 per cent.

In the work of Messrs Hardy and M'Clintock, on midwifery and
puerperal diseases, 6635 cases of delivery are described.  Of these,
5852 are described as natural deliveries.  Among them were 1752
first labours, and 4100 subsequent labours.  In the former the
deaths were 7, being 1 in 250, or ·399 per cent. ; in the latter 9,
being 1 in 455, or ·22 per cent.

The whole cases in the work of M'Clintock and Hardy are, as
already said, 6635.  Of these, 2125 were in primiparous women,
and 35 died, being 1 in 60, or 1·65 per cent.  Among multiparæ
were 4510 deliveries and 30 deaths, being 1 in 150, or ·66 per cent.

In Edinburgh and Glasgow, in 1855, there were 16,393 deliveries
of married women.  Of these, 3722 were in first labours, and 50
died within six weeks after delivery, being 1 in 74, or 1·34 per cent.

The multiparæ numbered 12,671, and of these 103 died, being 1 in 123, or ·81 per cent.

Having thus compared the mortality of primiparæ with that of all other parturient women, I proceed to inquire into the mortality of each successive pregnancy.

The accompanying table is made from the Edinburgh and Glasgow registers for 1855. It exhibits the number of wives delivered in each successive pregnancy, their mortality, and the

TABLE IV.—*Showing the Mortality among Wives delivered in Edinburgh and Glasgow in 1855, in each Successive Pregnancy.*

| No. of Pregnancy. | No. of Mothers. | No. of Deaths. | Per-centage. | Or One in |
|---|---|---|---|---|
| 1 | 3722 | 50 | 1·343 | 74 |
| 2 | 2893 | 24 | ·829 | 120 |
| 3 | 2534 | 25 | ·986 | 101 |
| 4 | 1982 | 13 | ·655 | 152 |
| 5 | 1543 | 13 | ·842 | 119 |
| 6 | 1221 | 7 | ·573 | 174 |
| 7 | 848 | 7 | ·825 | 121 |
| 8 | 641 | 8 | 1·248 | 80 |
| 9 | 425 | 3 | ·706 | 142 |
| 10 | 222 | 1 | ·450 | 222 |
| 11 | 152 | 1 | ·658 | 152 |
| 12 | 61 | ... | ... | ... |
| 13 | 34 | ... | ... | ... |
| 14 | 11 | ... | ... | ... |
| 15 | 6 | 1 | 16·666 | 6 |

per-centage of mortality to deliveries. Casting the eye along the per-centage column of this table, one does not discover any marked indication of a regular variation after the great mortality of primiparæ is passed.

I have no other similar directly procured data to add to what is given in the last table (IV.) The authors from whom I derive the following data as to the pregnancies of women dying after delivery do not give the pregnancies of all their cases with which to compare the pregnancies of those that died. But I here make the data regarding deaths available by comparing them with the pregnancies of the whole wives delivered in Edinburgh and Glasgow in 1855. This composite table will thus not give results or per-centages representing actual values, but only results for mutual comparison, and I venture to think they are valuable. The table is prepared as follows:—The first column states the number of the pregnancy; the second gives the number of wives delivered in Edinburgh and Glasgow in 1855, in each successive pregnancy; the third column gives the number in each successive pregnancy of a collected mass of cases of childbirth deaths; the fourth column gives the per-centages of these deaths in the deliveries in each

TABLE V.—*Showing a Comparative Per-centage of Deaths in Successive Pregnancies.*

| No. of Pregnancy. | No. of Mothers. | No. of Deaths. | Per-centage. | Or One in |
|:---:|:---:|:---:|:---:|:---:|
| 1 | 3722 | 254 | 6·82 | 15 |
| 2 | 2893 | 60 | 2·07 | 48 |
| 3 | 2534 | 64 | 2·52 | 39 |
| 4 | 1982 | 39 | 1·97 | 51 |
| 5 | 1543 | 31 | 2·01 | 49 |
| 6 | 1221 | 28 | 2·29 | 43 |
| 7 | 848 | 16 | 1·88 | 53 |
| 8 | 641 | 15 | 2·34 | 42 |
| 9 | 425 | 13 | 3·06 | 32 |
| 10 | 222 | 9 | 4·05 | 24 |
| 11 | 152 | 5 | 3·28 | 30 |
| 12 | 61 | 1 | 1·64 | 61 |
| 13 | 34 | 4 | 11·77 | 8 |
| 14 | 11 | ... | ... | ... |
| 15 | 6 | 1 | 16·66 | 6 |

successive pregnancy.  In the third column are given 540 deaths gathered with care from the following sources:—153 from the Edinburgh and Glasgow registers above referred to; 160 from a table in page 364 of the Practical Treatise of Dr Collins; 162 from the Practical Midwifery of Drs Johnston and Sinclair; and 65 from the work on Midwifery and Puerperal Diseases of Drs M'Clintock and Hardy.

This last table appears to me to show with considerable force that, after a woman has passed her ninth pregnancy or thereabout, she comes gradually into more perilous childbearing, danger increasing with every unit added to the number of her children.

Having now led all the evidence I intend to adduce, I shall, in conclusion, add a few general and recapitulatory remarks.

First of all, it must be noted that I have, hitherto at least, said nothing regarding the nature of the relation between the number of the delivery and the mortality attending it.  It is true the data recorded demonstrate more or less completely certain coincidences, which may be called laws.  But they establish nothing further. These laws are as follows :—

1. The mortality of first labours is about twice the mortality of all subsequent labours taken together.

2. The mortality from puerperal fever following first labours is about twice the mortality from puerperal fever following all subsequent labours taken together.

3. As the number of a woman's labour increases above nine, the risk of death following labour increases with the number.

4. As the number of a woman's labour increases above nine,

the risk of death from puerperal fever following labour increases with the number.

5. If a woman has a large family she escapes extraordinary risk in surviving her first labour, to come again into extraordinary and increasing risk as she bears her ninth and subsequent children.

These laws, although they merely state coincidences, have very important practical bearings, which are too self-evident to require description. They have also important philosophical bearings, which were alluded to in the commencement of this article. The most important, perhaps, of these relate to puerperal fever. These also I shall not enter upon farther than to say, that the attendance of puerperal fever specially on primiparæ, and women who have born large families,—its pretty close correspondence in relative amount to the general mortality of parturition after different pregnancies,—its subjection also to the law of the duration of labour,—do not appear to me to lend support to the views hitherto generally entertained regarding it, and expressed in the words accidental, fever, contagious, epidemic. Another point under this head I shall merely mention. Authors, comparing the mortalities of lying-in-institutions, whether from puerperal fever or from other causes, are frequently found neglecting to begin by ascertaining whether or not they are fit objects of comparison, and under this head, *inter alia*, neglecting to ascertain the comparative amount of primiparity in each institution. It is plain that, unless there be nearly the same comparative amount of primiparity in the institutions, their respective gross mortalities cannot be justly compared with one another.

The well-known protraction of labour in primiparæ may to some appear a sufficient cause of the increased mortality of first child-bearing. But mere prolongation of labour for a few hours cannot, in my opinion, be regarded as any satisfactory explanation of the causation of this increased mortality. In one set of Johnston and Sinclair's cases, the labours of primiparæ are called purely natural, and they are compared with similar purely natural cases in multi-paræ; and the mere addition of a few hours to the length of labour in such primiparæ is not a sufficient cause of their mortality being twice as great as that of similar multiparæ. Denman alludes to " a vulgar and pernicious error which makes no distinction between the slowness and the danger of a labour." It would be to fall into this error to explain the increase of mortality merely by increased length of labour.

It must be held as proved, that according as labour increases in length, so the mortality accompanying it increases ; and this is true not only of the whole mortality but also of the special mortality from puerperal fever. This law, although it must have weighty bearings on the mortality of primiparæ with their long labours, cannot be regarded as to any great degree throwing light on it ; for we find new increments of mortality after the ninth

labour, when we have no reason to believe that labour is more prolonged than in labours preceding the ninth, and in which the mortality is less.    In other words, we have the number of the labour denoting increase of mortality where there is no evidence of accompanying increase of its duration.    The law of duration, then, does not enable us to explain the variations of mortality in different labours.

To completely exclude the influence of the law of duration would be very desirable ; but we see no present prospect of doing this, except by processes of reasoning.    Without such, it could only be done by comparing a series of labours of different number, but in all which the duration was the same.

It must be remarked that, while the law of duration certainly has important bearings on the data and arguments herein adduced to show the influence of the number of the labour, and while the extent of these bearings is undecided, it is at the same time equally sure that the law of the number of the pregnancy has important bearings on the data and arguments adduced to show the influence of the duration of labour, and the extent of these bearings is undecided.    The mutual influence of the data and arguments in these demonstrations must be great, and it remains for future observers to accumulate materials for either showing the amount of these influences or for a separate demonstration of the laws by data which do not intermingle them in their conditions.

It is worth while to remark, that, restricting for a moment our regard to the great mortality of primiparæ as it exceeds that of multiparæ, taken together, we have a set of cases—those of Johnston and Sinclair—where the deaths were from puerperal fever, and in which the average increase of duration of labour in primiparæ above that observed in multiparæ was 4 hours.    In multiparæ the average duration was 8 hours ; in primiparæ 12 hours. Looking at this increased duration, and the correspondingly increased mortality in primiparæ, with the light thrown on it by tables published by various authors to demonstrate the law of duration, it appears to me that the increase of mortality in primiparæ is greater than these tables appear to give as the increase corresponding to a rise in duration from an 8 hours' labour to a 12 hours' labour.

These various remarks I have made with a view to keeping the demonstration of the influence of the number of labour on childbed mortality in its proper light, to keep it separate from other laws or supposed laws with which it may be confused.    I have alluded, with this view, to the causation of the variations of mortality according to the number of the pregnancy.    It is no main part of this paper to enter on this subject, but a few words may not be out of place.    It would be foolish to imagine that any injurious influence or the reverse could spring from the mere number of the pregnancy.    A woman in a first may and often does have as fortu-

nate a delivery as in any other. To ascribe to the number of pregnancy any potency would be to fall into the error of those students of the duration of labour who ascribe great potency to the mere addition of length to a labour. In the case of the law demonstrated in this paper, and in the case also of the law of the duration of labour, it appears to me that the causation of the variations of mortality is to be looked for in the introduction of complications. I here use the word complications in a much wider sense than is generally ascribed to it, wishing it to imply injuries or injurious tendencies far slighter than those ordinarily classed as complications of labour. I have no doubt that all of these, however minute or slight, have their weight in giving proclivity to a fatal termination of the childbed. Puerperal fever may have its root in an otherwise insignificant perineal laceration as well as in a phlebitis or endometritis.

In primiparæ, as labour goes on, complications occur which are not nearly so liable to attack a woman in her next subsequent labours. These have their origin in various sources, chiefly in mechanical difficulties, and these often so slight as not to take the case from the category of purely natural, in an arrangement where the labour is alone taken into consideration, to the exclusion of the childbed.

Multiparæ are specially and increasingly liable to complications of a different kind connected with constitutional diseases, and with local infirmities of the uterus.

This introduction of complications forms also the main explanation of the law of the duration of labour. Indeed, in a rough way, it may be held that the statement of duration is a statement of the increase of complications; for it is known that, as labour lengthens out, so complications increase in frequency. Without these complications duration would be of small importance, as the profession has generally held. Their introduction is present evil and the seed of future disasters. Tables have been framed to show the increasing introduction of complications as labour is prolonged, but I only refer to them. They are quite insufficient, so far as I know them, because they are founded only on an enumeration of those of the graver sort. Further, the introduction of complications is not ruled exclusively by the duration of labour. Many are rather connected with precipitate parturition. The complications which probably contribute largely to produce the increased fatality of labours after the ninth are not all included, or capable of inclusion, in any statement of duration, being present before and after the process.

ARTICLE II. — *Cullen and Gregory upon Change of Type in Inflammation.* By GEORGE W. BALFOUR, M.D.

(*Read before the Medico-Chirurgical Society, 7th June* 1865.)

THE late Dr Alison, in his Clinical Lectures,[1] and in his several papers upon what may be called the blood-letting controversy,[2] has based his opinions as to the efficacy of blood-letting in true pneumonia chiefly upon the statement, that the pneumonia of the present day is a *pathological* term defining a disease recognised by the physical signs of inflammatory effusion, while, in Cullen's day, the same term was applied to a *nosological* disease, including not simply inflammatory effusions, but inflammatory effusions " taking place in connexion with a certain group of symptoms—pain, deranged function of the affected parts, and inflammatory fever," and, in particular, with " a *frequent, full, hard, firm, and quick* (*i. e.* sharp) pulse ; that, therefore, the pneumonia of the present day would, under some other name, have been treated just as it now is, without venesection, while the pneumonia of Cullen's day, if it were to occur now, would still require precisely the same treatment as then,—that is, full and repeated bleeding continued so long as the pulse presented the above characters and the dyspnœa lasted." The opinions of a medical man so cautious, so trustworthy, and so distinguished as Alison are certainly deserving of the respectful attention of every physician ; I think, indeed, that we are all at one with him in regard to his fundamental statement, and if any of us differ from him in regard to the therapeutic corollary based upon it, I believe we are able to give good reasons for that difference.

I do not, however, at present intend to enter at large upon the question of the treatment of pneumonia. I only propose laying before you a few facts as to the prevalence of typical pneumonia in the days of Cullen and Gregory, and as to the nature of the cases which were treated by large blood-lettings in their days, so that by these we may be enabled to judge of the probable amount of truth contained in Dr Alison's opinion as expressed in his therapeutic corollary.

In the library of the Edinburgh College of Physicians there are no fewer than seven[3] volumes of MS. Clinical Lectures by Cullen, extending over several years, from 1764 to 1774, and comprehending a large number (upwards of 200) cases. I have looked carefully over the whole of these to see if I could find any cases of typical pneumonia or pleurisy, but I have been unable to find one single

[1] Edin. Med. Jour., Aug. 1850, p. 162; and Nov. 1852, p. 495.
[2] *Op. cit.*, March 1856 and May 1857.
[3] There are nine MS. volumes lettered, " Clinical Lectures," one of them is, however, a volume of Lectures on Practice of Physic, and another is a duplicate.

case; and this is so far satisfactory, as showing that typical pneumonia was quite as rare a disease in Cullen's day as it is now, and it also confirmed Dr Home, senior's, statement, quoted by Alison, that " true pleurisy (or pneumonia) is a very rare disease here."[1] Most of the cases recorded are precisely such as we find in hospital now-a-days, only there are a good many cases of intermittent fever, and of course a considerable—but no ways remarkable—number of cases of inflammatory fever and catarrh; and among them, though the frequency of the pulse is rarely mentioned, and is very often stated to be soft and not always full, sometimes indeed under 100, yet none escape the inevitable blooding on two or three days successively after admission, even though that is repeatedly stated to have been upon the ninth or tenth day. Now and then, too, Cullen naïvely congratulates himself upon the case not having turned out nervous fever; in which case he adds, " *the blooding would have been pernicious.*" As a sample of his treatment of recognised catarrh, I may give the following quotation:—" Our chief remedy is bloodletting; accordingly, our first prescription was a blooding of this patient, and though very little fever is present we have repeated the blooding four times since. Its effects, indeed, in relieving the disease have not been immediately evident; but I must bid you remember the doctrine I delivered in regard to ophthalmia, that as it is, so is catarrh in a great measure a topical affection, and the more they are so, they are the more liable to elude the effects of general venesection. So our venesections here had not that effect which they would have had in peripneumonia or pleurisy."[2]

Cullen also relates two cases, both of which he at first regarded as cases of pneumonia (or, as he termed them, indifferently peripneumonia or pleurisy).[3] The first, Grisel Cockburn, had been bled before admission; she had all the ordinary symptoms of catarrh, with copious expectoration for six or seven days before she was seized with that difficult breathing and violent pain in the side which Cullen subsequently acknowledges to have been chiefly confined to the serratus magnus, and to have been altogether rheumatic in character; nevertheless he bled her thrice, though her pulse was neither full nor hard, and the immediate relief obtained was not considerable; the expectoration continued free, and four days after the third blooding the breathing and the pain were much relieved, though Cullen is unable to say whether the relief should be attributed to the blooding or to a blister which had been subsequently applied. The second case,[4] that of Colin Reid, he regarded as somewhat singular, both in its form and course. C. R. was admitted on the ninth day of his disease, yet Cullen concluded, from his state then and what happened subsequently, that no effusion of blood had taken place into the lungs. He had no stitch nor pain in the side, only a pain in the breast, which might be catarrhal—I quote the

[1] Clinical Experiments, Edin. 1683, by a curious misprint for 1783, p. 60.
[2] MS. Lectures, vol. i. p. 595.    [3] *Op. cit.*, vol. iii. p. 581.    [4] *Op. cit.*, p. 588.

*ipsissima verba* of the report,—his breathing, though difficult, was never so bad as to necessitate the erect posture, as in Grisel Cockburn's case; he was continually bawling and making a noise, which showed that his lungs could bear expansion without much pain. His case was evidently only one of violent catarrh, with inflammation of the mucous glands of the bronchia. He was twice bled on the day of his admission, because the difficulty of his breathing and frequency of his pulse showed him to be in imminent danger; he was also bled once on each of the two following days; losing in all about 3 lbs. of blood; he was blistered on both sides and on the breast. His pulse, when admitted, was 126, and gradually fell to 124, 120, 112, 116, 104, 88, and 68, on successive days, becoming irregular and intermittent after the venesection; and as this was regarded as a fatal sign, Cullen at his second visit was inclined to regard his case as hopeless.

In the library of the London Medical and Chirurgical Society, they have several MS. volumes of Clinical Lectures, both by Cullen and Gregory, in which there are several well told cases of pneumonia, which are said not to differ from the ordinary run of uncomplicated cases met with in the London hospitals at the present day: none of them being of an intensely inflammatory character, and one, at least, being of a decidedly low type; he had been bled once before admission, which took place four days after a rigor; he suffered from heat, headache, pain in the right breast, aggravated by coughing, dyspnœa, and expectoration; his pulse was full and *soft;* he was bled five times in two days, and died within forty-eight hours. There was extensive double pneumonia, and turbid, greenish serum in the pleura.[1]

In the library of the Edinburgh College of Physicians there are also three MS. volumes, one of Clinical Lectures, delivered by Dr Gregory in 1771–72, and two of Notes of Cases treated by him during 1779–80 and 1780–81. In these I was fortunate enough to find the details of seven cases of pneumonia. The first of these was made the text of a clinical lecture, and is thus introduced to the notice of his pupils as a case just admitted on January 3d.

" Betsy Moffat :[2] This is an extremely unfavourable case, with all the symptoms of peripneumony; probably a very considerable effusion has taken place into the cells of the bronchia; she is ordered to be blooded, a blister is applied to her neck, and I shall order the solution of tartar emetic, according to the particular circumstances, and venesection as the pulse will bear it; she is in very considerable danger, but I make it a rule to take in every person in an acute case." On 4th February, Dr Gregory thus continues the case, " She was seized on the 24th ult. (Dec.) with coldness, trembling, etc. You may remember upon her coming in when I was giving a clinical lecture, I made a very unfavourable prognosis from her

[1] Brit. and For. Med. Chir. Rev., vol. xxii. pp. 32, 33, 34.
[2] Clin. Lectures, 1771, p. 192.

appearance; she was breathing with the utmost difficulty, and there appeared to be a considerable quantity of matter in her breast, which was rustling in her throat, and which she appeared scarce able to throw up.   I then diagnosed a peripneumony or inflammation of the lungs; and from its having continued several days, I thought it was probable that an effusion had begun to take place in the lungs, and that the chances were much against her.   The circumstance of delirium was also extremely unfavourable.   That is one of the terminations of peripneumonia, a translation to the head very generally proves fatal." Dr Gregory then goes on to give an admirable lecture on the nature and treatment of pneumonia, which I need not give *in extenso*, merely remarking that he commences by stating that peripneumonia is often confounded with pleurisy, but that is of no consequence, as the treatment is the same.   It is called pleurisy when the pain is acute, violent, and pungent, affecting the side, with cough and difficulty of breathing, and pneumonia or peripneumonia when the pain is dull and obtuse, affecting the breast.   In pneumonia the pulse is *remarkably soft*, though the febrile symptoms run very high; when the pain affects the side principally the pulse has always a great degree of hardness and tension.   In respect to the fulness of the pulse in pneumonia, in the beginning before the patient is blooded, it is not only *soft but small;* but commonly upon the patient being blooded it becomes fuller, though it always retains its softness.   The principal remedy is blood-letting; and in all topical inflammations this ought to be made with a large orifice, in order to induce sudden relaxation, from which the chief benefit is derived.   Blood-letting ought to be timeously performed, as after effusion has taken place it is of little or no service, but, on the contrary, seems to do mischief by sinking the patient's strength.   When pneumonia goes on to suppuration the end is very generally fatal, and always very uncertain, etc., etc. Dr Gregory then goes on with the history of Betsy Moffat.

" In her case it was evident that the disease must either be resolved, or she must be suffocated in a very few hours.   Not much blood, only ℥viij. was taken from her at first; by this she was relieved, and then ℥viij. more were abstracted.   Her belly was opened by a laxative glyster, tartar emetic in solution was administered, and a blister was applied to her back; what was to be done had to be done quickly, there was no time for delay.   Next day she was a great deal better; her pulse was 134.   The day after that she was sweating over her head and neck; her urine was turbid and high coloured; she rested well in the night; her pulse was 96.   On the 6th she complained of great throbbing of her temples, which was reduced by leeches applied to them, and on the 9th she was dismissed cured.   A few days subsequently she was exposed to cold; and on the 16th she was re-admitted, afflicted with frequent vomiting, and complaints of the rheumatic kind.   She was treated as formerly : ℥viij. of sizy blood were abstracted, and tartar emetic was

administered; the venesection was repeated as the pulse was still
130.  On the 18th she had passed a bad night, and when I saw
her she appeared to be quite insensible; she could not speak; her
pulse 104, but feeble; the great degree of defluxion at her breast
and rustling in her throat was like that in those within two or three
minutes of expiring, and I should not have been surprised if she
had died within half an hour.  She was ordered strong sack whey
with hartshorn, as the most powerful cordial she could get, a blister
to be applied to her neck, and fomentations to her legs.  The case
appeared not to admit of further bleeding.  Before I left the ward,
however, it appeared that the fit was due to sickness; for having
thrown up a quantity of stuff from her stomach she was much
relieved, and, in consequence, the blister was countermanded.  She
complained of pain in her head, which was relieved by leeches,
and ℥iv. of more blood were taken from her arm; afterwards she
complained of pain in her side, with difficulty of breathing, which
was removed by the application of a blister.  She continued to
improve gradually till the 25th, when she was dismissed cured."

The next case, that of Thomas Brown,[1] is stated to be a very
severe case of pneumonia, complicated with hepatitis.  The case is
very imperfect, however, and contains no particulars; he was bled
six times,—the first time to ℥xij.,—and recovered.

On the 17th December 1779, James Anderson,[2] æt. 60, was
admitted.  " Six weeks ago, after chilliness and trembling, he was
seized with acute pain in the extremities of the short ribs on the
left side, increased on inspiration, and exciting very severe cough,
which in four or five days was attended with a copious white
expectoration, which has continued ever since.  Within these four
days, after exposure to cold, the complaints have been aggravated;
the pain has shifted to the forepart of the breast; his expectoration
has been more scanty, and he has been obliged to raise himself in
bed to expedite respiration; he now lies easiest on his right side;
till lately he found most ease when lying on the side affected;
pulse about 90, weak; face flushed; sleeps ill; bowels regular;
has had two or three vomits, and had a blister applied to his arm
with little relief; appetite gone.  Had a trembling fit last night."
He was treated by epispastics applied to the chest, and mucilaginous
drinks.  On the 19th the pulse was 84, and soft, and he had ℥vj.
of blood abstracted by cupping, with relief to the pain in the side.
On the 23d he was convalescent, and on the 24th he was dismissed.

The next case is that of James Niven,[3] æt. 26, admitted on
January 7, 1780.  " On the 4th, after exposure to cold, he was
seized with pain of the breast, about the end of the sternum, and in
the small of the back.  The complaint, after some febrile accession,
terminated by profuse sweating, which flowed copiously all next
day.  The disposition to sweat continues, but he has a sensation of
chilliness or shivering.  Pain of breast is at present severe, and

---

[1] Notes, 1780–81.     [2] Notes, 1779–80, p. 85.     [3] Notes, 1779–80, p. 147.

increased by a frequent bound cough. Tongue foulish; pulse 80, and of natural strength; bowels regular; has received no medicine; sleeps ill. *Mitti sang. ad ʒviij. Jul. mucil. ʒss. subinde.*

" 8th.—Was not bled till this morning; blood sizy and buffed; cough severe during the night: expectoration difficult; pain continues, stretching from the sternum down the sides; pulse 96, and not very strong. *App. C. C. ad ʒvj. parti dolenti, sol. T. E. ʒss. 0·½ h. ad 4tem vicem nisi vomitus prius elicitur. Bibat decoct. furfuris, ad lib.*

" 9th.—Has vomited well; pain in the breast and cough relieved; expectoration more free, not yet copious, and very slightly tinged; pulse 82; bowels rather bound; copious sediment in urine. *Cap. elect. a chryst. tart. ʒss. C. M. S.*

" 10th.—No effect from purgative; pulse 72; skin cool and soft; free from cough, but slept ill; some viscid expectoration, without tinge. Continue.

" 11th.—Some pain in his back; slight tinge in the expectoration; slept well, and continues easy. Continue.

" 12th.—Physic operated well yesterday afternoon; slept ill; complains of pain in back. *Cap. H. A. ad. gtt. xx.*

" 13th.—Slept ill; pains in his back. *App. C. C. parti dolenti ut educantur sanguinis, ʒvj.*

" 14th.—Pain relieved.

" 15th.—Dismissed cured."

Next comes the case of Daniel Forbes, æt. 40,[1] admitted on January 8, 1780. " About a week ago, while undergoing a mercurial course for lues venerea, of six weeks' standing, was exposed to cold, and soon after seized with pain in his back and left side, at the end of the short ribs, increased on inspiration; has little cough, and lies best on his right side. The symptoms of lues venerea present are superficial ulcers on the penis. Has formerly been subject to ague, and complains of pain chiefly in the hypochondrium. Pulse 96, and pretty firm; bowels natural. Has had no medicine but physic. Bled to ʒviij. Next day his pain and cough were relieved; his pulse 96; the blood displayed a buffy coat, and he was cupped to ʒvj. on the pained side. Next day the pain and cough were much relieved; but the following night, about midnight, the pain in the side recurred with some cough and little expectoration, and he was again bled to ʒx. Next day, the 12th, the blood was found buffed; the pain was relieved; he had but little cough; his pulse was 108, and soft. On the 13th the cough was much relieved; the expectoration slightly tinged; the pain in the side gone, but that in the back continued; his pulse was 96, and firm, he slept ill. *H. A. gtt. xxx.* 14th, Pulse 90; slept well; his cough to-day spits up a good deal of matter tinged with blood; still some pain in the back, but breathing easier. 15th, Convalescent, and soon thereafter dismissed.

[1] Notes, 1779–80, p. 150.

The next case is that of Duncan Maccorkadale,[1] æt. 50, a glass-worker at Leith. " Complains of severe pains in the breast, which shift about from place to place, but principally affect his right side, shooting from thence backwards; he has also severe cough and scanty difficult expectoration and dyspnœa. He lies best on the right side. His complaints are of five weeks' standing, and attacked him in consequence of exposure to cold while sweating. The febrile symptoms which were at first severe are now remitted; but complains of oppression and vertigo towards evening. Pulse 90; bowels bound; tongue clean. *Hab. Infus. lini.*"

On the 12th he got antimonial solution. On the 13th his cough was still troublesome, but expectoration free; his pulse natural; his tongue foul; he still complained of a stitch in his left side, and for this he was bled to ℥viij. On the 16th he was cupped on the temples to ℥vj., after which he continued to progress favourably, and was discharged on the 29th.

The seventh and last case is that of Katherine M'Donald,[2] æt. 22, admitted on January 11. On the 1st she was seized with severe pain in the right side, and had a febrile accession. She had been subject to cough for about a month. The pain has continued ever since, but always aggravated at night; it strikes her from the extremities of the ribs to her shoulders, and for these five nights past has prevented her lying on the right side. Within these few days the expectoration has been more copious than before; her breathing is difficult; she has chilly fits at night, which she attributes to her being obliged to sit up in bed at night. On the 3d she was vomited without relief; on the 5th she was bled, since when she thinks the pain has increased; her pulse is 100, of natural strength; she was cupped to ℥xij. On the 12th her pulse was 90, and soft. On the 13th it was 84, and her skin cool; and on the 15th she was dismissed cured.

These nine cases of alleged pneumonia, which are all that I have been able to collect out of ten volumes of Clinical Lectures and Notes, extending over—though not comprising—a period of nearly forty years, teach us distinctly,—*first*, That typical pneumonia was at least as rare a disease in those days as it is now. *Second*, That the ordinary nosological pneumonia of the clinical wards in the days of Cullen and Gregory was as widely different from the typical pneumonia of Cullen's Synopsis as our modern pathological pneumonia can possibly be; indeed the cases seem to resemble very much the ordinary run of cases of pneumonia coming to hospital now-a-days, differing only in this that now we know positively when we have to deal with pneumonia, whereas in those days the diagnosis, as we may see, was extremely uncertain. *Third*, It is evident that however much consideration was bestowed on the state of the pulse and the breathing, as criteria for blood-letting, that such was the dread of inflammation that the mere persistence of pain or other

[1] *Op. cit.*, p. 163.                    [2] Notes, 1779–80, p. 199.

uneasy sensation was considered sufficient to warrant the abstraction of blood, even though this might otherwise seem hazardous. *Fourth*, We learn from these cases that even in hazardous circumstances the abstraction of blood is not, in skilful hands, by any means a fatal operation, nor does it necessarily much increase the mortality from pneumonia,[1] though we have the testimony both of Alison, Cullen, and others, that blood-letting necessarily protracted the period of convalescence. From Cullen's clinical lecture on the case of Colin Reid, already quoted, we learn the reason of the very active treatment to which inflammation of the lungs was in these days submitted. He states, " That effusion of blood into the parenchyma of the lungs always kills before the ninth day, or at least does not admit of cure, as you will observe from the cases of that kind that you have in Lieutaud's *Historia Anatomico-Medica*, in which there are sixty-five such cases of dissections. Of these there are a great number in which the inflammation of the lungs was an accidental occurrence, and by no means the original disease. Accordingly, twenty-three cases out of the sixty-five are narrated without mentioning the day upon which the patient died. Of the remaining forty-two, there are twenty-four of them in which the day of the death is marked, and at the furthest it happened upon the ninth day, and there are only seven that happened upon the eighth and ninth, the deaths in the other cases occurring before that period; and with regard to those that died some days beyond the ninth day, they are generally those cases that ended in suppuration, or such as were ambiguous, and generally died upon the tenth or eleventh day. Many more of them, however, die upon the ninth day, it being readily mistaken for the tenth, as if a patient is not attacked till noon, and dies on the morning of the tenth day, that is still only the ninth, the tenth day not commencing till after noon. In Cleghorn's *Diseases of Minorca*, we also find that such persons died very early, or else ended in suppuration; and in those who die so early it is by an effusion of blood into the cellular tissue of the lungs." From this very clear and distinct statement, it is obvious that in the absence of any means of precise diagnosis the physicians of those days, whenever they met with any patient having even dubious symptoms of a peripneumonia, considered themselves justified by their pathological doctrine and the supposed dangerous nature of the malady in bleeding them to within an ace of their lives, if seen before the ninth day; while, if not seen till after the ninth day, the very fact of the patients having lived so long seemed to prove that there was something anomalous in the course of the disease, and justified their still having recourse to their *summum remedium*, acknowledging, meanwhile, that in such cases it was but a forlorn hope, and only employed in accordance with the old dogma, *Melius anceps remedium quam nullum*. In their day and

[1] The use of blisters and epispastics in those days seems to have been one great means of keeping the flagging heart going.

generation, and according to their lights, Cullen and Gregory acted no doubt wisely and well; and we must agree with Gregory in thinking, that, from his own point of view, Dr Radcliffe was "no fool," when at the age of sixty he submitted to the loss of one hundred ounces of blood, not because he thought the remedy safe, but because he considered it less dangerous than the disease; though, I think, that had both been spared till our day they would have taken rather a different view of the matter. Indeed, but for that worst of medical failings, an overweening estimation of their own times, of the advanced state of their own knowledge, and of the most recent work upon pathology, to the exclusion of a due consideration of the history of the art, they possessed even then the means of connecting many of their views, and a little more attention bestowed upon "the doctrines of *nature curing diseases*, the so-much vaunted *Hippocratic* method of curing," which Cullen derided as having "a baneful influence on the practice of physic, and as either leading physicians into or continuing them in a weak and feeble practice, and, at the same time, superseding or discouraging all the attempts of art,"[1] could scarcely have had "a baneful influence" on the practice of such energetic physicians, and might possibly have somewhat modified some of those displays of high art in medicine which astonish us not more by the boldness than by the apparent impunity with which they were practised.

Quite recently it has been alleged that, "If at a former period blood was taken too often, in too great quantity, and too indiscriminately, it is now certain that at the present day too little use is made of this remedy. . . . . . A circumstance which is of special significance in this question, and which, with few exceptions, justifies bleeding, is the existence of hardness of the pulse. This kind of pulse separates abruptly the influenzal pneumonia from acute sthenic; with respect to its adaption to venesection, the former bears bleeding very badly, in the second it is useful and necessary."[2]   Now, in regard to this view it is of importance to remember that the existence of "a frequent, full, hard, firm, and quick (*i.e.* sharp) pulse," is not a necessary symptom of sthenic pneumonia; because if ever pneumonia was truly sthenic, and if ever it could be supposed to require blood-letting, as it certainly did bear it well, this was in the days of that great champion of venesection, Dr Gregory; and yet we have his authority for stating that the pulse in pneumonia is not only "remarkably soft," but also "small," and that it only becomes hard when the pain affects the side,—*i.e.*, when pleurisy co-exists. Of course, in our day, we have in auscultation a perfectly certain means of ascertaining the co-existence of pleurisy with pneumonia; for the present, however, I must omit the consideration of whether this co-existence is under any circum-

[1] Cullen's First Lines; author's preface. Edinburgh, 1829.

[2] Report of Vienna Hospital for 1860, 1861, and 1862: New Sydenham Society's Year-Book for 1864, p. 138.

stances a safe indication of the necessity for blood-letting. It is sufficient to point out now that the statements—and I may add the practice—of Gregory himself afford no warrant for the statement, that the existence of a small and soft pulse indicates a pneumonia less sthenic, less amenable to treatment by blood-letting, or more influenzal in character, than one characterized by a hard pulse. This assertion is merely one of those unsupported dogmatisms with which, in the course of this controversy, medical opinion has so often contradicted and stultified itself; and it may be of service to point out, in conclusion, one or two additional striking instances in which this has been the case.

When Dr Alison lent the support of his great name to the revived theory of a change in the constitution of disease as explanatory of an acknowledged change of treatment, he brought forward the opinions of several of his medical brethren which had been communicated to him in support of this theory. One of these, a man of large experience and long in practice, thus writes:—"Again the epidemic of influenza which happened about thirty years ago was of a most decided inflammatory type, many of the cases going on to phrenitis and pneumonia of a very intense nature, and requiring, as I thought, active blood-letting before the disease could be arrested." [1] Contrast this statement with the following remarks by one of the ablest and most accurate observers that ever lived,—Dr Graves:— "The wide-spreading epidemic influenza, which lately visited the whole of Europe, including the British Isles, was not only truly remarkable, both for the violence of the feverish symptoms and of the local congestions of the chest and heart which accompanied its attack, but likewise for the unexpected relation it was found to bear to all measures of active depletion. I appeal to the profession for their testimony in this matter,—I ask whether all our preconceived opinions as to the à priori indications for venesection, leeching, and purging, were not found to be contradicted by the effects of these remedies in the epidemic influenza of 1833? The sudden manner in which the disease came on, the great heat of skin, acceleration of the pulse, and the intolerable violence of the headache, together with the oppression of the chest, cough, and wheezing, all encouraged to the employment of the most active modes of depletion; and yet the result was but little answerable to our expectations, for these means were found to induce an awful prostration of strength, with little or no alleviation of the symptoms. In some who were thus treated, recovery was protracted and doubtful, and the strength was not restored for several months." [2]    Dr Graves makes this statement in

[1] Edinburgh Medical Journal, May 1857, p. 974.

[2] Clin. Med., Dublin 1843, p. 500. It may be well to mention here that there are good reasons for believing that any apparent change of type in epidemic febrile diseases, particularly in those which are usually comprehensively termed continued fevers, have been in reality a change in the disease.— Vide, Murchison on the Continued Fevers of Great Britain, Lond. 1862. While what

the course of an interesting and able argument in favour of there
being such a thing as a change in the constitution of disease.   The
contrasting views just given of one and the same epidemic afford
a very striking proof of how much this idea of a change of type is
based upon mere opinion ; and this is still more remarkably illus-
trated by the following quotations.   In a series of very valuable papers
upon Fevers and Inflammations, published in the Edinburgh Medical
Journal, during the year 1858, Dr Christison states :—" My own
earliest acquaintance with measles, during the summer of the same
year "—1817 or 1818—" was quite different.   Both then, and for
many year afterwards, it put on the same sthenic form—evinced by
a frequent, strong, incompressible pulse, great heat of skin, and per-
fect tolerance of blood-letting and other antiphlogistic remedies—
which characterized our continued fevers of that period." [1]   While Dr
Alison has stated that, " The inflammatory epidemic of continued
fever observed from 1815 to 1818 was co-existent with a more
typhoid form of measles than we have ever seen since ; the erup-
tion in the bad cases of this epidemic having been late, livid,
partial, rapidly fading, and attended and followed by stupor, retch-
ing, dry tongue, etc.,—such cases being easily depressed by evacua-
tion, and obviously benefited by stimulants, and, when the dyspnœa
was not very urgent, very often recovering under their use." [2]

These two contradictory statements can only be regarded as both
correct by supposing that both observers had generalized from too
limited data.   In any case, they afford a warning that medical
opinion to be really of value must not be based solely upon dog-
matic assertion, nor upon anything so unstable as medical statistics,
but must involve a due and rational consideration of all the elements
which go to constitute its truth.   To the student of medical history,
I cannot point to a more noble example of a mind, by a due consid-
eration of these elements, gradually freeing itself from the trammels
imposed on it by early education and lifelong habit, than is to be

has been termed a change of type in eruptive fevers has been in reality but a
more or less intensive development of the disease, which thereby becomes
more or less intolerant of unnecessary interference.   Further, Dr Alison, when
commenting on Sydenham's remarks as to the " *occulta acris diathesis* " and the
" *inexplicabilis temporum ratio*," says :—" It is perhaps doubtful whether Syden-
ham's observations applied only to what at this day would be called epidemic
diseases, especially varieties of continued fevers, or whether they applied also
to some cases at least of sporadic, or what he called stationary diseases, such
as pneumonia.   In the latter case only, are his observations to be regarded as
sim'lar to those now under discussion ; the varying types and varying modes of
fatal termination of epidemics, both of the contagious and the malarious
diseases,—and also of those which, as I believe, partake at times of the
qualities of both,—are generally admitted ; and, I presume, have been wit-
nessed by us all."—*Ed. Med. Jour.*, May 1857, p. 975.   Dr Alison thus sets
aside as irrelevant all arguments drawn from the supposed variations in the
types of epidemic diseases, when applied in favour of a change of type in
pneumonia.— *Vide* also Ed. Med. Jour., Oct. 1857, p. 294.

[1] *Op. cit.*, July 1858, p. 42.
[2] Edinburgh Medical Journal, Aug. 1850, p. 165.

found in the later clinical lectures of Dr Alison. At first, of course, like almost every one else, he entirely disbelieved in the successful treatment of inflammatory diseases by the expectant treatment. Then, in 1850, we find him saying :—"We must admit, I think, that this practice has appeared, on fair inquiry, to be more frequently successful in inflammatory diseases, than could have been expected —*i.e.*, the practitioners who have thought themselves justified, by that theory (homœopathy) in trusting more than we do to the provisions of nature, aided only by regimen, for the cure of such diseases, have had fewer deaths and better recoveries than we should have expected ; "[1] adding, in regard to a statement made by those practising expectantly, which he says is " certainly more staggering," viz., that under their plan of treatment the convalescence from inflammatory diseases is more rapid than under the depleting plan : " What we apprehend from a pneumonia, met by what we believe to be inadequate treatment, is not pure exhaustion of the vital power, but such an amount of inflammatory effusion as may either cause rapid death by asphyxia, or lay the foundation of chronic and incurable organic lesion of the lungs, the precise nature of which may vary according to the constitutions of the patients."[2] But in 1852, two years later, he says :—"The part of the statements of those witnessing such practice "—the cure of inflammatory disease by homœopathy—" which I was most inclined to distrust was the assertion, that the convalescence of the patients thus treated was usually more rapid than that of patients with inflammatory complaints treated by fuller evacuation. But on watching the progress of cases of the kind, . . . I have been satisfied that the observation is correct. The absorption of the inflammatory effusion in such cases, even when very extensive, . . . having often been effected with remarkable rapidity, and the subsequent rapid recovery of strength having indicated that the blood, although it must have undergone a change in the course of the inflammation, had quickly recovered its natural properties."[3] We see, then, that careful observation had gradually convinced Dr Alison of two most important points : 1st, that blood-letting is not absolutely necessary for the cure of acute inflammatory disease ; and, 2d, that it is not necessary either for its rapid[4] or its perfect cure. So decided a change in the opinion of a conscientious medical practitioner, who for a lifetime had held opposite views, coupled with the fact that there is nothing in the past history of medicine which tends to show that inflammations have ever varied in their constitution,—though they have indubitably in all times, and more or less in every case, varied

[1] Edinburgh Medical Journal, Aug. 1850, p. 162.
[2] *Loc. cit.*, p. 163.
[3] Edinburgh Medical Journal, Aug. 1852, p. 165.
[4] Cullen, in MS. Lectures, already referred to, distinctly states that blood-letting retards convalescence, and renders the patient extremely sensitive to cold and liable to relapses.

in degree,—would seem to justify me in concluding that, had Dr Alison been longer spared, he would ultimately have freed himself from this latest of medical delusions,—the idea of a change of type in the constitution of inflammations ;—a change of type, which, if it be necessary to explain the success of a non-evacuant system of treatment in inflammation, ought by a parity of reasoning to be necessary to explain the success of a similar treatment in cases of catarrh, of parturition, and of accident by precipitation, drowning, etc., in none of which is the formerly inevitable lancet now employed. The argument is as reasonable in the one case as in the other, and amounts to a *reductio ad absurdum* in both.

ARTICLE III.—*Notes on the Recent Epidemic in St Petersburg.* By JOHN MILLAR, M.D.

MUCH apprehension having arisen concerning the recent Russian epidemic in the minds of the general public, as well as interest and curiosity in the medical profession at large, it may be of some utility shortly to detail a few personal observations of the disease made during a recent visit to St Petersburg. In order to estimate rightly the extent as well as the strength of the epidemic, it may be as well, in the first place, before taking into consideration its purely medical features, to make a few remarks on the causes of its origin and spread in the Russian capital. Any acquaintance with the mode of living of the poorer classes in Russia, as well as the wretched hovels they inhabit, would incline one to wonder that epidemics are not more frequent, for certainly it would be difficult to find a more suitable locale for the origin and spread of infectious diseases than the quarters of the poorer classes in St Petersburg. During last winter, however, certain other causes were at work to raise the ordinary mortality, and produce an epidemic of considerable magnitude.

It is the custom during the winter months for a large proportion of the agricultural population, who are at that time deprived of their usual means of subsistence, to repair to the capital in search of employment; but last winter, owing to the recent ukase liberating the serfs, great numbers of them also went to St Petersburg for the same object, viz., getting work to do. It will therefore be easily understood how, with the limited and miserable accommodation, hardly sufficient for the ordinary wants of the usual fixed population, overcrowding at once occurred, rendering the hovels worse than ever, and exposing their inmates, many of them exhausted from the effects of a long pedestrian journey, to all the vicissitudes of a severe Russian winter. Consequent upon this, food became dear in proportion, and more especially owing to a recent regulation by which Government ordered the slaughtering of animals to take place at a

found by post-mortem examination, to a peculiar affection of the dura mater. To Dr Kremiansky of St Petersburg we are indebted for the following particulars of the "sectio cadaveris." He found the internal surface of the dura mater, both at the top and sides of the hemispheres of the brain, covered with a thin layer of yellowish deposit, over which were spread light reddish spots, more or less closely grouped together, varying in size from a poppy seed to that of a millet seed, pea, or even larger. When the surface of the dura mater was washed, there remained on its under surface, swimming in the water, a sort of fringe, which consisted of tender villous dendroid reddish excrescences, evidently capillaries produced on the under or vascular layer of the dura mater. Sometimes it was found that also the internal surface of the dura mater presented a turbidness and succulence which gave, on section, a thin turbid serous fluid. This fluid was found to contain blood corpuscles and epithelial cells that had undergone fatty degeneration. It was also observed that considerable hæmorrhage often arose from the ruptured vessels of these tender newly-formed textures, sometimes in considerable quantity, so that the internal surface of the dura mater was lined with a thick layer of coagulated blood, together with the other newly-formed textures. But it was found that, when no hæmorrhage occurred, a thin serous exudation took its place, which produced similar compression of the brain. Dr Kremiansky's opinion of these lesions is, that they are produced in the commencement or acute form of the hæmorrhagic inflammation of the dura mater. So obscure are the symptoms of this affection during life, that they may be confounded with other cerebral lesions, so that the diagnosis can alone be confirmed by post-mortem examination, though Lancelot (an authority quoted by Dr Kremiansky) affirms that they can be ascertained symptomatically during life; yet his own investigations show the contrary. According to Dr Kremiansky, this affection was almost entirely confined to the cases of typhus and relapsing fever that came under his notice; and from observations he was led to believe that it was connected with poisoning by intoxication, and that its epidemic character was due to the abuse of spirits among the mass of the population. Although the symptoms associated with those post-mortem appearances have been described as obscure, yet they were sufficiently prominent to mark out the relative position and locality of the morbid processes that gave rise to them. Accompanied with the usual symptoms of feverishness, such as furred tongue, accelerated pulse, etc., the patient complained of severe pain in the head, principally towards the occipital region, though not confined thereto, of a character continuous and acute. This induced a condition of extreme restlessness as well of absence of sleep, which in a very short time—from one to two or three days,—was followed by paroxysms of delirium and convulsions. Although most authors describe the delirium associated with inflammation of the lining membranes

of the brain to be violent and continued, yet in these cases the delirium was more subdued, neither violent nor continued, and passing generally into a state of coma in which the patient died. As to the convulsions, these in the majority of cases were severe, generally preceded by sickness, vomiting, and headache, sometimes long continued and in others short, but returning at frequent intervals. The emaciation which also accompanied this form, as well as in all the typhus cases generally, was very well marked, though as a particular symptom it hardly deserves notice, being general in all the different types of disease which formed the epidemic. In connexion with this hæmorrhagic inflammation of the dura mater, many cases appeared of cerebro-spinal meningitis of an aggravated and severe character. After the usual premonitory symptoms, the patient became prostrated by paroxysms of tetanic convulsions, which returned one after the other in increasing severity, until the patient sank into a comatose condition—the precursor of death. Consistent with the severity of the disease, its rapidity was great also, many being carried off on the second and third days, thus leaving little opportunity for the effects of remedies to have any influence on the course of the disease. This affection was confined, fortunately, within a small radius, having been principally observed on the borders of the Vistula and south-western provinces of Russia, so that in St Petersburg and the northern Russian provinces it was scarcely noticed at all. Although, as far as has come under my observation, no remarks on the post-mortem appearances seen in those cases have been made public, yet it may not be considered irrelevant to infer, that the same character of inflammation described by Dr Kremiansky as occurring in the cases he has described, of hæmorrhagic inflammation of the dura mater, may have spread downwards to the enveloping membranes of the spinal cord, producing similar post-mortem appearances, and from the same causes giving rise to its epidemic character. To what, however, its local character is due is somewhat difficult to determine : some supposing this to be produced by causes having their origin in the damp and otherwise unhealthy character of the country where this affection was particularly manifested, and others believing it to be produced by a combination of many causes in which the poor and famished condition of those affected, together with the severity of the weather, occupied a prominent place. Another question difficult of solution is the exact amount of per-centage of cases in which the meningitis hæmorrhagica interna described by Dr Kremiansky was present ; for though an approximate analysis is given by him of the cases he made observations on in the first Military Hospital in St Petersburg, yet as these refer only to one of the many hospitals in that large city, no definite conclusions can be deduced from it alone. Still, taking into consideration all the features of the epidemic, we are led to conclude that they must have borne a large percentage to the sum total.

Having shortly referred to the typhus cases in the epidemic, in the same manner let us consider those which fall more correctly under the term typhoid. That in an epidemic of such a character these should be common is not to be wondered at, more especially when one considers the nature of the causes which were at work in its production, so similar to those producing typhoid fever in this country. In this class of cases we find the symptoms more closely associated with disease of the alimentary organs and viscera of the abdomen. After the usual preliminary symptoms of shivering, headache, furred tongue, and nausea with vomiting, as well as great loss of strength, the abdomen became distended and was the seat of severe pain whenever pressure was made on it. Diarrhœa of the pathognomonic pea-soup character then set in, accompanied often in its latter stages with discharge of blood, in consequence of the ulcerations becoming larger in extent and encroaching on some of the mesenteric vessels. As, however, the disease advanced, the symptom of pain on pressure more or less disappeared, the diarrhœa increasing, so that the patient gradually sunk and succumbed to the energy of the disease. The pathological appearances seen on post-mortem examination require little description here, as they entirely corresponded with those observed in typhoid cases at home. One interesting feature, however, presented itself, which tends to shed a light upon a still somewhat obscure branch of medical science,—namely, the relation subsisting between various forms of disease; for in this late epidemic, although no cholera or plague as such existed, yet some spurious cases presented themselves in which symptoms of cholera were observed mixed with those of typhoid fever. In one especial case, to which my attention was directed by the "chef" of the Abukowsky Hospital in St Petersburg, the sympathetic system of nerves seemed to be involved, producing such changes in the temperature of the skin as when section of the cutaneous branches of the sympathetic has taken place. These cases were always fatal, and formed a curious link between the class of fevers proper and that of cholera, associated as it is with plague and such epidemics of extreme virulence. If, as has been asserted, the poison in these two classes of disease is the same, only differing in its effects, as influenced by its strength, amount of dilution, and the sanitary condition of those whom it attacks, then may not such cases as these serve to show, though indistinctly as yet, their relation to one another; so that, being able to reduce them all to the same family, they will lose their terror and become amenable to the same rational form of treatment. Another feature of the typhoid cases, on which much stress was laid in certain quarters, was the presence of boils and buboes in some of the more aggravated cases. This resource of nature to get rid externally of the poison which had entered into the constitution was so far successful that, in the majority of those in which they occurred, a marked alleviation of the symptoms was the result, so that had strength and other

favourable circumstances been present there is no doubt that recovery in most would have occurred. As it is, many after a long and tedious convalescence did get round, whose circumstances were such as to insure fresh air, and a plentiful supply of nutritious food; whereas those who were debarred from such auxiliaries fell on the very threshold of returning health. In addition to those cases referred to, many of typhoid pneumonia occurred. Pneumonia is of itself, in St Petersburg, a disease exceedingly dangerous to life, from the excessive extremes of temperature the climate is exposed to, especially in the spring; for at that time, on the breaking up of the ice, the weather is so changeable as to render St Petersburg by no means a desirable residence for those who are at all subject to pulmonary complaints, so that all who are in that condition manage, if at all possible, to change their residence for a more salubrious and healthy climate. It is therefore not surprising that this affection, arising as it did among those not only exposed to all the inclemencies of the weather, but by privation and want reduced to a cachectic condition, should spread with a rapidity and fatality increased tenfold in the overcrowded habitations of the labouring population. In one case which came under my own especial notice the disease lasted for more than a month, but the majority were cut off by the end of the second week. The symptoms on auscultation presented the usual stages of pneumonia, the first and second being well marked; but as the third progressed and demanded the whole strength available to throw off the puriform matter, then the patient generally sank, being unable to do so. The throat also seemed to be the seat of disease, as much pain was felt in the larynx and trachea, giving rise to spasmodic fits of difficult respiration. When this occurred, death soon set in, being preceded by slight delirium and coma.

Lastly, we have to notice a peculiar feature in this epidemic,—the presence of cases of relapsing fever, analagous to those described under that name, as well as that of famine fever, etc., observed repeatedly in epidemics in Scotland and Ireland, where they were first noticed and described. The symptoms varied little from the description given by observers of former epidemics, so that there is no necessity for here going minutely into them. As to the relapses, they rarely exceeded two in number, the mortality being highest with the first, but diminishing with the second and third when it did take place. Jaundice was a prominent symptom of the more serious cases, many of which proved fatal, but from the recovery of others showing that it is not necessarily so. No doubt many in these cases perished from the want of proper precautions and remedies being used when the relapses occurred; for, having left the hospital, they suffered a relapse, without again re-entering and obtaining medical aid until this was too late to be of any benefit.

Such an imperfect sketch as this would be more so, without referring shortly to the means which were employed during the

epidemic for the restoration of health, and prevention of the spread of disease. In the former, medical aid and the use of sanitary measures combined, were necessary for the restoration to health of those affected; whereas, in the latter, sanitary conditions had alone to be attended to. These, it is but justice to the Russian Government to admit, were carried out liberally wherever they seemed to be needed, though to have done so adequately it would have been necessary to have provided better house accommodation in the poorer parts of the town, where the disease principally reigned. But for the sick this was promptly done by converting barracks into temporary hospitals, and increasing, as far as was consistent with proper ventilation and room, the accommodation of the already existing hospitals. Another precaution taken was the providing of fresh Neva water daily in large wooden troughs in those parts of the town distant from the Neva, that the inhabitants might use it, instead of the filthy water comprising the many canals which intersect St Petersburg. This was an exceedingly useful and beneficial measure, as the canal water is of itself almost sufficient to breed and spread fever, thus becoming the more dangerous where fever already from other causes existed. It was currently reported at one time in some English newspapers, that fires had been lit in some of the streets for the purpose of ventilation,—a report which much amused the English inhabitants of St Petersburg at the time, from its not having any foundation in reality. Medically, a rational course of treatment was on the whole adopted, remedies being more used for the purpose of alleviating symptoms than attempting to check at once the course of the disease. In the cases of relapsing fever, quinine was largely used, and with beneficial results, though many physicians were unwilling to try its effects in these cases. In the typhus cases, ipecachuan, as well as other diaphoretics, seemed the favourite remedies, in promoting the action of the skin, thereby tending to remove the morbid condition. But in connexion with the various preparations of the pharmacopœia, stimulants, combined with nutritious food, acted marvellously in promoting the return to health of these poor famished creatures, to whom bread of a good and wholesome character had been long wanting, so that it was no wonder that under an almost purely dietetic treatment they got round.

This epidemic was not confined altogether to St Petersburg; for, when in Moscow, I found, on inquiry, it had manifested itself there some time previous to my visit; consequently I had no opportunity of examining it. Westward towards the Prussian frontier also it extended, gradually diminishing as it went in fatality of effects, so that its appearance in this country need not be dreaded, provided attention to sanitary conditions and laws, as the only safeguard against the invasion of such a deadly foe, be duly made not only a matter of legislation but of personal as well as public importance.

ARTICLE IV.—*Climacteric Insanity in the Male.*    By FRANCIS SKAE,
M.D. Edin.

IN a paper which appeared in the February number of this Journal,
I endeavoured to show that the symptoms of insanity occurring in
women at the climacteric period are so uniform, characteristic, and
peculiar, as to render it easily recognisable, and entitle it to be
referred to a distinct natural group or family, which may be dis-
tinguished as "Climacteric Mania or Insanity."

I stated then that it had been remarked by Sir Henry Halford,[1]
and subsequently by Dr Conolly, that an analogous climacteric
change took place in the male between the ages of 50 and 60, and
that, at that time, there was not unfrequently developed a form of
insanity for which no cause, except disorders connected with this
change, could be assigned.    I stated then that this agreed with
my own observations and experience; and my object in this paper
is to describe the history, results, and treatment of a considerable
number of cases of climacteric insanity in the male, treated in the
Royal Edinburgh Asylum, with a view to pointing out the distin-
guishing characteristics of this form of insanity, the results to
which it tends, and the treatment which seems most likely to be
followed by success.

*Age.*—In women, the irregularities immediately preceding the
cessation of menstruation, and the disorder of the general health
which frequently follows cessation, mark the grand climacteric as
generally taking place in them between 45 and 50 years of age.
That an analogous change takes place in the male is very generally
admitted; but as this change is unmarked by the alteration or
cessation of any natural function, the age at which it takes place
cannot be definitely fixed with any degree of certainty.

Sir Henry Halford considers this change as generally occurring
in men between 50 and 60 years of age; and I find that all those
cases which came under my own observation as presenting the
characteristic group of symptoms peculiar to climacteric insanity in
the female were those of men between 48 and 60 years of age.    In
none of the cases I have selected could any cause be assigned for
the insanity; they all of them presented the same uniform type,
and it appears reasonable to conclude that their insanity resulted
from the same cause; in other words, that a climacteric change
takes place in the male between 48 and 60 years of age, analogous
to that which occurs in the female between 45 and 50, and that in
the male a form of insanity is sometimes developed at this period
identical with that which is met with in women at the analogous
period.

The following table shows the age at which the insanity was
developed in 60 cases :—

[1] Essays, page 4.

TABLE I.

| Age. | | | Number of Cases. | Age. | | | Number of Cases. |
|---|---|---|---|---|---|---|---|
| | | | | | Brought over | | 37 |
| 47 | . | . | 1 | 56 | . | . | 4 |
| 48 | . | . | 2 | 57 | . | . | 6 |
| 49 | . | . | 3 | 58 | . | . | 3 |
| 50 | . | . | 8 | 59 | . | . | 0 |
| 51 | . | . | 4 | 60 | . | . | 5 |
| 52 | . | . | 1 | 61 | . | . | 3 |
| 53 | . | . | 3 | 62 | . | . | 1 |
| 54 | . | . | 3 | 63 | . | . | 0 |
| 55 | . | . | 12 | 64 | . | . | 1 |
| | | Carry over | 37 | | | Total | 60 |

From this it is seen that the greatest number of cases occurred at 55 years of age. Of the 60, 45 were married, 8 single, and in 7 information as to their domestic condition was not supplied.

Hereditary predisposition existed in 10 of the above cases, in which 10 cases the climacteric change must be regarded as the exciting cause. In none of these cases had the ordinary moral or physical causes, to the influence of which they had been more or less necessarily exposed before they were 48 years of age, induced insanity; none of them had laboured under previous attacks of insanity. They had passed with safety the age at which insanity is most frequently developed, and their history thus tends to show the very considerable and comparatively powerful influence of the climacteric change as an exciting cause of insanity in a constitution hereditarily predisposed to that disease. But, apart from these cases hereditarily predisposed, there can be little doubt, I think, that the climacteric change in man exercises a very powerful influence as a cause of insanity; and this view is strengthened by the fact, that a considerable proportion of the cases met with at that time of life are those of men who have been eminently successful in business or in their profession, who have secured to themselves comfort if not affluence, who are happy in their domestic relations, and who may reasonably be supposed to be relieved from the harassing influence of protracted mental exertion, domestic unhappiness, or pecuniary embarrassment. And yet men so circumstanced are not unfrequently seen to be oppressed by gloomy despondency, viewing the past with bitter remorse, the present with impatient discontent, and the future with exaggerated and delusional anxiety, — they become morbidly irritable and suspicious, give way to causeless terror, believe themselves guilty of crimes for which they are to be everlastingly damned, and afford unmistakable proof of their insanity in their conversation, conduct, and appearance; which insanity, I venture to submit, *is the result of the effect upon the brain of a constitutional disturbance accompanying a great climacteric change.* In men influenced in this manner by the climacteric change there is frequently developed an inordi-

nate craving for stimulants in those who have been habitually temperate in their habits, and this intemperance, frequently amounting to confirmed dipsomania, is often the assigned cause of their insanity. I have excluded such cases from consideration in this paper lest their insanity *might* have been the direct result of intemperance; but I am convinced that, in a large number of such cases, the craving for, and undue indulgence in, stimulants was the effect and not the cause of the disease,—that the patients suffering from deep depression, morbid fears and anxieties, sought relief in artificial stimulants, and that the constant repetition of this habit served to aggravate the disease which had given rise to the habit, the habit itself being merely a symptom of the disease. A very considerable number of the cases admitted into an asylum between 50 and 60 years of age, having intemperance assigned as the cause of their insanity, will be found, on examination of their history, to be those of men who, having been long sober and industrious in their habits, gave way to drinking *after* having been for some time subject to fits of low spirits, for which no cause could be assigned.

*Symptoms.*—The first observable symptoms of this disease are, frequent fits of depression of spirits, gradually becoming more permanent. Alternating with these fits of depression, there are periods during which the patient is restless, morbidly sensitive, and extremely irritable. As the depression passes into confirmed melancholia, it is associated with suspicion of others, fear of impending but undefined evil, fear of the soul's loss, refusal of food, and not uncommonly persistent delusions, hallucinations of the senses, and determined suicidal tendency. In some cases there are transient paroxysms of excitement, more or less maniacal, during which the patient is dangerous to others, but distinct homicidal impulse is rarely met with; while monomania of pride and delusions of exaltation are still more uncommonly seen. It is somewhat curious to remark, that in this form of insanity the majority of the patients more or less readily admit that they are insane; they rarely if ever complain of being considered insane, and placed in an asylum; on the contrary, many of them express a sense of security at being placed in an asylum, and a fear of being left to themselves not unfrequently induces such patients to place themselves under treatment; and it is by no means rare to hear such patients remonstrate with the physician on being told that they are well enough to go home, and say " that, although they feel well enough in the asylum, they are afraid to *trust themselves at home in case something should befall them.*"

The foregoing mental symptoms occurring *as a group* are, I think, perfectly characteristic of climacteric insanity. They are identical with those seen in climacteric insanity in the female, and they are met with as a group only in that form of insanity which occurs at the climacteric period in the male or female.

There are two forms of insanity occurring in men between 40

and 60 years of age, which may be confounded with climacteric mania properly so called.   The one is that form of insanity which is produced by long-continued intemperance; the other is premature senile dementia, the result either of atheroma or of fatty degeneration of the vessels of the pia mater.

The first, or that produced by long-continued intemperance, is distinguished,—1st, by its history; 2d, by the character of the delusions with which it is accompanied, which are much of the same character as those seen in delirium tremens,—such as fear of robbers, of assassination, of dogs, rats, or snakes; the fear being of *well-defined evil*, and not (as is invariably the case in climacteric mania) of *undefined evil*, of some impending doom, or of eternal loss; 3dly, by the unconquerable aversion to treatment, control, or moral restraint, and by the inordinate craving for stimulants, and a determination to gratify that craving on the first opportunity; which are more or less manifested in all cases resulting from intemperance, and which are never seen at all in purely climacteric cases. The second, premature senile dementia, is distinguished from climacteric insanity,—1st, by the progressive impairment of the mental faculties, more especially by the marked loss of memory, which is never seen in climacteric insanity; 2d, by the absence of the delusions before described (fear of undefined evil, soul's loss, etc.), by the absence of dangerous propensities, and of suicidal tendency; 3d, by the presence of the arcus senilis; 4th, by the hardness and tortuosity of the arteries.

*Bodily Condition.*—In the male, as in the female, this form of insanity is most frequently of an asthenic type; the bodily condition is generally weak, not unfrequently emaciated; the digestive functions very much impaired, and the bowels sluggish if not obstinately confined; the pulse is weak, easily compressible; the face pale, haggard, and bloodless; the tongue dark and furred; and the breath exceedingly offensive.

*Mental Symptoms.* — The subjoined table shows the relative frequency of the mental symptoms in 60 cases :—

TABLE II.

| | |
|---|---|
| Melancholic, | 52 |
| Suspicious, | 23 |
| Fear of undefined evil, | 45 |
| Fear of soul's loss, | 18 |
| Excited, | 23 |
| Suicidal, | 27 |
| Refusing food, | 18 |
| Dangerous to others, | 4 |
| Homicidal, | 1 |
| Exalted delusions, | 3 |
| Demonomania, | 4 |
| Hallucinations of hearing only, | 3 |
| ...... of vision only, | ... |
| ...... of both these senses, | 4 |
| ...... of smell, | ... |

*Melancholia.*—It is seen from this that melancholia existed in nearly all of the 60 cases. This melancholia, which is generally the first symptom manifested, shows itself in the same manner and is of the same type as that which is so characteristic of climacteric insanity in the female. It is first seen in the form of transient fits of depression of spirits, accompanied by a desire for solitude, a dislike to undertake any exertion mental or bodily, and a morbid sensitiveness and irritability quite at variance with the patient's natural disposition. The morbid sensitiveness and irritability shows itself in the patient's magnifying the most trifling annoyances, and resenting the want of sympathy in all his fancied troubles as a positive neglect. These transient fits of low spirits are most common in the morning, passing off towards night. It is at this stage that that craving for and undue indulgence in stimulants is frequently manifested to which I before alluded, and which is often stated to be the cause of the insanity, but which I believe is more frequently its result. As the disease progresses, the melancholia becomes either fixed and permanent, or alternates with periods during which the patient is restless and excited, watchful of the conduct of all about him, and very frequently the next symptom I notice is seen.

*Suspicion of Others.*—It is seen in Table II., that in 23 of the 60 cases, or more than one-third, this symptom was met with. It would seem from this to be more generally present in this form of disease in men than in women, as, in the 200 female cases before referred to, it was met with in 63 only, or rather less than one-third. The first evidence of the presence of this symptom is the patient becoming watchful of the most trifling actions of the other members of his family. As the disease progresses and the suspicion becomes a fixed delusion, the patient attributes the most artful and evil motives to the most harmless and innocent actions; he becomes more and more reserved, surrounding all his own doings with great mystery, and viewing all the doings of others with deep distrust. As in the female, so also in the male, the suspicion is almost invariably of the patient's nearest relatives. He suspects them of conspiring to obtain possession of his money, or of an intention to poison him. This last suspicion generally leads to refusal of food, afterwards to be noticed. Suspicion sometimes gives rise to paroxysms of excitement, during which the patient may threaten or attempt violence against those whom he suspects. Long-continued and deeply-rooted suspicion of the patient's nearest relatives not unfrequently leads him to meditate and attempt suicide, under the impression that he is viewed as an encumbrance, and that his death is longed for by his family. I may here remark, that the well-intentioned but clumsy and exaggerated attempts of the patient's friends to remove this suspicion only serve to increase it. The suspicion of climacteric insanity is peculiar, in being almost entirely confined to the patient's relatives, most commonly his wife or children, and

rarely if ever being directed—as in senile dementia, insanity caused by sunstroke, etc.—against perfect strangers, medical attendants, or servants.

Closely associated with the two foregoing symptoms, and strikingly characteristic of and peculiar to climacteric insanity, both in the male and female, is *fear of undefined evil*. This symptom was present in 45 or three-fourths of the 60 cases. This fear of some impending unknown evil gives rise to a very peculiar expression of terror in the patient's face, accompanied by a shrinking startled manner, with great sleeplessness and restlessness. Although it would appear to be more common among men, the terror is not in them of such an intense kind as is seen in women. In men this fear seems to add very much to the general gloom and despondency; but it does not, as in women, take such complete possession of them as to make them shriek loudly for help, or crouch trembling in a corner, moaning, and wringing their hands. One of the most common and characteristic evidences of this fear is the use of such expressions as, " When are ye going to do it?" " When will it be?" " I wish it was over." I have frequently asked patients to describe this fear, or rather to say *what* they were afraid of, and they appeared quite unable to do so, further than to say that there was " something awful hanging over them," or " they felt some terrible thing was going to come upon them." Some, however, described this fear as the dread of some unknown but terrible form of death they sooner or later must die.

The dread of this unknown form of death was such that it induced *three* patients who were continually oppressed by it to attempt suicide, and yet none of them could describe the form or mode of death the fear of which was so terrible. Fear of undefined evil is often accompained by, but is quite distinct from, the next symptom I notice, viz. :—

*Fear of the Soul's Loss.*—This was present in 18 or less than one-third of the 60 cases. As in women, so also in men, this delusion is generally the result of an overpowering conviction in the patient's mind of his own unworthiness, in consequence of his many grievous sins. In addition to this, most of the above cases expressed the distinct delusion, that they had been guilty of " the unpardonable sin," and were on this account eternally lost. The delusion of the unpardonable sin would appear to be more common in men than in women. It may appear somewhat strange, yet it is nevertheless undoubtedly the case, that suicidal tendency is most determined and persistent in those patients who are constantly expressing the above delusion. This delusion is exceedingly difficult to combat; indeed it would seem best not to attempt to do so at all by arguing on the subject with the patient, but to trust to its disappearing with the other symptoms of the disease on the re-establishment of the general health; because it is very frequently seen that the patient, in arguing on this subject with his friends, has had his belief in the

delusion strengthened by the very anxiety which they show and the strenuous efforts which they make to convince him of its fallacy.

*Excited.*—In 23 of the 60 cases there were attacks of excitement, more or less prolonged, but in none of these cases did the excitement amount to acute mania. It would be better described perhaps as a very aggravated restlessness, and inability to sleep or even to lie still for any length of time. Excitement would appear to be less frequently seen and is certainly less maniacal in this form of insanity in men than in women. This excitement, when caused by deeply-rooted suspicion, is sometimes accompanied by threats of and attempts at violence, rarely by suicidal impulse; when, as is more frequently the case, it is the result of intense fear of unknown evil, the patient is rarely threatening or violent, but is generally, on the other hand, very suicidal.

*Suicidal.*—Of the 60 cases, 23, or nearly one-half, had threatened or attempted to commit suicide. It will be seen that the proportion of suicidal cases is considerably larger, in this form of insanity, in the male than in the female (see previous paper), which is in accordance with the generally-entertained idea that suicidal tendency is more common among men than women. I am, however, inclined to think, from what I have seen of suicidal tendency, not only in climacteric insanity, but in other forms of insanity where it is met with, that in women the suicidal determination is more persistent than in men, more carefully concealed, and the means used to effect their purpose more outré, and such as are rarely if ever employed by men. Thus, a man tries to destroy himself by blowing his brains out, or by poison, by cutting his throat, or by hanging, drowning, or precipitation, and watches his opportunity to effect his purpose by one of those means, no other method occurring to him as practicable; whereas a woman, failing any of the above, will swallow reels of cotton, pieces of glass, pebbles, etc., or thrust needles into her breast, in hopes of penetrating the heart, deliberately set fire to her clothes, and endeavour to prevent any one extinguishing the flames, utterly regardless of the pain they cause, or (as quoted in previous paper) with a grotesque ingenuity strikingly feminine, fill her shoes with water and sit in wet feet, knowing she was getting calomel from the doctor, and believing that under these circumstances a cold would prove fatal.

I would further observe, with regard to suicidal tendency, that it is more commonly manifested in this form of insanity than in any other, with the exception of puerperal insanity alone; that it is almost invariably associated with melancholia, fear of some terrible undefined evil, and fear of the soul's loss, and is most determined and persistent in those who express the last-named delusion; that it is sometimes dependent on demonomania alone, and that at other times it is seen in connexion with hallucinations of hearing, without melancholia or the above delusions. It is hardly necessary to add, that suicidal tendency is a symptom demanding the most careful

watching, inasmuch as it frequently exists, carefully masked and concealed, long after the delusions with which it was accompanied have ceased to be expressed, although undoubtedly still harboured by the patient.

*Refusal of Food.*—This was met with in 18 cases. In all those cases the refusal of food was determined and persistent, and the patients required to be fed by the stomach-pump. In several other cases the patients had refused all food previous to their admission; but as in these the refusal was the result of suspicion of poisoning, it was not persevered in after admission, the suspicion being entirely confined to the patient's relatives. Again, patients not unfrequently persevere in refusal of food till they see the stomach pump, when its somewhat formidable appearance induces them to forego their resolution. Refusal of food may be result of suicidal impulse, in which case it is generally very determined, and can rarely be overcome by advice or entreaty, and the patient requires to be fed; or it may be the result of distinct delusions, as, that "food turns to fire in his inside;" that "his bowels are full of holes;" or, what I have frequently heard expressed, that "his bowels are stopped up, and nothing can pass through them;" or it may be the result of fancied instigations or injunctions, as is seen in those who have hallucinations of the senses, or more rarely in *demonomaniacs;* lastly, it may arise, as before stated, from suspicion—in which case it is rarely very persistent. In this last case, where the patient refuses food from a suspicion of poison, it has been recommended that food should be left about, as if accidentally, in the patient's way, as it is known that patients who refuse food regularly set before them will often eat voraciously, when unobserved, what they can find in this way. But it may be doubted whether this is judicious; whether, on the contrary, it is not calculated to foster those suspicious delusions the removal of which is one of the principal objects to be attained. It may be doubted whether, if the patient were forcibly fed, he would not far sooner become convinced of the absurdity of his suspicions than if he were allowed to refuse his regular meals, and pick up such scraps as he thought were left accidentally, and consequently unpoisoned, lying about.

*Dangerous to Others.*—Of the 60 cases, four were described as dangerous to others, having both threatened and attempted violence. Three of the above were dangerous only while under the influence of excitement produced by suspicion, and in one case by demonomania; but the fourth was the case of a man who was never either excited or suspicious, who never expressed any delusions, or was subject to any hallucinations, but who was seized at various times with a homicidal impulse, which he himself described as almost irresistible, and which made his life so miserable that he frequently meditated suicide. His repeated declaration that he would kill either himself or somebody else led to his being placed in the Asylum; and as his case presents peculiar features, and possesses

some medico-legal interest, I may be pardoned entering somewhat into its details.

The case was that of a farmer from the most northern district in Scotland, 57 years of age. He had suffered from a previous attack when 47 years of age, brought on by drinking. During this attack he had been violent and excited, was placed under treatment, discharged recovered, and since that time had remained (for ten years) perfectly well and habitually temperate.

Fourteen days previous to his admission, he is stated to have become " unsettled in his manner, restless and sleepless ;" expressed himself as in " constant dread of doing some one an injury," and as feeling quite unable to restrain himself. He afterwards threatened to cut off his wife's head with an axe which he saw in the kitchen, and said, " he had the greatest difficulty in restraining the desire to murder somebody." In consequence of this he was sent to the Asylum by his son. He was quite willing to be placed under treatment. On admission he appeared very quiet and reserved, but was sullen in his manner of answering questions, expressed himself as wretched and miserable, and said two or three times, that " he was afraid he should do something awful." His appearance was very striking. He was 6 feet 4 inches in height, and of herculean proportions ; his face was haggard, and wore a desponding expression, occasionally momentarily relieved by a transient gleam in his eye, almost undescribable, which suggested the idea that he was then picturing in his imagination the perpetration of some violent and tragical deed. His bodily health was much disordered, the digestive functions considerably impaired, and the bowels obstinately constipated. Although taciturn and reserved, he was civil and obliging, and was much liked by the other patients, and by the attendants from the quiet unobtrusiveness of his manners. As his bodily health improved, the gloomy despondency he exhibited on admission gradually wore off, and he was induced to work in the gardens. I had frequent conversations with him regarding his homicidal impulse. At first he avoided the subject, but subsequently he spoke most freely of it, and described his feelings as follows :—He said, that " when at home he often felt very low and depressed in spirits ;" that frequently while, in these " low fits," he was seized with an " awful impulse to kill some one, any one in fact." This feeling came on him most strongly one day. He came into the house and found his wife alone in the kitchen ; she had been chopping wood, and the axe was lying near her. " When I saw the axe," he said, " I could hardly keep from splitting her head." He had no dislike to his wife. He afterwards threatened her life ; and he declared he very often had this feeling, *always when he saw a weapon.* I asked if he ever felt the impulse in the Asylum, and he said, that although he had the feeling sometimes, it was quite different ; that he never felt that he was unable to restrain himself. " I'm not afraid of myself here,—I

know I'm in a madhouse." But he would add, "I'll be always that way again when I go out."

After being some months in the Asylum, during which time he had been employed in the garden, and was induced to join actively in the amusements, his bodily health was quite restored; the deep depression from which he suffered had entirely disappeared, and he admitted that he was free from the terrible impulse above described. Under these circumstances, his son was anxious that he should go home; but he most strongly remonstrated against this, and said, that he was certain "something would happen;" that he would injure himself or somebody else. He also said he was sure "all the old feelings would come back on him." His discharge was made out shortly after this; and his son being exceedingly anxious to get him home, his passage was taken in the steamer. He left the Asylum accordingly with his son, but only went as far as the gates, where he turned, and positively refused to leave the place. He remained a month longer in the house, apparently quite well, and was again discharged. This time he appeared to have quite made up his mind to go, bidding the patients goodbye, promising to write, and so forth; but he returned the next morning, saying he felt he was getting dangerous, and losing his self-control. He remained several months longer in the Asylum, became cheerful, active, and appeared to take a great interest in the other patients. Although still unwilling to leave, he took a much more hopeful view of his own condition, and at his son's request was discharged as relieved and went home. I ascertained a fortnight ago (on the 12th April), that he has continued ever since, as he now is, perfectly well.

*Remarks.*—I consider this man as a climacteric case, because, although failing to exhibit some of the most characteristic peculiarities common to the majority of such cases, his insanity supervened at the climacteric age without any assignable cause, and was accompanied by such disorder of the general health as indicated a constitutional change, and because the history, symptoms, and result of his case bore a striking resemblance to those of many climacteric cases. I observe, with regard to his case, that it furnishes an instance of what is undoubtedly very rare, namely, homicidal impulse without any co-existent impairment of the intellectual faculties, without either delusions or hallucinations, and unaccompanied by any other symptom of insanity except profound depression. That this man would be considered insane, not only by medical men, but by those guided in their judgment by common sense alone, cannot be doubted; for it must be admitted that so morbid an impulse, unconnected with any motive whatever, is as strong a proof of unsoundness of mind as any one or more of the ordinary expressed delusions of the insane. In a medico-legal point of view, his case is interesting, as showing that a form of insanity undoubtedly exists under the influence, and as the result

of which a man may commit homicide, but which, as it is not recognised by law, affords no palliation of the crime, and does not prevent the deliberate infliction on the victim of disease of that punishment merited only by the criminal of perfectly sound mind, actuated by the basest motives which can disgrace humanity. There can be little doubt, that if the man whose case is here detailed had not been placed in an asylum, he would have killed some one. There can be even less doubt that, had he done so, in the existing state of the law he would have been hung; and there can be no doubt whatever, that such a vindication of the law would involve a gross violation of the principles of justice.

The remaining symptoms require only a brief notice.

In 3 only of the 60 cases were delusions of an exalted character met with; they differed from the delusions so common in general paralysis in being more transient, and in not being frequently or vociferously expressed.

*Demonomania.*—Of the 60 cases 4 were demonomaniacs; they were all suicidal, and one of them, as before stated, was dangerous.

*Hallucinations* of both vision and hearing existed in 4 cases. Of these, 2 recovered, 1 was removed unimproved, and 1 died.

Hallucinations of hearing alone were present in 3 cases, 2 of whom recovered, and 1 died.

*Results.*—I now consider the results of the 60 cases :—

TABLE III.

| Of 60, | . | . | . | 34 recovered. |
|---|---|---|---|---|
| ,, | . | . | . | 7 became demented. |
| ,, | . | . | . | 3 removed improved. |
| ,, | . | . | . | 7 died. |
| ,, | . | . | . | 9 removed unimproved. |
| | | | | 60 |

It is seen from this that recoveries took place in the ratio of 56·7 per cent. This is much higher than the average per-centage of recoveries from melancholia in general, which is stated by Huslane to be only 27 per cent., although Dr Tuke found it as high as 54·88 at the Retreat. The duration of the disease in those who recovered is as follows :—

TABLE IV.

| | | | | Number of Cases. |
|---|---|---|---|---|
| Under 1 month, | . | . | . | 1 |
| ,, 2 months, | . | . | . | 1 |
| ,, 4 ,, | . | . | . | 17 |
| ,, 6 ,, | . | . | . | 5 |
| ,, 8 ,, | . | . | . | 4 |
| ,, 1 year, | . | . | . | 1 |
| ,, 2 years, | . | . | . | 4 |
| Over 2 ,, | . | . | . | 1 |
| | | | | 34 |

It is seen from this that the duration of the insanity in the majority

of the recoveries was less than four months, an important point in favour of the curability of the disease.

The causes of death were as follows :—

| | | |
|---|---|---:|
| Apoplexy, | . . . . . | 1 |
| Dysentery, | . . . . . | 2 |
| Exhaustion from prolonged excitement, | . | 2 |
| Cardiac disease, | . . . . | 1 |
| Bright's disease, | . . . . | 1 |
| | | 7 |

This rate of mortality is so low as to demonstrate that, apart from suicide or organic disease, this form of insanity has little tendency to a fatal termination.

*Treatment.*—The means of treatment suggested as most likely to be successful in climacteric insanity in women are equally applicable to the treatment of the same disease in men.

They are, 1*st*, The immediate removal of the patient from home, and from the influence of the scenes and associations in connexion with which the disease was first developed; the separation from the society of relatives and friends, who, in their mistaken kindness, minister to a morbid craving for sympathy, or endeavour to combat delusions in a manner which, however well-intentioned, rarely if ever does any good, and very frequently has a most injurious effect.

2*d*, Placing the patient under a well-regulated system of moral discipline and control, by providing occupation (as especially work out of doors), amusement, and exercise at regular hours.

3*d*, Good nourishing diet, together in many cases with stimulants, is indispensable to the proper treatment of a form of disease so intimately associated with a weakened and frequently emaciated bodily condition.

4*th*, Gentle laxatives and tonics are in most cases required, owing to the torpidity of the bowels and the impaired activity of the digestive functions.

5*th*, Narcotics undoubtedly form one of the most efficient means of alleviating and curing this form of insanity. The cases most suitable for this method of treatment are those chiefly which are characterized by a general feebleness of the circulation, and a well-marked anæmic state of the system. In such cases, both of men and women, I have seen large doses, such as two drachms of the solution of the muriate of morphia, given nightly for a lengthened period, attended with the most beneficial effects, and without any disturbance of the general health. It is almost unnecessary to add, that the exhibition of such powerful doses must always be continued under strict supervision, and must be gradually diminished and carefully withdrawn before the patient is left to his own control.

In many cases, however, morphia does disagree with the patient,

and other narcotics must be substituted. Those which I have found of most service are,—the tincture of hyoscyamus, and the tincture of cannabis indica. These medicines require to be used in larger doses than in ordinary practice; but the requisite dose can only be arrived at by a gradual increase of the ordinary medicinal dose, up to that by means of which the desired object of natural repose is attained.

6th, The other indications of treatment consist in the removal of all local causes of irritation, and the re-establishment of the general health in accordance with the recognised code of therapeutics.

*Conclusions.*—From the foregoing description, I draw the following conclusions :—

1st, That there occurs in men between the ages of 48 and 60 a form of insanity, accompanied by more or less constitutional disturbances, which, in its symptoms, progress, and results, is identical with the insanity met with at the climacteric period in the female, and which may therefore with propriety be called climacteric insanity in the male.

2d, That the symptoms of this form of insanity are so characteristic as to render it easily recognisable.

3d, That this is the most curable form of insanity associated with melancholia which occurs in men, the recoveries being in the ratio of 56·7 per cent.

4th, That the duration of the insanity in curable cases rarely exceeds four months.

5th, That this form of insanity, apart from suicide or organic disease, rarely tends to a fatal termination.

6th, That the most important indications of treatment are,—early removal from associations and friends; careful watching; occupation, as especially out-of-door work; nutritious diet; and the judicious administration of narcotics.

ARTICLE V.—*Vertical Hemiplegia of the Palate in a Case of Diabetes Mellitus.* By WILLIAM R. SANDERS, M.D., F.R.C.P.E., Physician to the Royal Infirmary of Edinburgh, Lecturer on Physiology and on Clinical Medicine.

IN a paper on Paralysis of the Palate in Facial Palsy, published in the last number (for August) of this Journal, I drew attention to the peculiar form of the palatal hemiplegia which occurs in these cases. This form might be called the vertical or pendent hemiplegia of the palate. It consists in a vertical relaxation of the velum palati on the paralyzed side, in consequence of which the corresponding posterior palatine arch is lower and less curved than its fellow on the opposite side. The uvula is usually twisted, but its

direction is not constant, the point being turned sometimes to the paralyzed side, and sometimes to the sound side, so as to present little diagnostic importance.   There is no lateral deviation of the palate as has commonly been described.   I have now to record an example of the same vertical hemiplegia of the palate recently brought under my notice, which is more decided in some of its phenomena than the instances previously described, and which is remarkable as occurring in a case of diabetes mellitus, unconnected with facial palsy, or any other kind of paralysis.   It is the case to which I allude in the note at the conclusion of my paper above referred to (p. 154).

James J.,[1] aged 28, labourer at a limestone quarry, admitted to Ward IV. of Edinburgh Royal Infirmary on 24th November 1864, was transferred to my care in May 1865, affected with diabetes mellitus, which came on after gastric fever in August 1864.   Has passed as much as 486 oz. of urine in twenty-four hours; in May and June, the daily quantity of urine averaged 175 oz., of specific gravity 1040, highly saccharine.   In spite of his complaint he still exhibits a tolerably healthy appearance.   The only nervous symptoms present are langour and feeling of weakness, especially in the right leg.   When he walks far he loses power in the right foot. He suffers sometimes from cramps in the legs.   He sleeps badly; he used to suffer from headache, and although usually cheerful, is subject to occasional depression of spirits.   On the 29th June, he had an attack of neuralgic pain behind the left internal malleolus, which was removed by the subcutaneous injection of the bimeconate of morphia.   No facial nor other paralysis.

*Palate.*—While in a state of rest, as delineated in the first woodcut, the vertical relaxation or pendent condition of the left half of
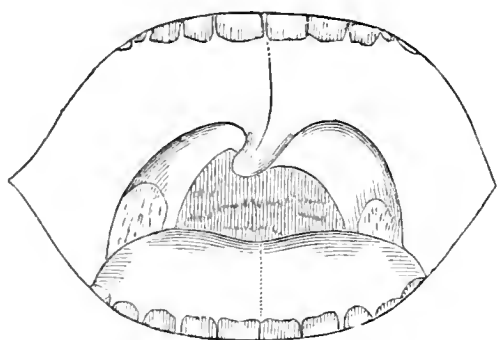


the velum palati is observed.   The left posterior palatine arch is depressed, and is less curved than the right; the raphé of the velum, however, is straight, and in the mesial plane (not drawn to either side); the uvula is oblique, its point turned to the right or unparalyzed side.

[1] From report of the case, by Alexander Drummond, Esq., Clinical Clerk.

But when the palate is drawn upwards, by the action of the levator palati, the irregularity of the palate is greatly increased, and the distortion represented in the second wood-cut is produced.
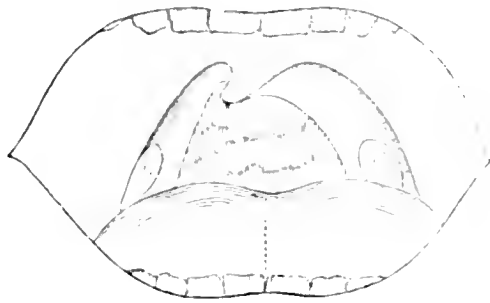


Here, it will be observed, the velum is drawn diagonally upwards, and to the right side, and both palatine arches are pulled upwards and across, as it were, towards the right ear. In this position the right posterior pillar of the palate becomes almost vertical, and the uvula and raphé are bent, and dragged upwards to the right side. In consequence, the right palatine arch is narrowed to a mere slit, or is indeed quite obliterated by the uvula becoming applied to the right side of the velum, between its anterior and posterior pillars. At the same time the left half of the velum is drawn across the pharynx, the left posterior pillar of the palate forming a low, wide arch, which extends from the lower left to the upper right side of the fauces. The velum seems to be scarcely at all raised on this side.

This condition is well shown in the wood-cut; but, as I have just mentioned, the distortion is sometimes even greater than is there represented, the uvula being so completely dragged upwards and across, as to be laid against the right pillars of the velum.

This diagonal lateral distortion occurs whenever the palate is elevated, whether voluntarily by a long inspiration, or reflexly by mechanical irritation of the velum and fauces. In the latter case the muscular action is more powerful, and the distortion more complete and permanent, lasting for a few seconds.

There can be no doubt that this irregularity and distortion is caused by paralysis nearly complete of the left levator palati, due, doubtless, to paralysis of its motor nerve. No other muscle of the palate seems to be affected. The right levator palati acts powerfully, being the efficient agent in producing the diagonal distortion. The circumflexi or tensors are normal, for the velum is well and evenly stretched, and feels equally tense on both sides; and the palato-glossi and palato-pharyngei contract freely and strongly when the fauces are irritated.

No symptoms whatever, neither nasal intonation nor difficulty of swallowing, were produced by this condition of the palate.

It is curious to inquire, why in this case there is no lateral deviation of the palate in a state of rest, when such marked oblique distortion occurs during muscular action? The explanation doubtless is, that, during rest, the circumflexi, palato-glossi, and palato-pharyngei, which are unaffected, are sufficient to maintain the balance on opposite sides, and keep the raphé in the middle line. But, in the upward movement of the velum, this equilibrium is overcome by the increased force of the sound levator palati during contraction.

It seems probable that the appearances just described may have given rise to the common, and, as I believe, erroneous account given in books, of a lateral displacement of the palate, when affected in facial palsy. But it is to be noted that the oblique lateral distortion, represented above, does not occur during rest; that its direction is diagonal, upwards and sideways, not simply lateral and horizontal; and that it is attended by the pendent condition of the velum and depression of the palatine arch, which are not mentioned by authors. Indeed, lateral distortion with a pendent velum has never hitherto been described. It is therefore more likely that the position of the uvula turned to the sound side, which gives the appearance (though not the reality) of widening one arch, and narrowing the other, may have given origin to the description of lateral deviation of the velum in facial palsy, which is current in systematic and clinical works—(Todd, Trousseau, Hasse, etc.) But as the position of the uvula is inconstant, varying in normal as well as abnormal palates, and is consequently of no diagnostic value, it follows that the common descriptions of horizontal deviation of the palate require revision, and ought most probably to be altogether discarded.

What significance, in the present case, has the paralysis of the left levator palati in relation to the diabetes mellitus? To the coincidence of symptoms as yet observed only in a single example, it is impossible to say what importance should be attached. The present instance is, I suppose, unique, and it was discovered almost accidentally. As I happened to be investigating the palatal hemiplegia in facial palsy, I had requested my friends to show me any irregularities of the velum they might meet with. Dr James Carmichael, the Resident Physician attached to my wards, had occasion to examine J.'s mouth on account of toothache, and noticing the condition of the palate, similar to that observed in W.'s case (see my last paper), he drew my attention to it. As J. had no paralytic symptoms, I at first thought the condition of the palate was a natural malformation, which might readily be mistaken for the result of paralysis; and, indeed, it raised a doubt in my mind as to the value of the facts observed in W.'s case. Almost by chance, it occurred to me to test the action of the palatal muscles, when the remarkable distortion represented in the second wood-cut was immediately produced. This at once convinced me that I had here

to do, not with a malformation, but with hemiplegia of the palate, similar to that I had previously seen in connexion with facial palsy, but more interesting in those respects, that the paralysis of the levator palati being in this instance more complete, the distortion during contraction was observed which was not exhibited in the previous cases, and also, that here the palate alone was affected, independently of any other paralysis.

It may, however, be worth while to observe whether the vertical hemiplegia of the palate is to be found in other cases of diabetes mellitus. As the nerve which supplies the levator palati is a filament of the portio dura, the reference to the medulla oblongata, which Bernard's experiments have shown to play so prominent a part in glycogenesis, is at once suggested. However this may be, the case is remarkable as an instance of the anatomical limitation of disease. In the whole body, only one small muscle, the left levator palati, is paralyzed, yet its symptoms are distinct and unmistakable. My more immediate inducement to record the case, however, is derived from the curious illustration which it affords of that vertical hemiplegia of the palate, which in my previous paper I had drawn attention to, in its connexion with facial palsy.

---

ARTICLE VI.—*Report of Clinical Cases, under the Care of* Professor SPENCE, *in the Surgical Wards of the Royal Infirmary, from July* 1863 *to October* 1864. By WILLIAM RUTHERFORD, M.D., late Resident Surgeon.

## TUMOURS.

### *Adipose Growths.*

1. Alex. B., æt. 56. Large fatty growth over scapula. Excision. Recovery.
2. Eliza L. Fatty tumour over deltoid muscle. Excision. Recovery.
3. James J., æt. 62. Pedunculated fatty growth of hip; four pounds in weight had existed for thirty-six years; but had not produced any uneasiness excepting from its bulk, until within a few weeks of his application to Mr Spence, when it had become the seat of stinging pains. Excision. Recovery.
4. Eliza M., æt. 56. Adipose growth of hip; double the size of a fist; had existed for twenty-one years. Excision. Recovery.

### *Fibrous Growths.*

5. Wm. R., æt. 32. Fibrous tumour over parotid. Excision. Recovery.

6. E. H., æt. 60.   Fibrous growth in axilla; had existed for fifteen years without giving her any but slight occasional uneasiness; latterly, however, she had experienced sharp, shooting pains, extending round to the breast, and down the arm; the axillary glands had undergone some enlargement.   She had for many years been subject to asthmatic attacks, often of alarming severity, but at the date of admission she was unusually well, and so anxious to have the growth removed that Mr Spence had no hesitation in excising it.   The wound healed entirely by first intention, and she progressed most satisfactorily until the fifth night after the operation, when, owing to the carelessness of a night nurse, a window was allowed to remain open, from which she caught cold.   An attack of asthma ensued, of such severity that, notwithstanding the early and diligent use of antispasmodics and other remedial agents, she died in the course of a few hours.

The tumour was examined microscopically, and found to be purely fibrous.

### Fibro-cartilaginous Growth.

7. James J., æt. 14.   Fibro-cartilaginous tumour in the tendon of the long head of the crural biceps.   Excision.   Pyæmia.   Death.

### Adenoid Growth.

8. J. B., æt. 45.   Adenoid tumour of breast of fifteen years' duration; the size of an orange; had lately begun to grow rapidly, and to be the seat of occasional pain.   Excision.   Recovery, long delayed by sloughing of the adipose tissue.

### Cystic Growths

9. Alex. T., æt. 66.   Cyst, double the size of a hen's egg, adherent to the tenth left costal cartilage, had existed for four years.   The skin over it had ulcerated.   Excision.   Recovery.

10. Mary B., æt. 53.   Cystic tumour of breast.   Excision.   Recovery.

### Erectile Growth.

11. E. M., female, æt. 64.   Erectile tumour on dorsum of foot; superficially ulcerated.   Excision.   Recovery.

### Scirrhous Growths.

12. S. P., æt. 46.   Scirrhus of mamma; skin unaffected; axillary glands but slightly enlarged.   Excision.   Recovery.

13. J. M., æt. 37.   Scirrhus of mamma; skin unaffected; axillary glands very much enlarged.   Removal of breast and diseased axillary glands.   Recovery.

14. A. M., æt. 50.   Scirrhus of mamma; skin ulcerated; axillary glands considerably enlarged; cervical glands unaffected.   General health pretty good.   Excision of breast and diseased axillary glands.   Recovery.

## Encephaloid Growth.

15. Mary C., æt. 65.  Encephaloid tumour in left side of lower jaw, of six months' growth.  Cervical glands not enlarged.  Removal of the whole of the diseased half of the jaw, together with a thin scale from the right side of the symphisis.  Erysipelas.  Death.

## Epithelial Growths.

16. J. B., æt. 46.  Epithelial cancer of penis, of one year's duration.  Amputation.  Recovery.

17. E. G. female, æt. 46.  Extensive ulcerated epithelioma of one side of lower lip, which had destroyed the soft parts so extensively that the saliva was continually trickling from the mouth, producing excessive irritation both of the ulcerated surface and the skin below it.  The growth had existed for five months, yet notwithstanding the irritation to which it gave rise, the rapidity and extent of the growth, the cervical glands had undergone but very slight enlargement; her general health, moreover, was tolerably good.  Mr Spence removed the growth, and dissected a flap from below the jaw, wherewith to form a new lip.

Secondary hæmorrhage, unfortunately, supervened, which necessitated the removal of the stitches, in order to secure the bleeding vessel.  On the following day the new lip was erythematous.  Erysipelas supervened, extending round the head and neck.  Delirium.  Death.

*Remarks.*—In addition to the above there were many cases of epithelioma of the lip, of small extent, all of which were successfully operated upon after the usual manner.  A case of medullary cancer of the lower jaw, with several ulcerated openings leading down to the growth; the cervical glands were enormously enlarged, and the patient in an excessively weak and cachectic condition; surgical interference was refused.  There were also several cases of cancerous breasts, which were deemed unsuitable for operation; indeed, it is not improbable that operative interference would have been refused, in Cases 13 and 14, by many surgeons, for in both the axillary glands were decidedly affected, and in the latter there was in addition an open sore over the diseased breast.  Fortunately, there was no evidence of internal organic disease in either case, and the general health was wonderfully good; in Case 14 there were general symptoms present which might hastily have been regarded as cachectic, but which were considered to be sufficiently accounted for by the exhaustion resulting from pain and discharge.  In consideration of the tolerably satisfactory general health, and the patient's anxiety for an operation, Mr Spence decided upon giving her a chance of a short respite from her disease, duly reminding her of the almost certainty of a speedy return of it.  In both cases the affected glands were removed by extending the original incision downwards into the axilla.  Case 14 returned to the Hospital six

months after the operation in excellent health, the disease having apparently received a decided check. It is, of course, only too well known that a result so fortunate seldom occurs, the patient's fate being apparently only accelerated by operative interference; but the fact of such being frequently the result ought not to deter from operation, in all cases, where the skin or lymphatic glands, or both, are affected, but ought rather to necessitate a nice and careful discrimination between cases which are suitable and those which are unsuitable for operation when the skin and glands are affected. The age of the patient, the state of the general health, the presence or absence of disease in other organs, the possibility or impossibility of removing entirely the diseased organ, together with the affected glands, are in such cases the chief indications to be attended to in deciding as to operative interference. Although, of course, it would be most reprehensible surgery to operate in such cases simply to gratify the patient's desire, still the degree of his anxiety is often of service in enabling the surgeon to decide as to the treatment which he should adopt when other things are so closely balanced as to leave him in doubt as to whether he should operate or not. In all such cases, however, the patient ought to be fully acquainted with the risks of operation, and the probability of the return of the disease. In Case 14 two large clusters of diseased glands were removed from the axilla without the slightest inconvenience of any kind resulting. It is still an open question as to what extent one might safely remove lymphatic glands. Questions not easily answerable might be suggested as to the conduct of the distal lymphatics after the removal of the gland. It is not improbable, however, that a cancerous gland is of but little service in carrying on the lymphatic flow. At any rate it is, in the case of the axillary glands, e.g., a centre of disease far more dangerous than the original site, and, consequently, its removal is clearly indicated. This is probably only true of glands very decidedly enlarged and hard, for there is every reason to believe that glands but slightly enlarged in the neighbourhood of a cancerous growth are not cancerous at all, but are enlarged simply as a result of irritation, as often occurs in the case of glands in the neighbourhood of innocent growths, when they become very active, or the seat of irritation, as apparently took place in Case 6. That simple irritation may bring this about is sufficiently shown by the occurrence of enlargement of the lymphatic glands in connexion with a limb the seat of erysipelas, or sometimes even of simple ulceration.

## HERNIA.

1. J. J., æt. 26. Oblique inguinal hernia, strangulated for four hours; extraperitoneal division of stricture. Recovery.

2. Jas. B., æt. 60. Oblique inguinal hernia, strangulated for ten hours. Operation. Sac opened. Recovery.

3. John M., æt. 54. Oblique inguinal hernia, strangulated for

thirty-six hours. Operation. Intestine in a state of slough. Death from collapse, ten hours after operation.

4. H. L., æt. 61. Oblique inguinal hernia about the size of a man's head, strangulated for forty-one hours. Patient in a state of collapse. Operation. Sloughing intestine. Death four hours after the operation.

5. B. P., female, æt. 45. Femoral hernia, strangulated for fifteen hours. Operation. Sac opened. Peritonitis. Death five days after operation.

6. Eliza B., æt. 63. Femoral hernia, strangulated for twelve hours. Operation. Sac opened. Recovery.

The above cases call for no special remark ; and Mr Spence's views on the subject of hernia have been so fully given in former reports that it is unnecessary to particularly allude to them in this.

## GENITO-URINARY CASES.

### Stricture of the Urethra.

Some of the following cases of stricture will be found interesting, on account of the treatment adopted.

1. J. C., æt. 54, seaman. Simple organic stricture, of six years' duration, which had been repeatedly treated by slow dilatation. On admission, a No. 2 catheter could be passed, and on the fourth day a No. 3 was passed without exciting undue irritation. He was ordered to take half a drachm of acetate of potash, and fifteen minims of tincture of henbane, thrice a-day. On the sixth day the stricture was split with the aid of Holt's instrument, and a full sized catheter passed immediately afterwards, and the urine drawn off. An opiate suppository was introduced into the rectum, five grains of quinine ordered to be taken thrice daily for the first three days after the operation. No rigors followed; the urine was regularly drawn off by catheter for the first forty-eight hours after the operation. He was instructed in the use of the catheter, and discharged on the ninth day after the operation.

2. Peter B., æt. 34, soldier. Simple organic stricture, of ten years' duration ; was twice treated in a military hospital, by gradual dilatation, on the last occasion for a period of seven months. On admission he was suffering from complete retention, resulting from exposure to cold and wet, and intemperance. He was placed in a warm-bath where he voided urine without the aid of a catheter. Hyoscyamus and potash were given, and next day a No. 3 catheter was passed, and he was ordered to take a grain of quinine thrice daily, in addition to the previous mixture. The urine was ascertained to be healthy. On the eight day after admission the stricture was split with the aid of Holt's instrument. The dose of quinine was increased to five grains thrice daily, but was discontinued on the third day after the operation, when the occurrence of rigors was not further to be apprehended. He was not allowed to

micturate without the use of a catheter for forty-eight hours after the operation. A slight rigor followed the operation, which was easily checked by a little brandy and hot water, and never afterwards recurred. He was instructed in the use of the catheter, and dismissed on the ninth day after the operation. Shortly after leaving the hospital he resumed his intemperate habits, and was frequently under the necessity of sleeping in open fields by night. An acute attack of orchitis ensued, which compelled him to return to the hospital six weeks after his previous dismissal; he had never had an instrument passed, and it was consequently expected that the stricture would have returned to some extent. Such was not the case, however, for a No. 11 bougie could be passed without difficulty. Rest in bed, hot fomentations, attention to the state of the bowels, soon cured the orchitis, and he was discharged a fortnight after his admission.

3. W. C., æt. 29, spirit-dealer. Simple organic stricture, of seven years' duration, barely admitting a No. 3 catheter. Dilated with Holt's instrument. Catheter used for forty-eight hours afterwards. No rigors. Discharged cured on seventh day after operation.

4. J. D., æt. 41, skinner. Simple organic stricture, of sixteen months' duration, easily admitting a No. 3 catheter. Dilated with Holt's instrument. No rigors. Recovery. Discharged on ninth day after operation. Returned a month afterwards to have an instrument passed; the calibre of the urethra was found to have diminished from that of a No. 11 to that of a No. 10 bougie.

5. W. G., æt. 40, labourer. Cartilaginous stricture, with perineal abscess, followed by fistula. Patient had suffered from stricture—resulting from gonorrhœa—for eight years. Six years previous to the date of his present admission he had had it slowly dilated by the use of bougies to the size of a No. 8, since then it had gradually become tighter, and admitted with difficulty a No. 3.

There was a small abscess behind the scrotum, which, on being opened, was not found to contain urine, but on the following day a few drops of urine passed through the wound during micturition. The urine was ascertained to be free from the presence of albumen. He was ordered to take a grain of quinine thrice daily; and on the seventh day the stricture, being then found to admit a No. 3 catheter easily, was split with Holt's instrument,—considerable difficulty was experienced in doing so, owing to the denseness of the tissue composing the stricture. A severe rigor followed the operation, but did not afterwards recur. The fistula rapidly healed after the dilation of the stricture, and he was discharged cured on the twenty-third day after the operation.

6. A. O., æt. 38, labourer. Simple organic stricture, of seventeen years' duration, admitting a No. 4 catheter with difficulty; had on two occasions been subjected to gradual dilatation. Split with Holt's instrument. No rigors. On the third day after the opera-

tion, was seized with orchitis of the left testicle, which soon, however, subsided on the diligent application of hot fomentations. Discharged cured on the nineteenth day after the operation.

7. A. D., æt. 33, porter. Congestive organic stricture, of four years' duration. He came to the hospital suffering from complete retention. An attempt to pass an instrument failed, and produced pretty copious hæmorrhage. An opiate suppository was introduced into the rectum, and he was placed in a warm bath, in which he micturated, unaided. On the following day, a No. 3 catheter could just be passed; he was ordered tincture of hyoscyamus and acetate of potash, and, five days afterwards, Holt's instrument was used successfully, the stricture being dilated to No. 10. No untoward symptoms followed, and he was discharged on the tenth day after the operation.

8. A. B., æt. 42. Cartilaginous stricture, of eight years' duration. He had been in the Infirmary on two occasions; one of these extended over four months, during which gradual dilatation was being employed. Every attempt to dilate it above a No. 9 bougie had always been followed by excessive irritation. It was dilated by Holt's instrument to the size of No. 9, and he was discharged cured on the ninth day after operation.

9. J. C., æt. 42. Organic stricture, with renal disease. Patient had suffered from stricture for six years, resulting from gonorrhœa. He had for many years led a most intemperate life. The urine was found to contain a little albumen, no tube-casts were, however, detectable; he had no lumbar pain, nor swelling of the eyelids, over the sacrum, or of the limbs; in short, no other symptom of renal disease, except the trace of albumen in the urine, which was not considered a sufficient contra-indication for the performance of the operation, and the less so, seeing that no history of any renal attack could be ascertained. As his nervous system had suffered severely from his intemperate habits, it was deemed prudent to proceed with caution; he was therefore ordered to take a grain of quinine thrice daily, a beef-steak, and strong beef-tea daily for dinner. On the tenth day after admission, the stricture was split. No rigor followed; the dose of quinine was increased to five grains as usual; and he was cautioned not to attempt to pass urine without the use of an instrument. Notwithstanding the injunction, he attempted to micturate a couple of hours after the operation. During the passage of about an ounce of urine he shivered violently, and nearly fainted. He was immediately placed in bed and surrounded with hot-water bottles, and brandy and hot water given. He partially recovered, until when, three hours afterwards, yielding to his urgent desire, a catheter (No. 8) was passed, and about an ounce and a-half of urine withdrawn; notwithstanding the gentleness with which the instrument was used, and the ease with which it was passed, he shivered violently, and vomited a considerable quantity of greenish fluid. Gin-toddy

was plentifully given, and a lighted spirit-lamp was placed below the bedclothes in order to induce diaphoresis, and prevent the recurrence of the rigor. In the evening he again shivered violently, and afterwards became delirious for some hours. Complete suppression of urine followed, and notwithstanding the diligent use of diuretics, administered by the mouth and endermically, of vapour and hot-water baths, death ensued on the evening of the day following the operation.

*Autopsy.*—Both kidneys were found to be affected with chronic interstitial nephritis, and riddled with cysts. The bladder was small and contracted, containing only about an ounce of urine. The stricture was about an inch in breadth, and situated about an inch and a-half from the point of the penis. The mucous membrane of the urethra was only torn at the strictured part, and there it was finely fenestrated, and not torn in any special direction. The tissue of which the stricture consisted was also torn in the same fenestrated manner, and the laceration did not extend beyond the stricture in any direction. No urinary infiltration had occurred.

10. G. G., æt. 45, iron-worker. Simple organic stricture, of seven years' duration. On six different occasions he had had it gradually dilated to the size of a No. 8 bougie. The stricture was split to the size of a No. 8. Severe rigors followed the operation, but never recurred, and he was discharged cured on the twelfth day after the operation.

11. J. T., æt. 30. Simple organic stricture, of four years' duration, barely admitting a No. 4. Split with Holt's instrument. Discharged cured on fifth day after operation.

12. G. B., æt. 22, sailor. Traumatic stricture, of two months' duration. He had fallen astride the edge of a boat from a height of forty feet. The perineum was not ruptured, but on passing water shortly after the accident, he experienced excessive pain and scalding, and the urine was mingled with blood; no infiltration ensued, and he soon recovered from the accident, but observed that the stream of urine became less and less until his admission into hospital, when it barely admitted a No. 3 catheter. He was otherwise in perfect health. Holt's instrument was immediately used. A No. 10 catheter was passed immediately after the operation, to ascertain that the stricture was thoroughly dilated, but he was afterwards allowed to micturate without the use of an instrument. On the evening of the second day, he had a slight rigor. On the fourth day, considerable swelling of the penis showed itself: it was then suspected that the rigor on the second day had ushered in the occurrence of urinary infiltration, which had since then been slowly occurring. Free incisions were made into the subcutaneous tissue of the penis, and a catheter was afterwards regularly used to remove the urine from the bladder. Infiltration, slight in extent, also took place into the scrotum, a portion of which sloughed. Although his

progress was tedious, he completely recovered, and was dismissed six weeks after the operation. The urethra was kept from contracting by the occasional passage of a No. 10 bougie. He presented himself at the clinique a month after his dismissal; a No. 10 could then be passed easily.

13. A. B., æt. 55, tailor. Simple organic stricture, treated by the use of bougies. Prostatitis. Suppression of urine. Death. The patient had suffered from stricture for six years; during four years the stream of urine had been very small, and he had frequently had complete retention of urine; two years before admission, the stricture had been gradually dilated to the size of a No. 7, but afterwards gradually contracted; and, on admission, a No. 2 catheter could just be passed. His health was very indifferent, having suffered severely from many years of intemperance. Four years ago, he had had an attack apparently of nephritis; at the date of admission, however, the urine was free from the presence of albumen. Nos. 3, 4, and 5 were successively introduced at intervals of four days. No untoward symptom followed the employment of the former two, but prostatitis followed the use of No. 5. He was repeatedly placed in warm baths; poppy fomentations were applied to the perineum and hypogastrium; tincture of hyoscyamus and alkalies administered internally. No improvement followed the employment of these measures; suppression of urine supervened, and death speedily ensued. A post-mortem examination could not be obtained.

In addition to the above case, there were several others treated by gradual dilatation in the earlier period of the session, all of which terminated satisfactorily; none of them presented any feature worthy of record.

### Perineal Section.

14. J. B., æt. 30, sailor. Irritable stricture, with perineal abscess. Perineal section. Patient not dismissed until the end of the eleventh week, owing to the extreme slowness with which the perineal fistula, resulting from the wound, healed.

15. W. G., æt. 32. Irritable stricture, of eight years' duration. Complete retention of urine. Perineal section. Discharged cured at the end of the eighth week.

16. J. M., æt. 50, tailor. Irritable stricture, of four years' duration. Urinary abscess in perineum. Perineal section. Rigors on day following operation. Suppression of urine. Death. A sectio could not be obtained.

17. H. C., æt. 50. Stricture and infiltration of urine. Patient had suffered from stricture for ten years. For a week previous to admission he had felt a hard swelling in the perineum. On examination, the perineum and scrotum were found hard and infiltrated, and there were several bluish spots on the dorsum of the penis. He was immediately placed on the operating table, and a free

incision made through the râphé of the perineum into a collection of pus and urine ; the stricture was divided on a grooved staff, and several incisions were made into the infiltrated tissue of the penis. A catheter was as usual passed from the perineum into the bladder, and tied in. Stimulants, etc., were freely given, but he sank on the fifth day after the operation.

The rupture of the urethra was found to have occurred in the membranous portion ; the anterior half of the triangular ligament was ulcerated at its base, thereby allowing the escape of the urine anteriorly.

*Remarks.*—The above cases of stricture illustrate—as far as three methods of treatment are concerned—what is unfortunately true of every radical method of treating stricture hitherto proposed, viz., the liability of all to be followed by consequences not less disastrous than the death of the patient. That a new method of treatment should occasionally be followed by the same result, ought not therefore to be considered an insuperable objection, if the operation is not more fatal than the others, and is moreover possessed of several advantages which render it preferable. Moreover, that Holt's method of treating stricture does not effect a " radical cure" is equally true of every other radical treatment of stricture hitherto proposed.

The method of treating stricture proposed by Holt is applicable to all cases where gradual dilatation by bougies has hitherto been recommended, and also to some cases where perineal section has been advised. Over the treatment by bougies it possesses the advantage—important to most patients, but especially so to those met with in hospital practice—of requiring, in ordinary cases, a comparatively short time for its successful completion ; and further, although the morbid tendency to the formation of stricture is, in most cases, not removed, the stricture is much less liable to return than where gradual dilatation has been adopted.

With perineal section, Holt's operation cannot be properly compared, for the former will probably never be superseded by the latter,—at any rate, in the treatment of irritable stricture, and most cases of stricture complicated with perineal abscess or fistulæ. That, however, a case of stricture with perineal abscess and subsequent fistulæ, may be successfully treated by Holt's method, Case 5 affords sufficient proof ; and, further, the same case and Case 8 show that a dense cartilaginous stricture—and Case 12, that a pretty tight traumatic stricture—may be satisfactorily treated in the same manner. Probably, resilient strictures will be found amenable to the same treatment.

With internal urethrotomy, Holt's operation may be favourably compared : here again, however, the comparison is not quite fair, for death has most frequently followed urethrotomy, in cases of irritable stricture, where, had Holt's operation been performed, not

improbably, a higher rate of mortality would have resulted. Internal urethrotomy, as may be performed by Maisonneuve's most ingenious urethrotome, has not been so generally adopted by the surgeons of this country as the merits of the operation would appear to indicate. In France, although its good effects have frequently been less permanent, it has much more seldom been followed by fatal results than perineal section. In the case of ordinary organic stricture, it possesses no advantage over Holt's operation, and, indeed, is not so safe; but for irritable stricture it seems, judging from present experience, to be preferable to perineal section. In internal urethrotomy, the great difficulty has hitherto been, to obtain an instrument which will divide the strictured portion of the urethra, and nothing more; and, moreover, one which can be used in tight strictures. Maisonneuve's instrument, which is far superior to Civiale's, admirably fulfils these indications.

It may, perhaps, be of service to give a short résumé of the treatment generally adopted in the above cases where Holt's operation was performed. If the stricture did not readily admit a No. 3 bougie, it was necessary to dilate it gradually by the passage of bougies until that size could be easily passed. The patient was directed not to pass his water on the morning before the operation, in order that, after the passage of Holt's instrument, satisfactory proof of its being in the bladder might be obtained by the flow of urine through the fine catheter contained in the instrument. This valuable precaution was on no occasion neglected; indeed, so strongly did Mr Spence insist upon its being attended to, that in one case, where the bladder had unfortunately been emptied just before the time for operation, he, although almost positive that the instrument was in the bladder, postponed the operation until urine accumulated in the bladder. The full-sized dilator was most frequently used, but the calibre of the urethra was always previously measured, in order that it might not be dilated beyond its normal size. The operation was usually performed in bed, as rigors were in that case less liable to supervene.

Immediately after the operation, a catheter of the same size as that to which the urethra had been dilated was passed, and the urine drawn off, thereby proving the complete division of the structure. After the operation, the safe precaution of not allowing the patient to micturate unless with the aid of a catheter, was adopted in all, excepting Case 12,—(which, although placed for convenience in this report as the 12th Case, was in reality among the first operated upon.) Possibly had this patient been prevented from passing urine over the recently torn surface, the subsequent infiltration of urine might not have occurred. It would not, however, be fair to conclude, that, because infiltration had followed the non-employment of a catheter for two days after the operation in this case of *traumatic* stricture, it would also be liable to supervene after operation in cases of *organic* stricture; for, in the latter, the

laceration of tissue is confined to the mucous membrane and the submucous strictural tissue ; whereas, in a traumatic stricture, the stricture has usually no such definition, and it is, consequently, impossible to say how far the laceration produced by forcible dilatation may extend : it is, however, even in cases of organic stricture, a safe precaution, and one most acceptable to the patient, for the careful passage of a moderate-sized instrument is much less painful than the contact of urine with the raw surface.   In Case 9, the passage of the urine through the torn urethra gave rise to rigors, fainting, and far greater pain than the operation itself did.

After the first two days, a full-sized instrument was occasionally passed to prevent recurrence of the stricture, and the patients were instructed how to pass an instrument, and enjoined to do so, or to have it done, once a-month or so.

With regard to the internal treatment, the patient was prepared for the operation by having his bowels moved, if necessary, by a small dose of castor oil,—by rest in bed, diluents, hyoscyamus, and alkalies, if there was any tendency to irritation in the urethra.   A grain of quinine thrice daily, with abundance of nourishing diet, was given if the patient's general health was below par ; immediately after the operation, a third of a grain of muriate of morphia in a suppository was introduced into the rectum, and five grains of quinine given thrice daily during the first three days, in order to check the tendency to the occurrence of rigors.   Mr Spence has found this small dose prove quite as efficacious as the much larger dose recommended by Mr Holt.   When rigors occurred, brandy toddy and other hot drinks were given, and hot-water bottles were placed round the patient.

The unfortunate result in Case 9 must be regarded as an accident which would probably have occurred had any other radical treatment been adopted, and one which every precaution was taken to prevent.   The only evidence of renal disease was the small quantity of albumen in the urine, which could not have been regarded as indicative of anything very serious, seeing that the history of the case did not yield any decided indication of there having been a renal attack.

*Lithotomy.*

1. James J., æt. 4.   Weak and emaciated ; had suffered from symptoms of calculus for two years.   Lateral operation ; removal of two pretty large calculi.   Sank from exhaustion on the day following.

2. Hugh A., æt. 16.   Had suffered from symptoms of stone in the bladder since childhood.   Was always of very delicate constitution, and when admitted was, moreover, just convalescent from an attack of small-pox.   He was treated with tonics and the most nourishing diet, and after three weeks, the lateral operation was performed, and a calculus two inches in diameter extracted.   Recovery.

3. D. B., æt. 67. Symptoms of vesical calculus for six years. Lateral operation; calculus size of walnut. He progressed most favourably until the tenth day after operation, when, unfortunately, a relative brought him a basket of green gooseberries, of which, without the cognizance of the nurse, he partook largely. Vomiting and diarrhœa followed, ushering in unmistakable symptoms of pyæmia, from which he died on the twenty-first day after operation.

## FRACTURES.

### Pelvis—Simple.

J. M., admitted Dec. 3.   Cured Jan. 21.
D. C., admitted Dec. 24.   Cured Jan. 27.

### Pelvis—Compound.

J. M'C., admitted Nov. 17.   Died Nov. 19.

### Femur—Simple.

J. S., admitted Feb. 11.   Cured April 17.
W. K., admitted Aug. 10.   Cured Oct. 4.
M. N., admitted Jan. 1.   Cured Feb. 23.   (Extra-capsular fracture of neck.)
L. B. (æt. 60, admitted July 28.   Cured Sept. 3.   (Intra-capsular fracture of neck.)
J. B. (æt. 68), admitted April 21.   Died June 11.   (Intra-capsular fracture of neck.)

### Femur—Compound.

E. R., admitted Sept. 8.   Died Sept. 19.   (Primary amputation at hip.)

### Patella.

A. H., admitted July 7.   Cured Aug. 28.
J. R., admitted Aug. 23.   Cured Oct. 5.

### Both Bones of Leg—Simple.

J. C., admitted Oct. 22.   Cured Nov. 29.
A. F., admitted June 23.   Cured Aug. 13.
M. S., admitted Aug. 25.   Cured Oct. 4.
W. L., admitted Nov. 12.   Cured Jan. 12.
H. C., admitted Dec. 2.   Cured Jan. 8.
J. R., admitted Jan. 19.   Cured March 1.
T. F., admitted Feb. 23.   Cured April 5.
W. S., admitted March 1.   Cured March 30.
P. M., admitted April 19.   Cured May 31.
M. C., admitted Aug. 15.   Cured Oct. 2.

### Both Bones of Leg—Comminuted.

M. M., admitted Aug. 20.   Cured Nov. 4.
H. M., admitted Oct. 15.   Cured Nov. 23.
W. H., admitted Nov. 26.   Cured March 2.
R. S., admitted Jan. 4.   Cured Jan. 27.
W. H., admitted July 26.   Cured Sept. 16.

### Both Bones—Compound.

H. O., admitted Oct. 20.   Cured Dec. 22.

### Tibia—Simple.

G. G., admitted Aug. 18.   Cured Sept. 30.
B. G., admitted Sept. 10.   Cured Oct. 29.
J. W., admitted Jan. 26.   Cured March 7.
D. J., admitted March 26.   Cured May 6.
T. N., admitted Aug. 4.   Cured Aug. 30.
J. P., admitted July 21.   Cured Sept. 7.   (Fracture of malleolus, with dislocation of foot backwards.)

### Fibula—Simple.

E. M., admitted Oct. 13.   Cured Dec. 9.
B. M., admitted Aug. 23.   Cured Sept. 14.
D. M., admitted Aug. 13.   Cured Sept. 10.
J. W., admitted Sept. 10.   Cured Oct. 29.
M. D., admitted Sept. 24.   Cured Oct. 29.
D. K., admitted Sept. 29.   Cured Nov. 13.
J. K., admitted Oct. 29.   Cured Nov. 10.
W. L., admitted Jan. 3.   Cured Feb. 5.
W. M., admitted April 14.   Cured May 16. ⎫
A. M., admitted June 9.   Cured Aug. 18. ⎬(Pott's fracture.)
J. C., admitted Aug. 14.   Cured Sept. 26. ⎭

### Foot—Compound.

R. F., admitted May 24.   Cured June 30.   (Amputation.)
R. K., admitted April 13.   Cured May 31.   (Amputation.)

### Ribs.

A. D., admitted Jan. 5.   Cured Jan. 25.
J. D., admitted July 21.   Died July 25.   (Pleurisy.)
C. F., admitted Dec. 24.   Died Dec. 30.   (Pleurisy and emphysema.)
A. P., admitted April 22.   Cured May 4.
J. P., admitted July 7.   Cured July 24.   (Emphysema.)
W. M., admitted July 20.   Cured Aug. 17.   (Pleuro-pneumonia.)
G. D., admitted Sept. I.   Cured Sept. 18.   (Emphysema.)

### Miscellaneous Cases of Fracture.

M. H.—Fracture of fibula, with compound dislocation of ankle. Amputation; pyæmia.   Death.

N. M.—Fracture of radius, tibia, and fibula.   Cured.

J. C.—Compound comminuted fracture of humerus, fracture of ribs, and femur, incurred two days previous to admission.   On admission, he was in a most exhausted condition.   Pulse, 126; tongue dry; amputation was, in consequence, contra-indicated. Died on the fifth day after the accident.

J. P., æt. 45.   Admitted Aug. 21.   Comminuted fracture of tibia and fibula.   On October 7, Mr Spence, finding that the fragments had not united with sufficient firmness, irritated the fibrous medium uniting the bones, and the osseous extremities by means of a sharp needle; firm osseous union resulted.   Dismissed December 15.

Nearly all the cases of fracture of the upper extremity were as usual treated as out-door patients, without the occurrence of a single unsatisfactory result.

*Remarks.*—The apparatus used by Mr Spence in the treatment of fractures is of the simplest description: the long splint for fractured thigh; the Macintyre splint for comminuted or splintered fracture of the leg, with great tendency to displacement; Dupuytren's splint, and Gooch's splint; but the splint most generally employed consists of two pieces of pasteboard, accurately shaped and moulded to fit the limb. These are separated from the skin by abundance of cotton wadding, and are secured by two or three looped bandages, which can be easily tightened or loosened without disturbing the fracture. This arrangement is a most admirable one, and is followed by as successful results as plaster of Paris, so extensively employed by Berlin surgeons, or starch and dextrin, so much used in Paris and London, without being subject to the disadvantages which result from not being able to examine the state of the limb from time to time, to see that no undue pressure is being exercised on any part, or that abscess is not forming; moreover, the disturbance of the fracture, and difficulty attendant upon the removal of a thick and firm casing, are not encountered; farther, the results which this method of treatment yields are, as regards straight limbs and united fractures, nearly all that could be desired. Those patients with broken legs must, of course, remain in their beds, and not walk about until they are mended, as they are often allowed to do by those who use dextrin and starch, but they have the advantage of the greater certainty of a successful result.

When a fracture of the leg or thigh has thoroughly united, narrow lateral splints of pasteboard and a starch bandage are applied, and the patient is allowed to leave his bed.

There were several cases of dislocation of the shoulder and elbow, none of which, however, presented any feature worthy of note.

## Wounds of the Throat.

W. N., æt. 45. Suicidal wound across the throat, just below the pomum Adami, dividing only the superficial textures and a few small vessels, which were ligatured, and the edges of the wound brought together by silver suture, leaving, however, a small space open in the centre. Recovery.

M. C., æt. 22. Endeavoured to commit suicide with a penknife. There was a punctured wound over the crico-thyroid membrane, leading into the cavity of the larynx. Emphysema had resulted, which was speedily relieved by simply enlarging the cutaneous. wound. No tube was used. Recovery.

## Miscellaneous Cases.

### Ischio-rectal Abscess.

Alex. W., æt. 54. Had suffered from hæmorrhoids for many

years. A month before admission, he had sat for some time upon a cold stone, and ischio-rectal abscess resulted, which, unfortunately had never been opened, although he was frequently visited by his medical attendant. Mr Spence opened the abscess, which was filled with fetid pus and necrosed tissues, and had literally dissected the lower part of the rectum. A large sloughing external pile was also removed. The patient was, when admitted, in an excessively feeble state, from which he never rallied, and sank three weeks after admission.

### Retention of Urine from accumulated Menstrual Discharge.

J. T., æt 16, was admitted suffering from complete retention of urine. After the urine had been drawn off, the hymen was discovered to be unruptured, and distended by a fluid which appeared blue through the white membrane. A crucial incision was made through the distended membrane, and about two pints of fetid menstrual discharge evacuated. Tepid water, containing a little Condy's fluid, was injected twice a-day until all fœtor was removed. No further retention of urine occurred.

### Scald followed by Tetanus.

John H., æt. 25. Four weeks before the supervention of the tetanus the whole of his left arm had been scalded with boiling oil ; the scalded surface had almost entirely healed when he discovered, one morning at breakfast, that his lower jaw was stiff, and that he could not open it as widely as usual. He was ordered a pill containing one drop of croton oil and five grains of aloes. It operated freely, but as the stiffness only increased, and he, in addition, began to complain of a pain in his back, he was admitted into the Infirmary. He was placed in bed and surrounded by hot bottles, dry cupping was performed over the spine, and twenty-five drops of tincture of Indian hemp administered every two hours. On the following day, 25th December, pulse 108. Stiffness of jaws unabated ; abdominal muscles rigid ; to be rubbed with chloroform liniment.

26th.—Pulse 100 ; complains of difficulty in making water. Ordered sweet spirits of nitre.

28th.—Pulse 98. Greater mobility of the lower jaw ; abdominal muscles less rigid ; difficulty in micturition disappeared, tincture of Indian hemp continued.

29th.—Dry cupping again employed. No further improvement.

31st.—In statu quo. Chloroform liniment and tincture of Indian hemp continued.

3d January.—Blister applied to abdomen. Ulcer resulting from burn completely healed.

4th.—A little improvement ; tinct. cannabis ind. stopped.

11th.—Has been gradually improving since last date. No internal

medicine has been given; the rigid muscles have been fomented with hot water, and rubbed with chloroform liniment.

19*th*.—Completely recovered.

The occurrence of the tetanus just before the wound had finally healed was remarkable, but not altogether unusual. None of the remedial measures resorted to seemed to be followed by any marked benefit. Improvement began after the wound caused by the burn had healed, and it is probable that there was some connexion between the two circumstances.

### *Chronic Abscess of the Abdominal Wall above Poupart's Ligament.*

Arthur T., æt. 42, fisherman. Two years before admission, had "strained himself" while lifting a heavy stone. Shortly afterwards a swelling began to appear over the left external abdominal ring, which gradually increased in size until, on admission, it was as large as two fists. The swelling never was painful, but ever since the accident he had constant pain over the left posterior superior iliac spine, and down the back of the left thigh. There was a smooth, painless, colourless, fluctuating swelling of the size above indicated, extending, above the line of Poupart's ligament, from the external abdominal ring to the anterior superior iliac spine. The dimensions of the swelling had always been gradually though very slowly increasing, and were not subject to occasional diminution or increase. The swelling did not apparently communicate with any internal channel, for it received no impulse on the patient's coughing, was not diminished by pressure either constant or manipulatory; there was no pulsatory impulse, no bruit, no borborygmi. On percussion it was invariably dull; there was no projection of any of the vertebræ nor pain on pressure over them. The diagnosis Mr Spence arrived at before making any direct exploration was, that it was a cyst or an abscess, and if the latter, that it was superficial, although the persistent pain over the posterior iliac spine and down the back of the thigh seemed to indicate a deeper seat. An exploratory trocar and canula were introduced into the tumour, and its contents were found to be curdy pus. An incision, two inches in extent, was made in a dependent part of the swelling, and sixteen ounces of pus evacuated. The cavity, which was found to be quite superficial, was lightly brushed over with tincture of iodine, and a compress secured by a bandage placed over it. Steel drops, wine, and nourishing diet were administered. The cavity of the abscess was occasionally painted with iodine and healed slowly. The progress of the case was considerably retarded by the supervention of a rather obstinate attack of diarrhœa, which, however, ultimately yielded to treatment. Recovered.

The above case was evidently one of chronic abscess, originating either in or around the inguinal lymphatic glands, and is of service

in showing with what caution the diagnosis of tumours in the inguinal region ought to be effected, and the difficulty which may attend it.

### Cyst behind the Bladder.

J. W., æt. 29.   Had experienced some difficulty in micturition for some months previous to admission, but never amounting to complete retention until two days previous.   Complete inability to micturate came on somewhat rapidly after his having been exposed to cold and wet.   He was seen by a medical gentleman, who passed a catheter and emptied the bladder; but on the following day he failed to get anything but blood through the instrument.   When brought to the Infirmary the patient was in great agony from the accumulation of urine.   Mr Spence had considerable difficulty in passing a catheter, owing to the previous formation of a false passage; but on his finally succeeding, and drawing off the urine from the bladder, he found that there still remained a considerable swelling in the hypogastric region.   On examining per rectum he detected a fluctuating swelling behind the prostate, which he at once pronounced to be cystic, and proposed to puncture it when the urine had re-accumulated in the bladder.   There was no stricture of the urethra, but the tilting forwards of the prostate by the swelling behind had evidently given rise to the difficulty in passing an instrument, and had rendered easy the formation of a false passage in the lower part of the prostate.   In the evening, Mr Spence punctured the cyst per rectum, and drew off twelve ounces of a pale, slightly albuminous fluid; immediately thereafter the patient micturated without assistance, and the swelling and dulness on percussion disappeared from the hypogastrium.   Prostatitis, cystitis, and afterwards inflammation of the post-vesical cyst, supervened; the latter was at the end of a fortnight again punctured, and a large quantity of purulent fluid evacuated; the canula was left in to act as a drainage tube, but happening to become displaced had to be withdrawn, and, of course, could not again be passed until the fluid re-accumulated.   Sedatives and diluents were frequently administered.   Hot hip-baths were repeatedly used, and poultices or hot fomentations continually applied to the hypogastrium.   Hectic. Death.

At the autopsy there were found,—a suppurating cavity behind the bladder, with well-defined, rather thin walls, containing a few ources of purulent fluid; great enlargement of the prostate, with a suppurating cavity communicating with the floor of the urethra; inflammation of the bladder, with several patches of diphtheritic exudation on its mucous surface; catarrhal nephritis.

The above instance of this rare affection is the second which Mr Spence has met with in his practice.   The fatal result must, unfortunately, be attributed to the false passage in the prostate and distention of the bladder, giving rise to general inflammation of the

urinary organs, and apparently to suppuration of the cyst: for it is probable that had the urinary organs been in a quiescent state, simple puncture of the cyst would not have induced suppuration; and had it done so, the chances of a favourable result would, notwithstanding, have been infinitely greater than they otherwise were.

ERRATUM.—In remarks on operations, page 109 (August No.), the following sentence.—" Out of 52 cases of amputation for disease of the knee-joint only 8 have died, or 1 in 4½,"—ought to have ended thus,—" or 1 in 6½."

# Part Second.

## REVIEW.

*Lectures on the Pathology and Treatment of Lateral and other Forms of Curvature of the Spine.* By WILLIAM ADAMS, F.R.C.S., Surgeon to the Royal Orthopædic and Great Northern Hospitals; late Lecturer on Surgery at the Grosvenor Place School of Medicine, etc., etc. Pp. 334. John Churchill and Sons: London: 1865.

THE difficulty of arriving at a decision regarding any point on which doctors disagree is proverbial, and probably no single subject in the whole range of surgery has illustrated the truth of this trite saying better than has the treatment of spinal disease; and the conflicting opinions of surgeons, physicians, and spine doctors, have left a glorious field which the cupidity and the impudence of quacks have not failed to cultivate, and from which they have reaped and still enjoy a lucrative harvest.

Books on the subject are very numerous, and theories of pathology and systems of treatment are almost equally so, for nearly every bookmaker is also an originator of something new. One can almost fancy that the spine doctors had all prayed Job's prayer, "O that mine adversary had written a book," and that their prayers had been answered; for each volume is a criticism on the past as much as a system for the future. One explanation of this may be found in the very great diversity of opinion and indistinctness even in theory regarding the *pathology* of spinal curvature. Even the great primary and absolutely fundamental distinction between cases of caries of the vertebræ, and those other deformities in which the vertebræ may indeed be altered in shape but are otherwise healthy, is in very many of these works most inefficiently brought out. The tendency is to systematize spinal disease, by arranging cases in groups according to the mere direction of the curvature, rather than take a broad scientific view of the textural changes in the various bones, ligaments, or muscles. An arrangement which clas-

sified tumours of the mamma according to the direction upwards, downwards, to right or left, in which they increased, irrespective of their nature, would be as scientific and probably quite as practically useful as a classification of spinal curvatures under the heads of kyphosis, lordosis, etc. Mr Adams endeavours to be more clear on this point than many of his predecessors, but even he is not successful. Thus, at pages 70–77, we have the treatment of lordosis in the lumbar region under the various heads of,—

1. Cases depending on hereditary configuration; 2. Rachitis; 3. Congenital dislocations of hip; 4. Destructive disease in the lumbar region; 5. Consequent on hip disease.

The following sentence, from the paragraph on the treatment of the fourth form of lordosis, describes a principle of treatment which may be original, but which most surgeons will think erroneous :—

"In all surgical works, without exception, so far as I have noticed, the necessity of the patient's lying down during the progress of caries or destructive disease of the spine is strongly insisted upon; but since my connexion with the Orthopædic Hospital, now more than twelve years, I have invariably opposed this rule, and adopted the practice of applying mechanical support to the spine in these cases, by means either of a leather or steel apparatus, according to the age, and allowing the patient to walk about when so disposed. This treatment I find to be equally applicable to all cases of caries or destructive disease of the spine from its commencement, which we may in most cases diagnose before any angular projection has taken place, and even when the destructive process is evidently advancing, provided the patient be able to bear the support and is disposed to walk."—Pp. 72, 73.

On the nomenclature, causation, and pathology of so-called lateral curvature of the spine, Mr Adams is very full, interesting, and instructive. He specially insists on the possibility and even frequency of cases of spinal curvature in which the *spines* of the vertebræ maintain their relative position in a straight line, while, by a rotation of the *bodies* of the vertebræ on each other, very considerable internal deformity of the vertebral canal may exist; and on the importance of observing this change in its early stages. He proves very well that in many cases this rotation is an important and early clinical fact rather than a mere pathological complication.

This as well as other pathological facts are well illustrated by three beautiful plates, which have a personal and historic as well as surgical interest, as they are drawings of the vertebral column of the well-known geologist Dr Gideon Mantell, examined by Mr Adams, in pursuance of a special written permission left by the owner. Mr Adams very justly lays great stress on the changes which occur in the shape and direction of the oblique articulating facets of the vertebræ, as determining the amount and direction of deformity.

The chapter on the causation of lateral curvature is very sensible, and gives an intelligible digest of the principal theories hitherto advanced, with a fair estimate of their respective values. It includes a very thorough demolition of the "tonic muscular contraction theory" of M. Guerin, and of the practice founded thereon.

From the chapter on Diagnosis the following sentence is well worth quoting, as a caution against trusting too much to the mere position of the spinous processes :—

"The only certain and reliable indication of the existence of lateral curvature of the spine in the dorsal region is a prominence posteriorly of the angles of the ribs on one side, and a corresponding depression of the angles of the ribs on the other side; and in the lumbar region a prominence posteriorly of the apices of the transverse processes on one side, and a corresponding depression of the transverse processes on the other side."—P. 248.

In the chapters on Treatment, while the general hygiene, tonic, and restorative medicines and generous diet are well described, and recumbency and properly managed exercises have their full share of praise, there is still too much trust in metal stays, spinal supports, and other mechanical contrivances, against which, as erroneous in principle and destructive in practice, the teachers of the Edinburgh school have so often borne witness in these pages.

# Part Third.

## MEDICAL NEWS.

### MEDICO-CHIRURGICAL SOCIETY OF EDINBURGH.

#### SESSION XLIV.—MEETING IX.

*5th July* 1865.—Dr HANDYSIDE, *Vice-President* of the Society, in the Chair.

#### I. OSTEO-CEPHALOMA OF THE THIGH.

*Professor Spence* exhibited a malignant tumour of the femur, on account of which he had performed amputation of the thigh about a month ago. The rapid growth of the tumour had been the principal indication of the nature of the disease, otherwise there was some doubt. Accordingly, before amputation was performed, an incision was made; but even then there was some doubt occasioned by the thickening of the periosteum; the operation was nevertheless proceeded with. The girl did well, and the stump was now almost whole. During her recovery she suffered from ascarides, and passed nearly three dozen under the use of santonine.

#### II. EXCISION OF HEAD OF HUMERUS.

*Professor Spence* showed two specimens of disease of the head of the humerus; in both cases excision had been performed. The *first* was a well-marked instance of necrosis of the cancellated texture of the head of the bone; there was very little disease of the surgical neck, and none of the scapula, with the exception of a very little of the margin of the glenoid cavity. In removing the diseased portion of the glenoid cavity, Mr Spence had used a French gouge, which he had found very useful. The *second* case was an interesting one. There was no open sinus, but there was an abscess, flattening of the shoulder, and extreme pain. The neck of the humerus was found to be diseased, and more of it than usual was removed. The cartilage was not abraded externally, but there was a dark spot where it seemed to be separating. There was disease of the cancellated texture, and at the dark spot the beginning of caries.

### III. ANEURISM OF THE AORTA.

*Dr Haldane* showed a specimen of aneurism of the descending aorta, which had ruptured through the right lung into the sac of the pleura. The patient, J. G., a seaman, thirty-eight years of age, was admitted into the Royal Infirmary about the middle of March, complaining of great difficulty in swallowing, and pain in the back of the chest. He had been in good health till eight months before, when he began to suffer from these symptoms, which had gradually become aggravated. On admission he had a pale, somewhat cachectic appearance, was so weak that he was obliged to keep his bed, and could only swallow liquids. When he attempted to swallow any solid aliment he felt it stick at a point which he referred to as behind the middle of the sternum, and the morsel was soon returned. On examination no special lesion could be detected; there was no glandular enlargement, and the circulatory and respiratory systems seemed quite normal. The opinion come to, as expressed in a clinical lecture, was, that the patient was suffering from cancer of the œsophagus, probably of the epithelial form; at the same time the possibility of the existence of an aortic aneurism, though revealed by no physical signs, was not kept out of view. Under rest, careful diet, iodide of potassium, and occasional sedatives to relieve pain, the patient improved greatly. The pain and difficulty of swallowing were considerably relieved, the patient gained flesh and strength, his complexion lost the cachectic appearance it had worn on admission. This change led to a modification in the diagnosis. Had the disease been cancer it was to the last degree improbable that such improvement should have taken place, while it was by no means inconsistent with the presence of an aneurism. Accordingly, it was now considered probable that there was an aneurism of the descending aorta compressing the œsophagus, and by its pressure on the vertebræ, giving rise to the pain complained of. The patient, after having been in the habit of going about freely for some time, was dismissed early in May at his own request, as he said he was unwilling to occupy a bed which some one else might require more. A week afterwards a police-officer came from Leith to ask Dr Haldane if he could give any information in regard to the patient. The officer stated, that the day previous J. G. had fallen down on the streets of Leith, and that the ground, for some distance behind him, was stained with blood. He was immediately conveyed to the Leith Hospital, but was found to be dead. On examining him with a view to his identification, a diary was found in his pocket in which it was stated that he had left Dr Haldane's ward in the Infirmary on such and such a date. Dr Anderson, Dr Haldane's house-physician, went down to Leith, made a post-mortem examination, and brought away the preparation which Dr Haldane now exhibited to the Society. On opening the body the right pleura was found to contain a large quantity of liquid and coagulated blood. The heart was of natural size and quite healthy, as were also the ascending and transverse portions of the arch of the aorta. There was an aneurismal sac about the size of a duck's egg connected with the descending aorta. The sac communicated with the aorta by an opening in the posterior wall of the vessel, which commenced two inches below the origin of the left subclavian, and was about an inch in length. Posteriorly the walls of the sac were chiefly formed by two of the upper dorsal vertebræ, the bodies of which were considerably eroded. The greater portion of the sac was to the right of the vessel; its extremity was adherent to the inner margin of the right lung; the sac had burst into the lung and through the lung into the pleura. The œsophagus was found to be much constricted by the pressure of the sac. Dr Haldane remarked on the great obscurity in the diagnosis of some cases of thoracic aneurism, especially when affecting the descending portion of the vessel. We might, by the general symptoms, be convinced of the existence of such a lesion, while it might be impossible by means of physical diagnosis to recognise its presence.

### IV. OVARIOTOMY.

*Dr Thomas Keith* exhibited four ovarian cysts which he had recently removed by ovariotomy. All the operations were simple and easily performed, and

were followed by the rapid recovery of the patients. Dr Keith had performed ovariotomy 31 times, with the result of 22 recoveries and 9 deaths.

### V. SPECIMEN OF TYROSINE.

*Dr Arthur Gamgee* showed specimens of tyrosine, which he had separated from the urine of a patient with acute yellow atrophy of the liver. Dr Grainger Stewart had sent him a few ounces of urine obtained from the patient after death, and the presence of tyrosine was readily demonstrated.

The following papers were read:—

1. Notes on Acute Yellow Atrophy of the Liver. By T. Grainger Stewart, M.D.

2. On Pepsine in Diseases of Children. By Wm. Stephenson, M.D.

3. On Certain Positions of the Head as a Cause of Syncope. By J. Smith, M.D.

4. Notes on the Polarization of Light by Crystals of Oxalate of Lime. By George W. Balfour, M.D.

---

# GRADUATION IN MEDICINE AT THE UNIVERSITY OF EDINBURGH.

ON the 1st of August, the annual ceremony of Graduation in Medicine took place in the Assembly Hall, under the presidence of Sir David Brewster. The following is the list of Graduates with the titles of their theses:—

*Those whose names are printed in* CAPITALS *passed the Examinations with Honours.*

\*\*\* *Those who have obtained Prizes for their Dissertations.*
\*\* *Those deemed worthy of competing for the Dissertation Prizes.*
\* *Those commended for their Dissertations.*

#### Candidates who received the Degree of Doctor of Medicine.

* Anderson, James Keith, Scotland. On the Typhoid Symptoms of Typhus Fever, and their probable cause.
* BARNES, GEORGE ROBINSON, England. On Pyæmia or Ichoræmia.
  Besnard, Joseph James Alfred, Mauritius. On the Origin of Typhoid Fever.
* BLUNT, THOMAS, England. On Acute Rheumatism.
5*Breda, Peter Johannes Albertus Van, Cape of Good Hope. On the Pathology, Etiology, and Therapeutics of Hæmorrhage.
  Bush, Richard Hake, India. On the different Varieties of Bright's Disease.
  Campbell, Duncan, Scotland. On the Origin and Progress of Medical Science.
  Chiappini, Antonio Lorenzo, Cape of Good Hope. On Natural Labour.
  Chisholm, William, Scotland. On Hæmoptysis.
10*CHIENE, JOHN, Scotland. On Diseases of the Breast.
  Compigné, Horatio David Steele, England. On Diabetes Mellitus.
* Compson, John Charles, England. On the Nature and Treatment of Tetanus.
* Cook, John, London. On Rheumatic Fever.
  Corns, James, Scotland. On Pneumonia.
15 Dayson, Smith Houston, British Guiana. A few remarks on Embolism, bearing chiefly on its Origin from Detachment of Fibrinous Deposits from the Interior of the Heart.
  Denton, Thomas John, England. On Diphtheria.
* DUNCAN, ANDREW JAMES, India. On Ulcer of the Stomach.
** Evans, Thomas, Wales. On the Clinical History and Pathology of Typhus Fever.
* Fentem, Philip Sheldon, England. On Inversion of the Uterus.
20 Forbes, Walter, Scotland. On Antimony.

Fothergill, John Milner, England. On Fever, and the Fevers of Growth and Decay.

Gentle, David, Scotland. On Exophthalmic Goitre.

Glendinning, Douglas, Scotland. On Auscultation and Percussion.

Gregory, William Henry, England. On Hydrophobia.

25 Hoggan, Edward, India. On Dysentery.

Inman, Robert, England. On Tedious Labour.

\* Johnston, William, M.A., Aberdeen, Scotland. On Deaths attributed to Inhalation of Chloroform.

Kennedy, John George, Scotland. On Cancer.

Knight, Alexander Angus Halley, Scotland. On the Position of the Fœtus in Utero.

30\*\*\*Lightbody, William Henry, Wales. Observations on the Comparative Microscopic Anatomy of the Cornea of Vertebrates.

\* M'Donald, William, Scotland. On the Epidemic Fever, at Ayr in 1861–62–63.

\*\* M'Dougall, John Aymers, Scotland. Observations on Acute Phosphorus Poisoning.

\* Mackelvie, Robert, Scotland. On Enteric Fever, its Etiology and Prophylaxis.

M'Laren, Roderick, Scotland. On Heart Disease.

35 Macpherson, Peter, Scotland. On Natural Labour, with some Remarks on certain Complications.

Morison, John, Scotland. On Diabetes Mellitus, its Symptoms, Pathology, and Treatment.

Renton, George, England. On Variola.

\* Richmond, Sylvester, England. On Acute Orchitis.

Rockett, Joseph Hildreth, England. On Epilepsy.

40 Sabben, James Thompson, England. On Parturition, its exciting Causes, and the Gorged and Clotted Placenta.

Sucur, Ryk Tulbagh le, Cape of Good Hope. On Cholera Morbus.

Thin, Robert, Scotland. On the Mechanism of the Human Thorax in Respiration.

Thorburn, Robert, England. On Uræmic Poisoning.

\* Werry, Augustus, Smyrna. On Hysteria.

45 Wright, David, Scotland. On some Diseases of Joints.

46\*\*\*Wyllie, John, Scotland. Observations on the Physiology of the Larynx.

#### Candidates who received the Degree of Bachelor of Medicine.

\*\* Fenn, Edward Liveing, England. An Inquiry into certain points connected with the Physiology and Pathology of the Cerebellum.

\* Fothergill, Thomas Prince, England. On Abscess of the Liver.

Greenfield, Charles Bailey, England. On Obstacles to the Progress of Therapeutics.

4 Wolston, Walter Thomas Prideaux, England. On Erysipelas, its Symptoms, Causes, Nature, and Treatment, with cases in point.

#### Candidates who received the Degrees of Bachelor of Medicine and Master of Surgery.

\* Aitken, Lauchlan, Scotland. On the Hepatine of the Liver, and its relation to the Pathology of Diabetes Mellitus.

\*\* Armistead William, England. Researches on the Histology of the Blood Corpuscles.

\*\*\* Clark, John Barclay, Scotland. On the Reproduction of Limbs of the Crustacea.

Hamilton, Thomas, Scotland. Remarks on the Operation of the Excision of Diseased Joints.

5\* Jamieson, William Allan, Scotland. On Acute Rhuematism.

KIRK, ROBERT, Scotland, On the Pathology and Treatment of Ovarian Dropsy,

Kirkpatrick, William Hutton, Scotland. On Diseases of the Mammæ.

\* Mitchell, Samuel, England. On the Early Stages of Inflammation.

\* MOORE, JOHN MURRAY, England. On certain Diseases of the Pharynx, Larynx, and Trachea, with an Appendix on Tracheotomy.

10 Oman, Nathaniel Daniel Isaac, Scotland. On Vesico-Vaginal Fistula.

Ridpath, David, Scotland. On Symptoms.

Russel, James Cunningham, Scotland. On the Connexion between Cardiac and Pulmonary and Renal Diseases.

Torrop, James Spens, Scotland. Notes on Inflammation.

Warburton, William Pleace, Prince Edward Island. On Pleurisy.

15 Wight, George, Scotland. On Melanosis and Spurious Melanosis.

Wood, Julius John, Scotland. On the Etiology of Climate.

17 Young, Walter Wilson, Scotland. On the Diseases of Joints.

---

## BRITISH MEDICAL ASSOCIATION.

THE Annual Meeting of this Association was held at Leamington on the first four days of August, Dr Jeaffreson of Leamington in the chair. The addresses in Medicine and Surgery were delivered by Dr Stokes and Professor Syme. The matured opinions of two such men on the branches of the profession with which they are respectively connected are so important that we lay them in detail before our readers.

---

## THE ADDRESS IN MEDICINE.

### BY WILLIAM STOKES, M.D., D.C.L., Regius Professor of Physic in the University of Dublin.

GENTLEMEN.—Before entering on the subject of the Annual Address, it is right that I should offer to the Association my thanks for the honour done to me in the request that I should deliver the Address in Medicine in 1865. It is unbecoming for any one to speak of his own labours; but, if I have a claim to your consideration, it is that, for the whole of my professional life, I have sought to improve the social position of medicine in this country. How far those efforts have been successful is not the question here: but that this has been my object, I may simply aver. And here let me declare that which I believe to be true, that the cause of medicine, taken in its broadest sense, whether as to its social, political, or scientific relations, is to be advanced more by the cultivation of the minds, the morals, and the manners, of those who are engaged in it, than by all other influences whatsoever.

But, in your selection of a member from Ireland, I feel that you have honoured its School of Medicine—a school of which all portions of the United Kingdom may justly be proud—a school of which the leading feature has been its devotion to practical medicine, surgery, and midwifery; that is to say, that the application of every discovery in physiology, chemistry, and pathology, to the purposes and ends of the healing art in its widest signification, is, and has always been, the great object of our teachers.

There is a fitness, at all events, in a member of that school appearing before an Association mainly composed of the workers in medicine, who bear the burden and heat of the day, and the chill, and darkness, and storm of the night, bringing, as they best may, health and counsel and comfort to the suffering man.

If we look at the contributions to medicine by the Irish School for the last half century, we shall find that, with a few exceptions, they consist of works, reports, memoirs on clinical medicine, surgery, and midwifery, or of researches in pathological anatomy, mainly having reference to the diagnosis of disease. As compared with other schools, we have not much to show in the way of

discovery in pure anatomy, in animal or vegetable physiology, in microscopic anatomy or pathology, or in organic chemistry; but what the School has done, or attempted to do, is, on the one hand, to enlarge our knowledge of medicine and surgery by careful clinical study of the characters and history of disease; and, on the other, guided by a sound eclecticism, which neither rejects nor blindly adopts a newly announced principle or observation, to test it by the light of experience, and find how far we can give it a place among the aids, the practical aids, of the healing art.

This character or tendency of the Irish School may be traced to traditional and accidental causes. Among the latter, one may be the existence of so large a number of hospitals in Dublin, which, with a population of about 300,000, has not fewer than twenty-one hospitals, each with its distinct staff, the members of which are appointed for life. In this way, the attention of a large number of the young men of the profession was, as they became hospital officers, necessarily turned to clinical study.

Medicine, in its great quality, as a practical art, advances in many directions; of which two may be indicated as the most important.

One is the discovery of new facts, whether relating to physiology, pathology, or therapeutics, each of which, even although its practical bearing be not apparent, enlarges the boundaries of the field of certainty.

The second is the application of those new facts, on the one hand, to testing the value of methods long in use; and, on the other, as a guide in exploring the wilderness of the unknown which stretches around us, which we are seeking to discover, and which we hope in time to reclaim.

For example, it has been long admitted that internal solutions of continuity are so often attended with new, unforeseen, extraordinary, and, above all, sudden symptoms, that their occurrence may be taken as characteristic of this class of internal lesions, as applied to the viscera. Laying hold of this fact, we inquire, Does this formula apply to solutions of continuity of the fluids—say blood? Now, it is found, that new, sudden, and extraordinary symptoms referred to a particular organ, may often so occur, and yet be without change perceptible to any mode of investigation. But the researches of Virchow show that, even here, the truth of the principle is established; for there is, in one sense, a solution of continuity, not indeed of the tissues of the suffering organ, but in the current of the fluids which supply it. An embolus suddenly obstructs an artery, and causes symptoms having a common character, though, of course, varying according to the organ affected,—symptoms which are new, sudden, extraordinary, often violent. Here the embolic pathology confirms principles already in use so far as the solids are concerned, and extends their application to the fluids.

Take it, again, as a lamp for guiding us to new knowledge, and therefore new power. The embolic pathology has at once discovered, as it were, a new set of diseases. It was long known, that interruption of the arterial supply would induce the death of parts for which that supply was intended, as in the disease described by Mr Pott. Here the deficiency is caused either by feebleness at the centre of circulation, or disease at the extremity of the arterial tree. The process, as might be expected, was slow and gradual. But, in this newly discovered class of diseases, we observe the sudden obstruction of a nutritive artery from an embolus, which often proceeds from a disease of one of the valves of the heart; so that, from the sudden occurrence of paralysis of function, we may be led to suspect the existence of an embolus, and to seek for its source. But what if we could see the embolus in the central artery of the retina, as Liebreich did, in a case of sudden and complete amaurosis, and so was led to the diagnosis of a disease of the heart never before suspected to exist?

Here the new observation or discovery exhibits its twofold value. It confirms an important principle already acted on—it leads to a new mode of discovering changes far remote in nature and in seat from that which is the immediate subject of inquiry.

If we look at the collective mass of our brethren over the world, we may distinguish two groups, with lines of demarcation not indeed very sharply defined, but yet sufficiently distinct to justify their separation into categories. In one we find the workers in experimental physiology, in pathology, and in animal chemistry. I use the latter term, because it is not yet established that the laws of the inorganic chemistry are identical with those which operate in the living body. In the second category, we place those who practise the healing art as their daily calling, among whom is to be found a large mass of thinking men, who fulfil the great function of testers of the value of newly announced discovery,—men whose minds, originally strong, and essentially of an eclectic mould, are trained and shaped by working on a more extended field, and by having to employ a more difficult, because less mechanical method.

Now, it is in this class that we find that great body of observers, among which medicine can point out most of her representative men; and this is true for all time past, and for the present also. The observers of disease in the living man, and the faithful recorders of its phenomena, these are or have heretofore been the men who have made medicine a science worthy of respect, even before the introduction of physical means of diagnosis; even, further, before anatomy was known—before physiology had shed its light upon life in health or in disease—before chemistry was a science—on to the time of Morgagni and Bonetus, who studied the changes of organs in disease,—before the microscope, before the employment of those beautiful instruments and methods which in our day have done such wonders in advancing the certainty of our art—the ophthalmoscope, the laryngoscope, the endoscope, the galvanometer, and spectral analysis.

But all men are not, and cannot be, both observers and recorders. In the highest and in the lowest classes of our profession, the lets and hindrances of the daily work are so many and so great as to prevent the mass of practitioners from adding the fruits of their observations to the written records of medicine: whether it be from want of opportunity, of ambition, or of time or taste for writing, they simply work, apparently contented with the knowledge provided for them. But even in the hands of such the science does not stand still; for in their individual cases, at all events, it roots, and grows, and flowers; and so, it may be unconsciously to themselves, they acquire more and more the power of dealing with disease. For observation, even though it be best rendered fruitful by study, has its silent influence on our afterthoughts and actions, even when the special fact or circumstance is forgotten. So it happens that in this class there are many who advance the cause of medicine, inasmuch as they are in themselves exponents of its advance, and must influence more or less all with whom they come in contact.

We are now to study that state of mind by the help of which medicine is made useful. To acquire and improve this *mens medica*, as it has been termed, should be the labour of our lives. To find out its nature will not be a misspending of our time.

You will easily anticipate me, when I say that the condition of mind implied by the term in question is that which makes the good physician. It is not the age of the world that produces such a result, for there have been great physicians and surgeons from the earliest historic times. It is not—to put this in other words—it is not the number of established facts in medicine, nor the amount of such facts known to the particular individual, but it is the power of rightly using those which he does know.

And hence we can at once perceive, that what makes the great physician is less the possession of knowledge of isolated facts, no matter how numerous these may be, than that greater quality of judgment based upon observation—a function of the mind, which, like many others, may be indefinitely cultivated and developed, so that in its exercise it may become (if I am permitted to use the expression) an acquired instinct. This power of balance and of combination—ready for use at all times and in all emergencies, exercised rapidly, almost unconsciously, and leading its possessor to do that which is best and

safest under the circumstances—is that which stamps, not only the great physician and surgeon, but all those who are leaders in government, in arms, in art, or in the liberal professions. We can conceive a practitioner at the present day who knows all the ascertained facts in physiology and pathology, and who may be, notwithstanding, inferior to many who have lived more than a thousand years ago. There is no more decided evidence of an unexpanded mind in our profession, than the decrying the knowledge and usefulness of our predecessors. This was the fault of Paracelsus and Broussais, and in the present day we do not want examples of it.

Looking at the state of medicine in our day, and putting aside the consideration of its vast advances in power and usefulness, as derived from discoveries in physiology, pathology, and diagnosis, we observe that there are some great questions still waiting their solution—questions combining considerations so wide, that they may be said to apply to every branch of the healing art. I will indicate one of such questions—namely, that of the change of type; first, as regards essential diseases; next, as to local affections.

There are many of us who can remember the treatment of fevers and of acute diseases in our student days, characterized by a free use of general and local bleeding, and the employment of other decided antiphlogistic methods. Such practice has now fallen, at least in these countries, into disuse; and even on the continent the employment of an opposite method has been gaining ground.

We can hardly conceive a revolution in practice more complete. Venesection is now, from being the most frequent, the rarest of operations. In place of the loss of blood, we have the exhibition of stimulants; in place of a system of almost starvation, we have the careful use of nutriment.

This change has given rise to the charge against our predecessors and teachers, that they were bad practitioners, ignorant of true pathology, little better than blind followers of traditional error. Not only has their power of observation been questioned, but their morality and honour have been assailed; for it has been suggested that the doctrine of change of type was an invention to cloak their former errors.

It is interesting to note that this is not the first time that charges of the same kind have been brought against the profession. Of these, the most remarkable was that of Broussais, who arraigned all existing and former practitioners for not treating fevers and acute diseases by local bleeding and starvation. Can there be stronger evidence than this, that our modern practice is not a novelty? All his predecessors were in error, because they practised as we do now. I say that this charge was remarkable, inasmuch as its author's views largely influenced European practice for many years.

But the thinking man finds it hard to believe that the fathers of British medicine were always in error, and that they were bad observers and mistaken practitioners. They, indeed, have rested from their labours, but their works remain; and he who reads the writings of Sydenham, of Haygarth and Fothergill, of Heberden and Fordyce, of Gregory, Cullen, Alison, Cheyne, or Graves, must have a very inapprehensive mind, if he fail to discover that there were giants in those days, and that the advocacy of such ideas only indicates a state of mind not consonant with the modesty of science.

The declaration that it has been or can be proved by a more advanced pathology, that bleeding never was the proper remedy for fevers and inflammations, has as yet no scientific ground. It is not yet given to us, notwithstanding all our advance in normal and in morbid anatomy, in the physiology of health or in that of disease, to be able to say, from the most minute examination of the dead organ or structure, what were *all* the conditions which attended it during life, in health or in disease—what were its local vital phenomena, what was its accompanying constitutional state. The words of Goethe, so well rendered by Dr Anster, convey a deep practical lesson to those who would base medicine on anatomical change:

> " Alas! the spirit is withdrawn—
> That which informed the mass is gone.
> We scrutinize it when it ceases to be itself,
> Finger and feel it, and call this
> Experiment analysis."

But let us ask, Which is the most probable of these two suppositions? First, that our predecessors, including such as I have named, were bad observers, incapable of divining the truth, and blind adopters of an antiquated and mischievous method; or, secondly, that the type of disease has changed, and that in our own time. It happens, fortunately, that we can examine two living witnesses of great authority in this matter, and can refer to the works of two more who have left us their written testimony. Dr Watson and Dr Christison are still among us, in health and intellectual vigour,—long may they be so; Dr Alison and Dr Graves have been but lately removed.

Now, all these testify that the character of diseases has in our time changed from a sthenic to an asthenic type; that is to say, from a condition in which inflammatory reaction was the prominent feature, to another where that state was absent, or, if present, only ephemeral—a condition observable in essential and in local disease, in which the antiphlogistic treatment was well-borne, and productive of great relief, to one in which a tonic and stimulant and supporting system was found the best method of guiding the disease to a happy termination.

It is very important to note that these views were not formed from any historical study of the recorded labours of others, but come before us as the actual observations of the great men whose names I have stated to you. They tell us that which they know—that which they themselves have seen. If we refuse this collective though separate and independent evidence—if we hold, with Professor Bennett and with Dr Markham, that the doctrine of change of type is untenable—we must believe one of two things, either that these distinguished men were themselves deceived, or themselves deceivers. From this alternative there is no escape.

Let us hear Dr Alison:—" When we reflect on these facts, we cannot think it unlikely that the result of the inquiry, which I have stated as so important, may be to show either that all causes capable of exciting diseased action in the animal economy, or, more probably, that the liability to diseased actions in the different departments of the animal economy itself, are subject to variations, which are made known to us only by the variation of such phenomena themselves, occurring merely in the natural course of *time*—an element affecting all vital phenomena quite differently from its agency on inanimate nature; and the effects of which on living beings we must take as ultimate facts, to be carefully observed, arranged, and classified, but which we are not to expect to be resolved into any others, which the study of this department of the works of Providence presents."

When I read these words of Alison—the best man I ever knew—it is with a feeling of wonder how it has happened that men should forget what reverence is due to his memory; whether we look on him personally as a man of science and a teacher, or at his life as an exemplar of that of a soldier of Christ. It was my good fortune to be very closely connected with him during my student days in Edinburgh, and to attend him by day, and more often far into the night, in his visits of mercy to the sick poor of that city, to whom he was for many a year the physician, counsellor, and support. This was forty years ago, and at that time he recognised the change. Often has he said to me, " We cannot bleed this man; we must get him wine;" and the wine was got, and given with an open hand, so long as it was required. He used to say, " I am not anxious to put these poor people into hospital; they will get on better at home, if we are guided by looking at their constitutional even more than their local state." This, however, has been well put by Dr Watson, who dates the commencement of the change from that of the first presence of cholera in London in 1833. We can easily believe, however, that the change in question would not occur in all parts of these countries at the same time.

It is very important, however, to connect that which I have now detailed to you with the observations of Alison, published at the request of Dr Christison, in part in 1850, and afterwards in 1856, in another memoir, entitled, "Reflections on the Results of Experience as to the Symptoms of Pulmonary Inflammation, and the Results of Blood-letting, during the last Forty Years."

In 1856, appeared Dr Christison's "Memoir on the Changes which have taken place in the Constitution of Fevers and Acute Inflammation, in Edinburgh, during the last Forty-six Years." This is a memoir eminently characteristic of its author; full of views and arguments which it becomes much more convenient to ignore than easy to confute. Dr Christison shows that the change of treatment in acute diseases is to be considered with reference to fever as well as to local affections. He bears witness that the abandonment of bleeding in idiopathic fevers preceded by a good many years its abandonment in acute inflammation; and that this change in practice took place gradually in all acute inflammations, not alone in pneumonia, because of the improved diagnosis of the disease, but in all others, in many of which no sensible progress in diagnosis had been made. Looking at the epidemics of fever in Edinburgh from the beginning of the present century, he shows conclusively that, in 1817–20, and in 1826–29, their characters were those of Cullen's synocha and synochus—inflammatory, relapsing, critical. Speaking of the epidemics of 1817–20, he dwells on the hard, incompressible pulse, the ardent heat of the skin, the florid hue of the venous blood, and the impetus with which it escaped almost *per saltum* from the vein, the vivid glow of the surface, and the distracting pain and pulsation of the heart and chest. Similar phenomena occurred in the epidemic of 1826–29, and in both bleeding was largely practised with the happiest effects; so that, in the epidemic of 1817–20, the mortality, which was at first one in twenty-two, fell to one in thirty,—a result which disposes of the charge of malpractice against the profession. But, in 1834, Dr Christison found that probably for two years previously a change had been going on: synocha had disappeared; synochus had lost vehement reaction of its early stages; typical typhus was much more common; and what did not come up to Cullen's mark of fully formed typhus was what physicians would now commonly call mild typhus, with more of introductory reaction than we observe now, but with less than in the two epidemics of 1817–20 and 1826–29.

"Accordingly," says Dr Christison, "I doubted, and all the physicians of our hospital also doubted, whether blood-letting was applicable as a remedy to that fever. We could not bring about resolution by a sweating crisis with it; we could not lessen by it the depth of the typhoid prostration; and, worse than all this, our patients ceased to sustain free venesection, a few ounces of blood bringing on faintness, and the constitution refusing to rally afterwards."

Lastly, to prove that this statement is not the result of an afterthought of the present day, Dr Christison refers to his clinical lectures, delivered between 1833 and 1835, to show that he then declared the necessity of a change of practice.

I shall not apologize for giving you another quotation. Speaking of the theory or generalization of the facts ascertained, he says,—

"In epidemic fevers, a change may take place in the constitutional part of the fever; and this change has been exemplified in Edinburgh during the last forty years, by a transition from the sthenic or phlogistic character in the first twelve years to the asthenic or adynamic character in the twelve years which have just elapsed."

And he adds these remarkable words:—

"If this change be admitted to have been proved, there is an end to all difficulty in accounting for the abandonment of blood-letting in the treatment of our fevers. In point of fact, I am able to state very positively, that the abandonment of bleeding in fever was suggested by the observation of a change in the constitution of fever, and in the effects of the remedy on it, and not by any other circumstance, whether extraneous or intrinsic. It is impossible to

ascribe such change of practice, as Dr Bennett has done in the instance of pneumonia, to an improved knowledge of disease. We have improved our knowledge of fever so far as to have been for some time well acquainted with the form of enteric typhus (dothin-enteritis), which was unknown or not recognised at the commencement of our epidemics. But this is a rare form of fever in Edinburgh, scarcely belonging to its epidemics at all. And as to our only undoubted epidemic fevers, typhus and synocha, with their intermediates, we cannot be truthfully said to be better acquainted with them in 1857 than we were in 1830.

"I have given, I hope, a sounder explanation; less flattering, perhaps, to the rising generation of physicians, but surely more honourable to physic itself, more creditable to medical observation and experience, more consonant with the advanced state of medical philosophy. My own convictions on the subject are so strong, that I regard nothing as more likely than that, in the course of time, some now present will see the day when a reflux in the constitution of fever will present it again in its sthenic dress, and again make the lancet its remedy. And in that event it is not impossible that, while we are now charged with giving up blood-letting, because it was discovered to have never been the proper method of cure, we will hereafter be assailed by some new enthusiast in blood-letting, who, in imitation of Dr Welsh, and regardless of the fate of his doctrines, will accuse us with equal justice of having made our late fevers asthenic and typhous by blindly withholding their fittest remedy."

In truth, the alteration of the epidemic character of sthenia and asthenia is established in the case of eruptive fevers, and by an observer who has been held up as a ruthless spiller of blood—Professor Gregory. He has described an epidemic of measles which occurred in 1807 and 1808 in Edinburgh, and which, he expressly states, was not to be treated on the antiphlogistic plan, but rather by tonics and stimulants. This fever was preceded by the inflammatory measles and scarlatina, in which the lancet was used with advantage until the type again changed, and the asthenic fevers, as we have had them for a quarter of a century, reappeared.

The change of type, too, of the local acute inflammations followed, as might be expected, that of the essential diseases; and the change in treatment resulted, not from any new light shed on the practice of medicine, not from any new views in pathology, not from our advance in diagnosis, vital or physical, but from the observation of the general symptoms on the one hand, and the results of treatment on the other.

I may now add the results of my own experience in this matter. I remember the period when the change of type took place in Ireland; and am under the impression that it was observed earlier in Ireland than in Scotland, or at least in England. The great epidemic of fever in 1828 was a remarkable one from its compound nature, and seemed to be made up of synocha, synochus, and enteric typhus. But nothing was more remarkable than the vehemence of the inflammatory reaction in many cases; and it is a curious fact that this was sometimes seen at its highest pitch in the relapses, when it was often far more violent and dangerous than in the first attack. Local bleeding was largely employed. In many cases, venesection or arteriotomy had excellent results; so that, although there were abundance of cases with prostration, and others marked by the typhoid condition, the old sthenic character had not disappeared. The amount of wine used at that time in hospital was quite insignificant as compared with its consumption for the last twenty or twenty-five years. In Dublin, at least, this epidemic passed into one of intermitting fever; and it was then that I ventured on testing the nature of the practice recommended by Dr Mackintosh of bleeding in the cold stage. The result of the experiment was against the use of the lancet; but I mention it, as indicating the time when it may be said that venesection was abandoned in our wards.

Thus, between 1822 and 1828, the sthenic character of essential and of local disease existed, and the lancet was freely used, often, as I believe, and as I

have elsewhere stated, with too great freedom; but I well remember observing
the frequent occurrence of the phenomena mentioned by Dr Christison—the
vehement action of the heart, the incompressibility of the pulse, the vivid red-
ness of the venous blood, and the force with which it spouted, almost *per saltum*,
from the orifice in the vein.  I have myself taken as much as sixty ounces in
a case of active congestion of the brain, with hemiplegia, before any impression
was made on the arterial excitement: in this case, complete success followed.
In rheumatic fever, too, we found the use of the lancet in the early stage of the
disease to be productive of great relief.  Venesection was seldom used more
than once; but its effect was to shorten the duration of the disease, to lower the
fever, to lessen the liability to the so-called metastases, and to render the whole
case much more amenable to treatment.  But I have not bled in rheumatic
fever for the last quarter of a century; for the whole character of the disease
has changed.  We have not had for many years the bounding pulse, the exagge-
rated heat and sweating, nor the same liability to acute inflammations of
internal parts.  The action of the heart is often feeble; and the tonic and sup-
porting plan seems called for from an early period.  Another point worthy of
remark is, that cardiac and aortic murmurs of the anæmic kind have for many
years been much more frequently observed, both during the attack and in the
convalescence, demanding the use of iron for their removal.  Observations of
a similar kind apply to other acute diseases; such, for example, as erysipelas
and other infections of the skin.  Before 1830, we had, as an ordinary disease,
the acute phlegmonoid erysipelas, attended with inflammatory reaction, vivid
redness, and great swelling of parts.  The practice of free leeching gave great
relief; so also did that of incisions.  All these characters have, to a great
degree, disappeared.

It is needless to add more examples; let us rather turn to another kind of
evidence.  Hitherto the change of type has been recognised and determined
less by anatomical observation than by the observation of symptoms, and still
more by the application of the therapeutic test.  Remedial measures of a cer-
tain kind were found to fail and to be hurtful, where they were formerly safe
and successful; and, conversely, the use of a supporting system of tonics and
the free employment of stimuli were found necessary and safe where formerly
they did injury.  To the all-important subject of the value of therapeutic study
as a means of elucidating the laws of disease, I may presently return.  But I
think that I am in a position, from actual observation, to declare that morbid
anatomy adds its testimony to the truth of these views.

The Pathological Society of Dublin has been now established for twenty-six
years, during which time it has held weekly meetings for six months of each
year.  As one of the secretaries of that society, I have had full opportunity of
seeing and examining the recent examples of diseased structure brought weekly
before the body—amounting to nearly 3000 specimens—the collected products
of the various hospitals of the city; and this result is remarkable, that the speci-
mens of acute disease have had a character very different from that commonly
met with in Dublin between 1820 and 1830.  As a general rule, these speci-
mens all showed appearances indicative of a less degree of pathologic energy.
In pneumonia, for example, the redness, firmness, compactness, and defined
boundary of the solidified lung was seldom seen; and that state of dryness and
vivid scarlet injection to which I venture to give the name of the first stage of
pne·monia, became very rare.  In place of these characters, we had a condi-
tion more approaching to splenisation,—the affected parts purple, not bright
red; friable, not firm; moist, not dry; and the whole looking more like the
result of diffuse than of energetic and concentrated inflammation; or we had
another form, to which Dr Corrigan has given the name of blue pneumonia, in
which the structure resembled that of a carnified lung, which had been steeped
in venous blood.

Let us turn now to the serous membranes, and the same story is repeated.
The high arterial injection, the dryness of the surface, the free production,
close adhesion, and firm structure of the false membranes in acute affections of

the arachnoid, pericardium, pleura, and peritoneum, with which we were so familiar before the time in question, ceased in a great measure to make their appearance. The exudations were more or less hæmorrhagic; the effused lymph lying like a pasty covering rather than a close and firm investment; it was thin, ill defined, and more or less transparent. In many of such cases, during the disease, as the late Dr Mayne has shown in his memoir on pericarditis, friction-sounds were never presented. Serous or sero-fibrinous effusions tinged with colouring matter replaced the old results of sthenic inflammations, and all tallied exactly with the change in the vital character of the disease.

It has happened to me—and I mention this in evidence that we were not mistaken as to cases peculiar to the sthenic form—that a few instances of disease in its old inflammatory characters have appeared in isolated examples, and at irregular intervals of time; so that we at once recognised their nature, and employed with success the old treatment in all its vigour—employed the lancet, although for many years its use had not been resorted to. This is very important, as showing that there are influences, the nature of which is as yet unknown, that affect the vital character of local diseases in an inconstant manner.

In an address of this kind, it is plain that this subject cannot be handled in an exhaustive fashion; it is enough that we touch upon a few of the larger subjects of inquiry. And now it will, I hope, be admitted that, with reference to the doctrine of change of type, we have brought to bear upon it the great sources of evidence as to the nature of disease.

Of these, the first is the study of vital symptoms—that study in which the older physicians so excelled, and which, from the very necessity of the case, they probably carried further than we now do, armed as we are with the many aids of physical diagnosis;

The second is the study of the characters of the anatomical changes induced by disease, and this in a comparative way, as referring to successive periods of time;

And the third is that which is derived from the results of therapeutic experiments.

Looking at the question from any one of these points of view, we come to the conclusion, that the doctrine of change of type is a true one; while, if we take all these facts, and observe how they point to the same conclusion, we must, to use again the words of Alison, accept the change of type as an ultimate fact in the history of disease.

But are we to conclude that this asthenic type of disease is always to continue? Are we to forget that in our own time we have witnessed its advent and growth? Is it not possible—nay, probable—that we or our successors may witness its disappearance, and, coincidently, the return to an antiphlogistic medicine, regulated and tempered by the advances in diagnosis and pathology which have been meanwhile made? I have given you the opinion of Dr Christison on this matter; let us now hear Dr Watson:—

" I am firmly persuaded by my own observations, and by the records of medicine, that there are waves of time through which the sthenic and asthenic characters of disease prevail in succession, and that we are at present living in one of its adynamic phases."

It is very important that the change of treatment of fevers and acute local disease be traced to its true sources. This change has not proceeded from any advance in our knowledge of physiology or of pathological anatomy, nor from any new principles of practice announced as applicable to all time, and therefore implying that our predecessors were groping in the dark, or wilfully or ignorantly following a system of traditional error. To each one of us the honour of our profession, which includes its scientific character and its power of development out of itself, has been intrusted. Medicine, like other professions involving human interests, has been continually assailed from without, and harmlessly. Attacks on her honour proceeding from her own children, no matter what amount of ability may be shown, while they inflict a deeper wound,

ever recoil upon their authors. This has been well exemplified in the case of Paracelsus, who burned the books of the Greek, Roman, and Arabian physicians. It is well exemplified in the case of Broussais, who, in speaking of the Eclectics, spares no term of contempt. According to him, they were guilty of shocking contradictions and absurdities, even of imposture. But, he says, "What matter for all this? Falsehood is no longer a vice. Its apotheosis has been made by this famous party, who think that they are to reign for ever." He goes on to speak of their gratuitous suppositions, of their assertions void of truth, of false imputations, inaccurate quotations, and impudent denials and total perversion of the use of words. He is the only light; and in his devotion to truth he has scorned the miserable ambition of practising in gilded saloons, and the possession of the honours of the profession.

In common with Dr Christison, I have to express my regret if in the discussion of this great question I have to introduce something of the controversial element. Let us inquire whether the distinguished Professor of Clinical Medicine in Edinburgh, as well as Dr Markham, have not in some degree followed the examples of Paracelsus and of Broussais, not indeed in violence of language and indiscriminate denunciation, of which they are incapable, but in the attempt to show that their predecessors were deficient in observation and erroneous in practice.

In the very limited analysis which I have given of Professor Christison's views on the subject of the change of type, and in the statement of such opinion as I have been led to form on the question, I have at least endeavoured to show that it is not one which is to be lightly disposed of. When I had determined on the general nature of a discourse fitted for this occasion and this audience, I felt a difficulty, on recollecting that in 1861 Dr Markham, who then filled the place that I do now, had in his address argued against the doctrine in question. But, on referring to the "Gulstonian Lectures" of 1864, I was happy to find that Dr Markham, when questioning the value of his predecessors' observations, does them the justice of declaring his belief, that as they have advised their followers to try all things by the light of their own reasoning and observation, they will be the last who would object to the freest criticism of their opinions. Let me, who am one of those who hold views opposite to those of Dr Markham, gladly reciprocate the compliment. It is but justice to Dr Markham to remind you, that he holds that general and local bleeding are remedies of great value when employed on fit occasions, and that at the present day cases are sometimes injured from our timidity in using them.

It appears pretty certain, that the change in treatment of such physicians as Alison, Christison, Watson, and Graves, did not solely spring from the results of the therapeutic test; but that the study of the symptoms and general characters of disease was equally considered by them. In this change, too, they and their many followers in the three kingdoms have only done that for which a knowledge of the history of medicine has given abundant precedents. It would be well if it could be remembered that in the study of disease we are to look beyond anatomy, and beyond physiology; as Professor Autenrieth well observes, the "constitutio morborum stationaria" of Sydenham has been nearly forgotten, or else confounded with the permanent influence of the seasons, or accidental atmospheric changes.

"All diseases, contagious and non-contagious, acute and chronic (the latter, however, seldom except when attended with some degree of general excitement), have been observed to preserve a certain constitution or general character, which continues for a number of years in succession, with occasional interruptions, until it is replaced by another constitution of a different kind."

"Again," he says, "accurate observations are still wanting to determine how this periodic constitution is confined to certain parts of the world, or extends over the whole, and whether its different species follow each other in a regular order of succession. If such should at any time be determined, it will enable the physician to foretell the character and most appropriate treatment of future diseases.

"The general indications, of course, vary with the nature of the prevailing constitution; and consequently during one period stimulating remedies, during another alvine evacuations, during a third venesection and the antiphlogistic plan, will constitute the most effectual treatment."

Let me now read another passage from Autenrieth. It is not very flattering, I admit; but it is well to know what other people think of us. This was written a quarter of a century ago.

"This very circumstance has caused much confusion in medical opinions, and has occasioned the reputation and the downfal of many an infallible system, each of which is in its turn consigned to oblivion, and perhaps again revived as a novelty at some future period. The English boast much of the astonishing improvements in science, and deride the ignorance of their predecessors, regardless of the old proverb—' Everything has its day.' Whenever, therefore, the periodic constitution undergoes an alteration, they either obstinately uphold their usual plan of treatment to the manifest injury of their patients, or else blindly embrace some system, to them new, but which really rests upon ancient and established principles. In general they do not fail to make use of so much exaggeration in support of their opinions, and thus succeed in misleading so many, that none but very well informed physicians can distinguish the fallacy of their arguments.

"The medical history of Great Britain affords many striking proofs of the truth of these assertions, and is replete with examples of the singular obstinacy with which the English cling to opinions once formed,—a circumstance which has materially contributed to obstruct their attaining to general views and impartial conclusions. Even to this day, a warm contest is carried on (less, however, in books than in the debates of learned societies) between the senior and the junior parts of the profession, the former still inclining to Brunonianism, while the latter attribute nearly all diseases to inflammation. Both, indeed, appeal to experience to prove the justice of their principles, and seem entirely to forget that, while the propriety of their practice, as applied to particular cases, remains unimpeached, the very nature of the diseases themselves may have been changed."

There is a statement made by Dr Bennett in his great work on the *Practice of Medicine*, which is at least a startling one. It is that in his treatment the mortality of pneumonia has been reduced by a large percentage. The mortality in Edinburgh, according to him, under the system of his predecessors, was no less than one in three—equal to that in the first outbursts of Asiatic cholera. The statistics belong to two periods—namely, from 1839 to 1849, and from 1812 to 1837. The mortality in the first division was even more than one in three. The results of other statistics are also given, of which the most valuable are those of Louis, who brings out the mortality of those bled moderately, and at an early period, as 1 in $7\frac{1}{4}$, and of those in which the blood was taken at any time from the first to the ninth day, as 1 in $3\frac{1}{2}$.

Now, it is unnecessary to remind an audience constituted as this one is, of the difficulties which attend medical statistics in general, and those relating to therapeutics in particular. When I began the study of medicine, pneumonia was considered to be far more manageable than other acute visceral inflammations; and that its rapid retrocession took place under the old treatment in a vast number of cases, no man can doubt who lived and practised in that time. But in these statistics of pneumonia I find an omission. If we look at the diagnosis of this disease in a purely physical point of view, we run the risk of committing the great error of confounding cases, the constitutional nature of which is widely different—cases on the one hand of original idiopathic pneumonia occurring from accidental causes, and cases in which the change in the lung is secondary to some form of fever. And this makes a most important difference. It is my conviction that many of the so-called cases of pneumonia which have occurred in the United Kingdom since 1830 were really examples of the latter form. But, further, it is certain that in many instances the occurrence of the pneumonia is attended with such a change in the constitu-

tional symptoms as to deceive the practitioner, and hide from him the fact that he had to deal with a secondary, in place of a primary affection. In some cases we see a change from the essential to the symptomatic character, while in others this remarkable circumstance occurs that, coincidently with, or very soon after the development of the symptoms and the physical signs of pneumonia, the fever ceases; so that we have long come to the conclusion in the Meath Hospital, that many of these cases with every local symptom and sign, are in truth only examples of aborted fevers, ending critically in pneumonia, just as we see, in some cases of variola, the fever ceasing with the pustulation of the skin. If these things be true, how important is their recognition—how inconsequential the conclusions as to treatment, based on statistics from which such facts are excluded!

Let us now inquire to what sources Professor Bennett traces the changes in practice, and that success in the treatment which has reduced the mortality of pneumonia from one in three to one in thirty-six in complicated cases, while in the uncomplicated cases there was no mortality at all. They are stated to be the improvements in diagnosis on the one hand, and the adoption of a practice founded on the cellular pathology on the other. As to the first, how the improvement in the diagnosis of pneumonia could have led to the change of treatment of fevers, and cerebral or abdominal inflammation, is hard to understand—this has been well put by Dr Christison—unless it could be shown that the failure of bleeding in pneumonia led men to think it would also be useless or injurious in other diseases. It must be remembered, however, that the change in treatment began first as to fevers; and it was the observation of the change of type in that class of diseases that led to the idea, and afterwards the demonstration of a similar change as to local affections.

But improvement in the physical diagnosis of pneumonia can hardly be said to have advanced since the time of Laennec. And it is clear that, looking at therapeutics, the influence on them of any such improvement is indirect, rather than direct. We know better the seat, the period of commencement, the periods of pathologic changes—their amount in some cases, and the complications with other forms and centres of diseased action; but we get little, if any, new light as to the proper remedy. Take any or all of the three great cases of intrathoracic inflammation. The physical signs as to character and succession are essentially the same in the asthenic or typhoid forms, as in those with the highest inflammatory reaction. Therefore, to say that the alleged improvement in treatment proceeded from the advance of the physical diagnosis, is a proposition which must be rejected.

The adoption of practice founded on the cellular pathology of Virchow is a much wider question. It is to be remarked here, that the therapeutic test of the value of these means is still to be applied. We find a treatment indicated as based on, or as directly flowing from, the new pathology. Yet what is this treatment? It consists in the use of certain measures, and the abstaining from their opposites; but neither in principle nor detail does it differ from that adopted by the older clinical physicians in asthenic local diseases, from the time of John Peter Frank downwards to that of Bateman in 1809, who was compelled to use venesection—a practice opposed to his former views. It is, then, no new treatment; and it, at all events originally, could not have been based on a pathology of which nothing was known at the time when it was first adopted. I do not say that, if the cellular pathology be finally demonstrated to be true, it will not—to adopt the expression of Dr Bennett—be attended by cell-therapeutics as its necessary complement. New modifications of treatment may be discovered, and probably will be discovered; while on the other hand the old methods stamped by experience, and the discoveries of enlightened observation, may be brought to bear in confirming the new truths.

But, in the present state of our knowledge, it cannot yet be said that the views of Virchow have had any direct influence on the healing art,—that is to say, they have not led us to any new remedy; they have not explained the action of many of the old ones.

I am not, you will believe me, speaking in a spirit of depreciation of the labours of Professor Bennett, and more especially those of Virchow. Looking at the practical results of Virchow's labours, we may separate the consideration of the cell-pathology from that which belongs to questions of another kind. The demonstration of the whole of embolic diseases, and the investigations of both Bennett and Virchow, as to the part played by the colourless globules of the blood, are priceless additions to medicine; not, however, as giving us directly any new means of cure, but as enabling us to avoid errors into which our ignorance of these things led us before. We shall not now confound a case of embolus of the pulmonary artery with asthma or with hydrothorax; nor, in a case of amaurosis from obstruction of the central artery of the retina, shall we be led to treat it as a symptom of disease of the brain. It is plain that, whatever be the result as to practical medicine of these labours, they are to be held in the highest estimation, as evidencing the onward march of discovery in a certain direction—as enlightened efforts to place pathology on a more definite basis. Yet, I repeat it, the cellular pathology with reference to practical medicine stands in a position analogous to that of our vastly improved diagnosis. Supposing all that is announced by Virchow, Weber, and Bennett, to be established; still, up to the present time, it would give to the healing art only indirect assistance.

Let us permit the elements of tissues, the ultimate cell, or granule, to share with the great compound organs of the body the property of specific action; let us admit that there is no spontaneous generation of cells from an amorphous blastema, but that every cell proceeds from a cell; that cells are the ultimate elements of animal as of vegetable structure; that differences of function depend on differences of their contents; that every animal is a sum of vital unities, every one of which manifests all the characters of life, deriving, it may be, its stimulus and intensity from other sources, but itself alone performing its actual special duties.

Let us hold, further, the whole doctrine of neoplasms, which sets forth that every pathological structure has its physiological prototype; and that even cancer has not its specific difference, any more than pus, but that its supposed peculiarity is traceable to the stage at which we examine it; that the law of histological substitution be accepted physiologically, as when one tissue of a similar type replaces another; and pathologically, when a different tissue, but still one having its physiological prototype, comes into play. Again, if it be found to be true that all dyscrasiæ have a local origin, and are dependent on a permanent supply from a local origin; that fibrine is not a constituent of living blood; that between the pus-cell and the colourless globules of the blood there is no difference, so that the term pyæmia must be given up as a condition susceptible of morphological demonstration, but implying a complex mass of conditions, the central point of which is not a purulent infection of the blood; that embolism is the key to the study of metastasis; and that inflammation is nothing, as Bennett teaches, but an alienation of nutrition,—if I say, all these things be true, it does not appear that they furnish knowledge that would tell us why this or that line of treatment is from time to time found efficacious—a knowledge that would direct us in the cure, though to a certain degree it might to the prevention of disease. In truth, such knowledge throws little light on the action of medicines, on the laws of periodicity, and on the great phenomena of essential diseases, as to their origin and specific character, spread, secondary lesions, their crises, and the influence of treatment on them. And therefore I conclude that, even if, under the treatment of a distinguished professor of this science, the mortality of a frequently recurring disease has been annihilated, that result cannot be as yet traced either to a new diagnosis or a new pathology, as exercised by him; nor, conversely, can the great alleged mortality of former times be attributed to imperfect diagnosis on the one hand, or to traditional errors in treatment on the other.

But let us ask, if it be true that the mortality of pneumonia has been in the latter years so much lessened, to what is this to be attributed? It may be that

we have to deal with a disease of less organic activity or tension, so that in certain cases it may, like a fever, subside spontaneously; perhaps, too, like a fever, under its own law of periodicity. And it is probable, at all events, that the more a local disease corresponds in its vital character to the epidemic constitution, to use the words of Sydenham, the more will it appear under the laws of periodicity, for good or for evil.

It is very hard to predicate the limits to which the study of healthy and of diseased structure in its mechanical or chemical relations may lead us; and it is possible that even such investigations of the laws of organization may result in giving us power to infer the existence of phenomena not yet discovered—to proceed, as Adams and Le Verrier have so gloriously done, from the seen to the unseen, and approach one degree nearer to the solution of the great problem of life in health or in disease—or, again, that of the action of medicines. But we must take heed not to leave any path of observation unexplored, nor to despise those investigators who, from necessity or choice, follow a less mechanical method, but who have made medicine progressive—every day less an art, and more a science. Every established microscopic observation in normal or in morbid anotomy—every faithful analysis of any solid or fluid in the body—every discovery as to the spectral phenomena of the blood or of the liquid secretions of any gland, nay, of any cell in the organism, though to a one-sided view it appears useless, is truly a precious thing.

And here it is fitting to remark, that there is nothing in the doctrine of cellular unity, of cellular independent action, and in all the processes of cell-growth, proliferation, and decay, which is inconsistent with the doctrine of change of type of disease. Dr Bennett allows that change of type may be admitted as to essential diseases, but seems to hold that, as to acute or chronic organic disease, the doctrine is to be rejected. But, if there be a change of type in the essential diseases, it is difficult to understand why there should not be a change in character of the secondary effects of those diseases, whether these are met with in the nervous centres, in the thoracic or abdominal organs; so that we may have sthenic or asthenic cell-growths, cell-proliferations, cell-transmutations, and cell-decay; and all this without referring to the notion, that, because local disease, as well as general, exhibits now a lower activity, therefore the physical state of man *in health* has deteriorated.

We are still very far from determining the laws of the so-called zymotic diseases; but this seems certain, that at the invasion of epidemics the strong man is struck down, and often exhibits the phenomena of the disease in the most aggravated forms. In the epidemic of 1827 in Ireland, nothing was more remarkable than this, that its virulent forms, especially that in which it so closely resembled the yellow fever of the tropics, were at first seen in the finest and strongest men. It is even probable that in these diseases the very vigour of the system may imply a greater malignity or activity of the processes which constitute the disease. The existence, then, of a changed type of disease may be admitted without the necessity for believing that the human species has degenerated.

Before concluding, it is right that we should consider the relations of therapeutics to medicine. It will be admitted by most thinking men that the study of diseased or healthy organization has revealed more of the effects than of the essence of disease. So subtle are the conditions by which the equality of life is preserved, that, in a vast proportion of instances of death, the most refined anatomy and chemistry fail in discovering a commensurate change, or in explaining why what was a living creature yesterday lies before us in a few hours a decomposing mass of clay. Hence, we must be cautious in extensively adopting any therapeutical system which is solely based on inference from visible organic change. In the present imperfect state of our knowledge, we must not neglect that study of therapeutics which is essentially experimental and inductive; and if there be one thing wanting more than another in our science, it is that men should know the nature and difficulties of therapeutic evidence. If, as I have often heard Professor Acland observe, only a

few of our well-instructed brethren who are in charge of public institutions, well aware of the established laws of disease, whether essential or non-essential, and good observers, were to take up any one remedy, whether new or old, say digitalis, and faithfully record on the one hand the character and history of the case, and on the other the results of the use of the particular medicine, or other therapeutical proceeding, we should erelong have such a mass of unbiassed statement of facts, that safe conclusions could be drawn. Until this is done, the position of therapeutics will be an inferior one. It will not be any trustworthy guide in practice, except in a few salient instances, and will be powerless in its other great function of being the key to, and the test of, pathologic conclusions.

To bring therapeutics up to this level seems to be the great desideratum. We may fairly hold that the time is ripe for the commencement of its study with the view to its higher functions or development. Without placing limits to the material investigations in which we are aided by the microscope and by chemistry, we may believe that our knowledge of the intimate structure and composition of the solids and fluids of the body is so extended as to give to the therapeutist reason for holding that he is now far better acquainted with the living organism than he was a quarter of a century ago; and that so he has a broader and more secure foundation to build upon. But the therapeutist must also possess assistance of another kind. He must know the principles of accurate reasoning: he must distinguish between the *post hoc* and the *propter hoc*; he must be content still to deal with vital phenomena as constituting a class of the nature of which our knowledge is so deficient, that we have still to study their modifications by external agents, experimentally, and without as yet much reference to their relations to structure or to vital chemistry; he must take into account the laws of periodic action in health and in disease, and determine, or seek to determine, as he proceeds, whether the simplest form of acute local as well as of general disease is not under some of these wonderful laws; he must study the question as to whether medicinal interference extinguishes morbid action, postpones it, or, by breaking its circle, as suggested by Professor Boeck, though this be followed by temporary good, deranges the process which is to end in its removal; he must well understand that certainty in medicine must be approached by the balance of probabilities, and have a full insight into the difficulties of medical statistics, which result from the labours of more than one observer. Other circumstances will suggest themselves to you—as the influences of locality, of race, of age, sex, habit, and previous history. I will not dwell on them, further than to remark that, had Broussais attended to one of them, in particular, he would not, I think, have fallen into the error of declaring the non-existence of essential fever from observing disease within a narrow circle of the world.

If therapeutic science is to advance, it must be followed and studied in the most severe scientific spirit.

I have to thank you for the courtesy and patience with which you have listened to this somewhat dry discourse. We have indeed dealt with important subjects; and you will, I hope, believe me when I say, that no one in this room can feel more than I do how defective has been their handling. I say this in no guise of mock modesty. But I have endeavoured to speak as a practical physician, who has worked for forty years, to a body of his brethren engaged like him in fighting the same battle, using the same weapons, and bringing all their powers to insure the same result.

## THE ADDRESS IN SURGERY.

By JAMES SYME, F.R.S.E., Surgeon-in-Ordinary to the Queen in Scotland;
Professor of Clinical Surgery in the University of Edinburgh; Member of
the Medical Council; etc.

MR PRESIDENT AND GENTLEMEN,—It is said, with truth, that those only can appreciate the comfort of travelling by railway who have experienced the delay and fatigue of the old coaching system; and it is no less true that surgeons of the present day can hardly imagine the facilities they enjoy in discharging their professional duties, when these are compared with the difficulties experienced by their older brethren. Forty years having now elapsed since my first course of lectures on Surgery, I venture to hope that some account of the changes in practice which have taken place during this long period may not be without interest on an occasion that has afforded me the honour of addressing so many members of my profession.

But before proceeding further, I must entirely dissent from the opinion which was expressed by my respected friend who addressed you yesterday, that the progress of improvement implied a censure on those who had preceded it. On the contrary, I have always understood that there was nothing more creditable than the admission of error; and that every man, instead of being ashamed to do so, should be proud of taking a step in advance, whether he leads or follows.

Commencing with the treatment of inflammation and its consequences, I may notice a most remarkable difference between the old and present practice, in the almost entire disuse of bleeding instead of its nearly constant employment. On looking back, it is indeed difficult to realize the reckless and indiscriminate profusion with which blood was made to flow. When I was one of the dressers of the Royal Infirmary of Edinburgh, two of us went every evening, at a stated hour, to bleed the patients whose names were entered in a book, with the respective quantities due from each. On one occasion, I recollect of sixty-five ounces taken at once, and followed by thirty-five next day. At present, few surgeons carry a lancet, and still fewer ever employ it; so that venesection, instead of being the most frequent, has become one of the rarest operations in surgery. The reason of this is generally said to be a change in the type or condition of the human system; but may, I think, rather be attributed to the influence of more correct ideas in regard to the treatment of disease; since it is certain that operations no less bloody than those of the old time are now performed without any evidence of less ability to bear them.

Before being appointed house-surgeon of the Edinburgh Infirmary, I was medical superintendent of the Fever Hospital; and there, under the direction of the attending physicians, both of whom were Professors of the University, I bled men, women, and children, who were brown, emaciated, and reduced to the utmost degree of weakness. Afterwards, when house-surgeon to the Infirmary, I had under my care a boy who suffered from compound fracture of the leg, which gave rise to profuse suppuration; and, about three weeks after the injury, seeing that his strength was much exhausted, I ordered him some porter with beef-steak. But, next day, the surgeon, who was one of the most largely employed medical men in Edinburgh, disapproved of this, which, he said, would feed the disease, and directed me to take fourteen ounces of blood from the arm. I obeyed with great reluctance; and need hardly add that, before the end of forty-eight hours, the boy was dead. Now, I would ask, could any man at present think of bleeding in such cases as these? and, if not, then I say, that whatever change there may have been in the type, there certainly has been a change in the practice.

In treating the sinuses which remain after the evacuation of abscesses, a great improvement has long been established through the substitution of effectual drainage, instead of the means formerly employed to promote a healing action. These were sponge-tents, stimulating injections, and external pressure. Nothing

could be more absurd than the first of these, since their alleged use was to keep the orifice open, while, on the contrary, they effectually closed it, so that at every dressing the pent-up matter issued in a stream. The injections and pressure, though less hurtful, were equally useless and unnecessary; and there can be no doubt that, if recovery be not impeded by an unhealthy state of the system, by some morbid texture, or by the presence of a foreign body, nothing more is required than an aperture sufficiently free, and so situated as to prevent any accumulation of fluid in the cavity. The principle of drainage has been applied by M. Chassaignac to the treatment of chronic abscesses, through means of perforated India-rubber tubes; and, from my experience of this method, I can bear testimony to the advantage that attends its adoption.

The dressing of sores tending to heal has been greatly improved by substituting moist applications instead of the ointments previously employed; and any one who can recollect the old method of treatment by means of calamine cerate spread on lint or linen, with its pledgets of carded tow and long bandages, must rejoice in the simplicity, facility, and efficiency of our present system. Mr Liston, to whom we are much indebted for advocating this method, had great confidence in the " red lotion," as it has been called, and for which the original receipt will be found in the *Surgical Essays* by Mr Hey of Leeds; but, for my own part, I have long been satisfied that water alone is sufficient for the purpose.

At the time when I commenced practice, the callous or indolent ulcer, from which the labouring classes suffer so much, was treated by means of adhesive plaster and bandaging, not without much time, trouble, and expense. In 1829, I proposed a different plan, which was to apply a large blister over the swollen limb, in order to induce absorption of the indurating effusion, and allow the healing process to accomplish cicatrization under the ordinary treatment of a granulating surface. In Edinburgh, and by Edinburgh students scattered over the world, this method has been found invariably successful; but it is still, I have reason to believe, not generally known in the profession, and I am, therefore, glad to take this opportunity of recommending its adoption, as being the most speedy, easy, and lasting mode of affording relief.

The sores that result from the use of mercury were formerly very frequent, and always occupied many beds of the hospital. My recollection hardly extends to the dark period during which these cases were treated by administering the poison that had produced them, but well do I remember the shrieks of unfortunate patients who were subjected to the means deemed requisite for their remedy. When superficial and affecting merely the integuments, their surfaces were repeatedly destroyed by caustic potass; and when more deeply seated so as to expose the bone through opening of periosteal abscesses, they were condemned to scraping, rasping, and the actual cautery, or even amputation. Such cases are now comparatively rare, and in Scotland are hardly ever seen except through importation from the southern side of the Tweed, where the Hunterian doctrines with regard to the use of mercury struck their roots more deeply in professional confidence; but when they do present themselves they are found to yield readily under the application of blisters with small doses of the iodide of potassium. Through this simple treatment, I have repeatedly known patients who had come from distant parts of the world, prepared to suffer amputation, obtain speedy and complete relief.

The sloughing sore of old people, or senile gangrene as it has been called, used to be regarded as a disease no less hopeless than painful. Mr Pott had pointed out the impropriety of stimulating local applications, but it was still thought proper to support the patient's strength by wine and brandy. The last case thus treated that fell under my observation, was one which the late Sir George Ballingall and myself were requested to visit at some distance from Edinburgh, as no improvement had resulted from the prescriptions of Mr Liston, which we found to be two bottles of Madeira and half a bottle of brandy daily. The patient complained that his foot felt as if enclosed in a red-hot iron boot, but we had no alteration to suggest, and he died before long in

great agony.  It soon afterwards occurred to me that if the disease, as it cer-
tainly did, depended on an inadequate supply of blood through ossification of
the arteries, the limb must be nearly in the same state as after ligature of their
trunk, when it is well known that the effect of stimulants would be the excite-
ment of inflammatory action, and that the treatment therefore should be similar.
Under this impression, I tried the employment of a milk and farinaceous diet,
with simple poultices and opiates to relieve pain.  The result fully answered
my expectation, and in 1840 I published a paper on the subject, with the view
of showing that the disease might thus be rendered much less painful and fatal
than it had been under the stimulating plan ; and the anticipation thus expressed
has since then been fully realized.

There is nothing more worthy of notice in our present inquiry than the
change that has taken place with regard to the old system of dressing wounds,
which is painfully impressed on my recollection by personal experience ; when,
about fourteen years of age, I was thrown from a pony, and had my knee severely
lacerated, just above the patella, from which the integuments were torn down
so as to expose the bone completely.  A very experienced surgeon who came
to my assistance washed out some of the mud that lay in the cavity, and then
brought its edges together by straps of adhesive plaster applied so closely that
they overlapped each other, lint spread with ointment, and a roller six yards in
length completing the process.  Intense suffering and fever followed, with
urgent petitions for relief, but all in vain till the fourth day, when the dressings
were taken off and reapplied, as they were every day afterwards for six weeks.
The gentleman who attended me on this occasion had been house-surgeon of
the Edinburgh Royal Infirmary, and pursued the method then regarded as
proper in all cases of wounds, whether resulting from operations or otherwise
caused.  In 1826, I published a paper on the treatment of incised wounds, in
order to show the bad effects that necessarily proceeded from immediate closure
of the aperture, since when this was done there could not fail to be such a
quantity of blood or serum accumulated in the cavity as must effectually pre-
vent primary union ; and in support of this position I appealed to the fact, that
students passed through the whole of their hospital career without ever wit-
nessing an instance of healing by the first intention except in wounds of the
cheek or lips, where, there being two orifices, the blood could not be confined
between the raw surfaces.  To avoid this great evil, I advised that the edges
should not be brought together until the bleeding had ceased, and that then
there should be no impermeable covering placed over them.  The principle
which I thus endeavoured to establish is now, I believe, generally recognised
in practice.

There are few subjects of practical surgery that in recent times have excited
so much discussion as the mode of performing amputation.  When I entered
the profession it was the invariable practice in Edinburgh, and I believe else-
where, to operate by circular incision, even for removing the fingers and toes.
At former periods, various surgeons at home and abroad had not only proposed,
but to some extent adopted, the method by flap ; and M. Lisfranc, more than
forty years ago, taught it in his operative course on the dead subject as appli-
cable to all parts of the extremities ; but, so far as I saw, it was never then
employed in Paris on the living body.  While things were in this state, Mr
Liston became a very strenuous advocate of the flap operation, and by his
example, as well as teaching and writing, made a strong impression in favour of
this method.  I also published a paper on the subject, with the view of calling
attention to the advantages for which we contended, and more particularly the
saving of pain by rapidity of execution, together with the provision of a good
covering for the bone.  The expectations thus held out were fully confirmed
by experience with regard to the arm, fore-arm, and upper part of the thigh,
but led to disappointment in operating at the lower part of the thigh, and
through the leg.  In the latter situation it was found difficult to prevent the
flap from dragging the integuments in front and making them adhere to the
bone, so as to ulcerate, or even slough, and thus occasion a sore of the most

unmanageable character; while in the thigh the result was apt to be still more distressing, through retraction of the flaps and protrusion of the bone. For my own part, therefore, I have long abandoned the flap operation in the leg, and employed in its stead the circular method, by making two semi-lunar incisions from side to side through the integuments, and reflecting them to a sufficient extent for covering the bones, without any risk of retraction. I pursued the same course with regard to the thigh until a comparatively recent period, when I felt great pleasure in adopting the important principle which has been established by Mr Teale of Leeds and Mr Carden of Worcester, that a long anterior flap is not liable to retraction, so that it may be safely trusted for covering the bone. Amputation below the knee is seldom required, since all the diseases and injuries which were formerly held to demand it may, with few exceptions, be remedied by removing the foot at the ankle. This operation, when properly executed, without any of the complications that have been proposed for its alleged improvement, affords the most satisfactory results, by providing a perfect protection for the bones, by shortening the limb merely enough for fitting it with a boot, and by avoiding the risk of life attendant upon dividing the tibia and fibula through their shafts. Upon the whole, then, it would appear that amputation of the arm and forearm should be performed by a double flap, of the thigh by one long anterior flap, below the knee by two semi-lunar flaps of integuments, and at the ankle by a flap from the heel.

In treating diseases of the joints there have been many and great improvements, of which perhaps the most important is the substitution of rest for counter-irritation as a means of subduing the morbid action. The advantage of this is well illustrated by the different results obtained from the old and the present practice in cases of hip-disease. Within my recollection they were regarded as nearly, if not entirely, hopeless of recovery; and the caustic issues, always deemed proper for their treatment, were employed rather in compliance with established usage than with any expectation of a beneficial effect, the morbid process being expected to pursue its course until suppuration took place, and left no alternative to the patient but death from exhaustion, or escape from this danger with a distorted useless limb. Instead of the painful feelings with which the treatment of these cases was undertaken in those days, we now, through means of the "long splint," which, by preventing motion in any of the joints, maintains the one concerned in a state of absolute quiet, look forward with confidence to the accomplishment of recovery before the end of many weeks, unless the progress to destruction has advanced too far, and already led to suppuration.

There is another form of articular disease, in which the improvement has consisted not in the disuse, but in the greatly increased efficiency of counter-irritation. This is that which has been denominated ulceration of the cartilages, and is characterized by intense pain, aggravated through pressure or motion, and other symptoms well known to the gentlemen whom I have the honour of addressing. In addition to the suffering experienced by the patient from this disease, there is a great risk, or, indeed, almost the certainty, of suppuration and caries being ultimately induced by it, so that effectual means of remedy are of no small value. Bleeding, blistering, and caustic issues generally palliate the symptoms, but frequently fail to do so, and seldom, if ever, produce a decidedly curative effect. It was, therefore, with great satisfaction that, more than thirty years ago, having, on the authority of the late Professor Rust of Berlin, adopted the actual cautery for the treatment of this disease, I found there was thus obtained relief no less speedy than complete. The actual cautery, I believe, had then never been used in Great Britain for the purpose of counter-irritation, and for many years made slow progress in professional confidence, but is now, I fear, encountering a more serious difficulty from its too free and indiscriminate employment, which must tend to lessen the credit so justly merited in proper cases. In the most frequent form of articular disease, which proceeds from constitutional weakness, leading to scrofulous degeneration of the tissues—there being merely a colourless elastic swelling without pain or

other symptoms of inflammation—it seems difficult to believe that in such a condition local treatment of an active kind should ever have been deemed proper; and yet, within my recollection, leeches, blisters, and even stronger counter-irritation, were employed for it. The more correct ideas now entertained have, it is to be hoped, greatly lessened, if not altogether prevented, such improprieties, and rendered improvement of general health the great object of treatment, the local means being simply of a protective character.

Notwithstanding the improvements to which I have alluded, and the consequently more satisfactory results of our treatment, it still unfortunately too frequently happens that the disease of a joint terminates in suppuration and caries. But in this event also we have made an advance in many cases, by removing the affected bone instead of amputating the limb. The elbow-joint is the one that most frequently requires this procedure, is most conveniently situated for the purpose, and affords the most valuable result by preserving the arm and hand hardly if at all impaired as to either mobility or strength. The success attending the operation on this joint has led to its employment in other articulations, with success more or less complete. In cutting out the shoulder-joint, great difficulty has been experienced in removing the head of the humerus and glenoid cavity through the same incision; and I have therefore adopted a different plan, which renders the process extremely easy. This is to cut directly downwards from the coracoid process, so as to accomplish the first part of the operation; and then, if the other is found requisite, to make a second incision through the posterior side of the joint along the inferior costa of the scapula, which affords free access to the neck of the bone. I may here remark, that both the scapula and clavicle admit of being treated with great freedom, and even removed completely, without materially impairing the arm in its usefulness. The wrist-joint, from the complexity of important parts lying over it, has, until lately, been deemed an improper subject for excision; but Professor Lister, by methodically attacking the different bones concerned, has succeeded in establishing a procedure by which the most extensive caries in this situation may be removed without injury to any of the digital tendons, bloodvessels, or nerves, and with the effect of preserving a perfectly useful hand. In the year 1830, I cut out the knee-joint; and four years afterwards exhibited the patient in the most perfect state of recovery at a meeting of the Medical Section of the British Association for the Promotion of Science. It may be asked why I did not continue to employ this operation? and my reply would be, for three reasons; since I feared, in the first place, that in adults recovery would be very tedious; secondly, that in children the limb would be stunted in its growth; and, thirdly, that some small sinus or oozing of matter, so frequent after excision of the elbow, might prevent the body from having a firm and useful support. It appears that these anticipations have been only partially realized; and that, in favourable cases, the operation may be performed with a fair prospect of success. There still, however, remains the question as to expediency in choosing between the prospect of a result thus obtained and that of a comfortable stump with an artificial limb; and here, I regret to say, the discussion has been characterized by a degree of fervour that savours more of personal acrimony than a simple regard for the relief of suffering. For the future, I hope that the exercise of a free choice on this point will not expose any of us to unworthy imputations.

The cartilaginous bodies, which are so troublesome in the knee-joint, were formerly regarded as an unsatisfactory subject of treatment, on account of the inflammation that was apt to attend their excision; but the subcutaneous operation suggested by myself in 1841, and afterwards improved by Mr Square of Plymouth, has afforded a means of remedy both safe and easy.

With regard to the pathology of the osseous system, instead of the vague and erroneous ideas entertained forty years ago respecting the formation of new bone, we now possess a clear understanding of the source from which it proceeds. My probationary essay on necrosis was written with the view of showing that the periosteum had nothing to do with ossification; but, ten

years afterwards, I was led by the observation of some facts in practice to take an entirely different view, and to perform experiments which completely removed any doubt that could have remained on the subject. By detaching the periosteum of dogs, and either removing the denuded bone or surrounding it with tinfoil, I obtained results that proved beyond all question the periosteum to be the great agent of osseous reproduction. Of late years, much attention has been devoted to this subject in France, especially by M. Ollier, who came to Edinburgh with his preparations of rabbit bones, and was not a little surprised to find them anticipated by my own from dogs, which had been made and published twenty years before.

Our knowledge of the ossifying power which is possessed by the periosteum has improved the treatment of necrosis, and has led to other applications, of which the one most worthy of notice is that employed for the remedy of fissure through the hard palate, where, by detaching the periosteum and obtaining sufficient relaxation of the dense superjacent textures by means of lateral incisions, we are enabled not only to close the gap, but to do so by the formation of new bone.

In the early part of this century, a large share of attention was devoted to the suppression of hæmorrhage, which consequently became so perfect as hardly to admit of any improvement during the period at present under consideration. Mr John Bell, followed by Mr Guthrie, had established the great principle that bleeding should always be arrested by means applied at the seat of injury; and that if the access for this purpose were not sufficiently free, it should be enlarged by extension of the existing wound, rather than by making a new one. Dr Jones, by his ingeniously devised and carefully executed experiments, had fully explained the different steps by which the blood is spontaneously prevented from continuing to flow. The tenaculum had given place to the forceps, and small silk ligatures had been adopted instead of the bookbinders' twine previously in use. Some attempts have lately been made to throw discredit on the ligature, by attributing the most injurious effects to its action, and by proposing in its stead various contrivances of needles or wires, or a combination of both. Such proposals must be received with regret, as evincing an uneasy desire for innovation, and as calculated to confuse the sound principles of practice which have been established by men of experience and reputation. The truth is, that the ligature occasions no irritation, inflammation, or gangrene, as it has been said to do, and merely prevents union to the extent of its presence. Thus, when the femoral artery has been properly tied, the wound heals completely by the first intention, except at the point where the ligature lies, and from which a few drops of matter are discharged. Indeed, so far from regarding the ligatures as injurious, I believe them to be of great service, by maintaining an outlet for the discharge of fluids that might otherwise accumulate in the cavity; and therefore, instead of cutting off one of the threads, as was formerly the custom, always preserve both with a view to this effect.

The treatment of aneurism also seemed to have been rendered perfect by the operation of Hunter being applied to all the branches of the aorta, primary as well as secondary; but during the latter half of our forty years' period there have been symptoms of a revolutionary tendency, which has led to important alterations of practice. Of these may be first noticed a revival and improvement of the treatment by pressure as a substitute for the ligature, which we owe to our brethren of Dublin, and more especially Drs Hutton and Bellingham. There can be no doubt that by means of properly constructed apparatus, and with sufficient endurance on the part of the patient, aneurisms, and more particularly those of the femoral artery, may usually thus be remedied without any long delay. But, on the other hand, it cannot be denied that pressure sometimes fails, and may also be attended by unpleasant consequences; while it appears that, in so far as the femoral artery is concerned, the operation, if carefully and correctly performed, is nearly, if not absolutely, free from danger. I have done it in thirty-five cases, and never met with any bad

result, except on one occasion when an attempt had been made, without success, to accomplish the object by pressure ; and the sac suppurated without any blame being attributable to the ligature.   The method by pressure, therefore, while justly regarded as a valuable addition to the resources of surgery, should hardly be considered as a substitute completely superseding the operation.   The treatment of popliteal aneurism, by simply bending the knee, has frequently been successful, and seems the perfection of simplicity, but is by no means certain in its effect, and as I have had occasion to see, may prove injurious by rupturing the sac.   Various attempts have been made to induce coagulation by introducing foreign matters into the cavity, and of these the injection of perchloride of iron by M. Nélaton has been most successful. This plan seems most suitable for aneurisms affecting arterial branches of secondary size, and even here would appear to be not altogether free from serious danger, through its effect upon the circulating system.   In another way I have endeavoured to improve the treatment of aneurism, by showing that the artery at the seat of rupture is not necessarily unable to bear a ligature, and that, therefore, in cases where the Hunterian operation is either impracticable or beset with peculiar difficulty and danger, it may be better to open the sac and secure the bleeding orifices.

Passing from general to more particular considerations, we may begin with the head, where much has been done in the way of improvement.   The eyeball, instead of being roughly scooped out along with the contents of the orbit, is now delicately detached from its conjunctival covering and muscular attachments, so as to be removed with little disturbance of the neighbouring parts, which quickly heal, and, before the end of many days, may be able to receive an artificial substitute, except for the office of vision, in every respect perfect.   Fistula lacrymalis, which was formerly with justice regarded as an opprobrium of surgery, and admitted of treatment only by the clumsy expedients of tubes or styles permanently retained in the duct, is now, through the ingenious suggestion of Mr Bowman, no less easily than effectually remedied by slitting open the canal from the inferior punctum, so as to obtain room for the passage of probes sufficiently large for removing the obstruction. The distressing deformity of squinting, also, which was formerly deemed incurable, is now subject to an operation that, when properly performed, leaves nothing to be desired.   Then the different sorts of nasal polypus are accurately discriminated, and when proper for extraction, instead of being nibbled away by the repeated application of clumsy forceps, are attacked by instruments sufficiently small for being insinuated to the point of attachment, and thus removed entire.   Enlarged tonsils, too, no longer permitted to torment the patient by frequent sore-throat, impeded respiration, and husky voice, are readily removed by the simple means of a hook and knife, which are infinitely better than any of the complicated apparatus that has been contrived for this purpose.

But the most remarkable evidence of progress in this situation is afforded by tumours of the jaw-bones, which were formerly dug out from the centre towards the circumference without the slightest prospect of any better result than frightful deformity and increased activity of the morbid growth.   It was, therefore, a great improvement which accomplished removal by dividing the bone beyond the part affected, where it was known to be sound.   My attention was early directed to this subject by my respected friend, the late Mr Cusack of Dublin, who adopted the new method before it was employed in either England or Scotland; I had thus an opportunity of performing the operation upon a very remarkable case, after it had been dismissed from the Royal Infirmary of Edinburgh, and also by Mr Liston, as incurable.   Some years afterwards, on the 15th of May 1829, I removed the superior maxillary bone, for the first time in Great Britain, upon the same principle of cutting through the sound bone beyond the confines of the disease.   It would be difficult to estimate the number of lives that have been saved by these operations ; and I beg to express my hope, that no love of change or desire to act the part of

an improver may ever resuscitate the old system of operating, with its chisels and gouges, and abortive efforts to accomplish what can be done effectually only in another way.

Descending from the head, we come to the neck, where the first difference between old and present practice that presents itself is in regard to the operation for admitting air into the lungs. Within my recollection it was rarely performed, and still more rarely, if ever, with success, the reason of which was twofold. For, in the first place, the tube employed was so narrow that, independently of obstruction from mucus in its cavity, there was not space sufficient for the passage of air; and, secondly, instead of being introduced into the trachea, it was thrust between the cartilages of the larynx, too near the seat of disease for rendering any service. It is needless to say how different the case is now, and how frequently life is saved by the timely performance of tracheotomy. But as it may not be generally known that we are indebted to Mr Liston for the wide, conical, and slightly curved tube so generally in use, the circumstance which suggested its contrivance seems not unworthy of notice. This was the case of a gentleman nearly related to myself, who suddenly suffered from obstructed respiration, which, having resisted the ordinary means of treatment, urgently required an opening into the air-passage. Mr Liston, finding that the tubes in his possession were quite useless, cut off a portion from the extremity of his largest catheter and inserted it into the trachea. Soon afterwards a case of œdema glottidis occurred, and afforded time for making the requisite preparation, when tubes of the present form were contrived, and found to answer the purpose perfectly.

Cancer of the tongue has always been regarded as a very unsatisfactory subject for surgical treatment, on account of the extreme tendency which the disease has to a rapid return; but complete removal of the organ has been found productive of more lasting benefit than a partial operation; and, if supported by further experience, may perhaps be available for the relief of a condition otherwise so hopeless and distressing.

The treatment of wry-neck has been greatly improved by the introduction of tenotomy, which was employed for this purpose long before its application to club-foot. On 2d November 1832, in the Edinburgh Surgical Hospital, I operated by subcutaneous incision on a boy suffering from wry-neck with complete success, and the case so treated stands first in the records of British surgery. This I mention to account for the interest which I have taken in tenotomy, and the regret which I feel in seeing so excellent a means of affording relief to a large extent withdrawn from the ordinary practice of surgery by specialists, who, through the use of complicated and expensive mechanism, alleged to be requisite for the purpose, have been allowed, in no small measure, to appropriate its employment. But the members of our profession who tacitly sanction such an arrangement, and decline to practise tenotomy, may be assured that they must frequently withhold the assistance required when it would prove most useful, and by allowing the evil to gain strength through delay, afford occasion for the requirement of apparatus beyond the reach of those who suffer from poverty as well as deformity.

The thoracic region presents no more remarkable evidence of progress than that afforded by the method of treating serous cysts in the mamma, for which we are indebted to the late Sir Benjamin Brodie. Within my recollection, there was hardly any attempt to discriminate tumours of the breast; and all of them passing, as they did, under the title of scirrhous, were equally supposed to require removal of the whole gland. But even after the distinction had been drawn between those that were malignant and those that, being of a simple or local nature, did not require the knife to go beyond the confines of their own extent, the cysts containing serous fluid were still believed to demand excision, until the late distinguished surgeon showed that mere evacuation of the contents followed by a rubefacient applied to the surface constituted an effectual remedy. I can bear ample testimony to the success of this treatment; and would only suggest that, instead of the lancet and embrocation employed by

its author, a small trocar and blistering plaster will be found to facilitate the procedure.

The radical cure of hernia, so long an object of desire in the practice of surgery, has at length been to some extent attained by the method which Professor Wutzer of Bonn proposed some years ago; and the complicated apparatus originally employed having given way to more simple means calculated to produce the same effect, the procedure is now within reach of any one who chooses to execute it.

We now come to ovariotomy, which has of late been the subject of so much attention; and it will here, perhaps, be supposed that a claim for the honour of priority may be advanced on the part of Edinburgh, where the operation was first performed. But, to confess the truth, I fear that the northern metropolis, so far from deserving any credit on this account, should rather plead guilty to having invested the procedure with an aspect so repulsive as to impede rather than promote its adoption. It was brought forward by the same person who had proposed to remedy hypertrophy of the heart by blowing air into the pericardium, to puncture the brain in acute hydrocephalus, and to treat enlargement of the prostate by cutting out the entire gland; so that the profession in Edinburgh were not either disposed to adopt the excision of ovarian tumours or at all surprised by the results of its attempted performance. From these it appeared that one woman was laid open from sternum to pubes without any tumour being found; that another so treated presented a mass of disease entirely beyond the reach of removal; and that a third, after having what was supposed to be an ovarian tumour extracted, was found, on dissection a few days afterwards, to retain both ovaries in a healthy state. These, and similar cases so ludicrous, if they had not been so shocking, led surgical teachers to conclude that the operation laboured under three serious objections,— 1. The uncertainty of prognosis; 2. The difficulty of diagnosis; and, 3. The danger of execution: whence it happened that, notwithstanding more favourable reports that after a time reached us from Manchester, with the exception of a few cases, all of which proved fatal, no further attempt was made in Scotland to establish the procedure until a recent period, when the successful experience of some gentlemen in London, and more especially Mr Spencer Wells, gave the matter an entirely new position. The objections originally entertained with regard to both prognosis and diagnosis have been in a great measure removed through the careful discrimination of cases, while the operative procedure has acquired a corresponding degree of perfection; and the results are so satisfactory that the proportion of deaths does not exceed from thirty to thirty-five per cent. The most successful operator in Scotland is my friend and former house-surgeon, Dr Thomas Keith, who has operated in thirty-two cases, and lost only nine of his patients.

Descending to the pelvis, we find a great improvement in the treatment of hydrocele through the substitution of iodine for port-wine, which very frequently failed, and, when unfortunately allowed to enter the cellular texture, produced the most violent disturbance, constitutional as well as local, or even proved fatal. When there was no better alternative than this, it is not surprising that many surgeons clung to the method of incision, which even now, in some parts of the world, is still deemed the most expedient means of remedy; but every one who has witnessed the certain success and freedom from unpleasant effects which result from the injection of iodine cannot hesitate in preferring it to any other mode of treatment. In order to obtain the good effect in full perfection, it is necessary that the tincture of iodine should be of proper strength, such as that of the "Edinburgh Pharmacopœia;" that the contents of the sac should be completely evacuated, and that the fluid injected, which need not exceed two drachms, should be diffused over the surface by a rough shake. The advantage of this treatment is not limited to hydrocele, since it is equally efficacious for the remedy of all cysts containing albuminous fluid, such as those of the thyroid gland or other part of the neck, and also those met with on the trunk or extremities.

Forty years ago, diseases of the rectum, being very imperfectly understood, were regarded with no less horror by patients than apprehension by surgeons. Fistula in ano was believed to require division of the septum to its summit, however high up the bowel this might be, whence followed profuse bleeding, protracted dressing, and frequent failure, from the internal aperture not having been included. Internal hæmorrhoids, under the title of prolapsus, were viewed with especial dread, on account of the hæmorrhage resulting from excision, and the inflammation apt to be caused by partial or imperfect ligature. Fissures and ulcers, when recognised, which was seldom the case, were held to require a complete division of the sphincter; while an ample field for quackery was afforded by the belief that curable strictures existed high up in the colon. The state of practice is very different now, when fistula is easily and effectually remedied by an incision extending merely to the internal opening, and therefore so slight as not to require any dressing, or hardly any confinement; when internal hæmorrhoids, and all the discomfort of prolapsus, are removed no less safely than certainly by ligatures comprehending the whole disease, and tightly drawn; when fissures and ulcers are known to require merely an incision no deeper than their base; and strictures, whether malignant or simple, are ascertained to exist only within reach of the finger.

For removing stones from the bladder, many attempts have been made to improve the process by cutting, though with little success, since, I believe, most surgeons are satisfied that the operation as performed by Cheselden is still the best for the purpose. But, during the period under consideration, another method of affording relief has been devised, improved, and I may also say perfected, so as to render the knife of comparatively little value in the treatment of calculous complaints. Sir Astley Cooper's plan of extracting small concretions by means of curved forceps was soon followed by the introduction of straight tubes containing branches that expanded, and held the stone while it was acted upon by a central drill. This lithotrity, in its turn, gave way to the safer and more efficient procedure of lithotripsy, by which the calculus, instead of being attacked from the centre, was broken into fragments by external compression,—at first through the force of a hammer, and afterwards through that of screws differently applied. From the improvements which have taken place in these instruments, and the mode of using them, there seems reason to hope, that patients who apply for assistance before the disease has advanced too far may in general obtain relief without submitting to lithotomy, which, however well performed, must always be regarded in adults as much more formidable than crushing; while, in children, the absence of a developed prostate renders cutting perfectly safe.

During the last forty years, few surgical derangements have attracted more attention, or been the subject of more keen discussion, than stricture of the urethra. It would be no less tedious than unprofitable to review the controversies that have hence arisen; and it will be sufficient for my present purpose to consider the various modes of treatment under some general heads, to which they may be referred. These are,—1. Caustic; 2. Dilatation; 3. Internal incision and rupture; 4. External incision. But before inquiring into the merits of particular remedial measures, it may be proper to remark that two pathological facts, ascertained in recent times, have had an important bearing on their application. It was formerly supposed that strictures were frequently impermeable, and consequently limited with regard to the means of relief, but they are now known always to admit the introduction of instruments, if sufficiently small and properly guided. It was also supposed that the seat of contraction lay most frequently in the membranous part of the urethra, while we now know that it is almost always anterior to the bulb.

The treatment by caustic has been so generally abandoned, and labours under so many objections, that it need not detain us at present. The process of dilatation may be conducted in three different ways, each of which has its respective advocates, and which may be distinguished as the gradual, speedy, and sudden methods. The first is effected by the gentle passage of bougies at

considerable intervals of time, so as to induce absorption of the thickened texture that causes contraction; the second is accomplished by keeping a succession of gradually enlarged catheters in the bladder; and the third is completed at once by a sufficient amount of mechanical stretching. By the first of these methods, in the great majority of cases, perfect and more or less permanent relief may be obtained, while the two latter are apt to produce only a temporary advantage by leaving the texture in its original state, and ready to contract when relieved from distention.

The plan of remedying strictures by internal incision or rupture has called forth an infinite variety of ingenious contrivances for accomplishing the object in view. Of these, the instrument brought into use by Mr Holt appears to be the most efficient and safe when properly employed, but like others of a similar kind, labours under the objection of requiring previous to its use such a degree of dilatation as in general yields readily to the simple bougie. It also cannot ensure complete division of the contracted texture, as I have seen in a stricture at the orifice, where the largest instrument produced merely stretching of the part, and my experience would lead me to believe that a similar condition may exist at other parts of the canal.

The remedy of strictures by external incision has long seemed to me the best way of affording relief in cases not amenable to simple dilatation. It met with strenuous, I might almost say intemperate, opposition, but has kept its ground, and will, I believe, continue to prove useful in cases of peculiar obstinacy. In cases anterior to the scrotum, it is best executed by sub-cutaneous incision, and whether here or elsewhere, may be performed upon a director of the smallest possible size, which greatly adds to its value.

With regard to the female organs, the most remarkable change that has taken place in the way of improvement is in the treatment of vesico-vaginal fistula, which was formerly held to be nearly, if not altogether, incurable, and is now remedied no less easily than certainly through means of silver sutures, for the introduction of which we are indebted to Dr Marion Sims. But our American brethren have laid us under a still more important obligation by the grand discovery of etherisation, or the induction of insensibility by respiration of an ethereal vapour. To Drs Morton and Jackson of Boston we owe this procedure, which has so wonderfully facilitated the practice of surgery, and divested it of its most painful features.

In conclusion, Mr President and Gentlemen, I beg to express my hope, that from what has been said, surgery will not appear to have stood still or pursued a retrograde course during the last forty years, but, on the contrary, to have been improved in many important points of practice, and to hold out the prospect of further advance; so that when forty years hence some senior member of the Association shall take a similar retrospect, he will find no lack of materials for illustrating the march of progress.

---

## FORFARSHIRE MEDICAL ASSOCIATION.

THE seventh annual meeting was held in the White Hart Hotel, Arbroath, on Thursday, the 13th July. It was agreed to hold the next annual meeting in Montrose, and that the following be the office-bearers for the ensuing year:—President: George Steele, M.D. Vice-Presidents: David Johnston, M.D., and Matthew Nimmo, Esq. Secretary: J. R. Begg, Esq. Treasurer: Geo. C. Pirie, M.D. Council: Drs Gibson, James Arrott, Christie, Crockatt, J. A. Cowper, and J. W. Miller. Local Secretaries: J. A. Dewar, M.D., S. Lawrence, M.D., J. Guthrie, M.D., and G. P. Alexander, Esq.

## COMMISSIONS IN BRITISH MEDICAL SERVICE.

THE Director-General furnishes a List of the Candidates for Commissions in Her Majesty's British Medical Service, who were successful at the Competitive Examination in February last, and who have passed through a Course at the Army Medical School, showing the combined results of the Examination.

| Names. | Studied at | Marks. | Names. | Studied at | Marks. |
|---|---|---|---|---|---|
| V. Wearne, | Cork & Lon. | 5602 | W. O'B. White, | Dublin, | 3410 |
| R. Collins, | Glasg. Univ. | 4080 | R. H. Carew, | Dublin, | 3398 |
| F. O'Farrell, | Gal. & Dub. | 3995 | C. White, | London, | 3343 |
| S. Robertson, | Edinburgh, | 3979 | A. J. Clapp, | Dublin, | 3263 |
| W. R. G. Hinds, | Dublin, | 3940 | G. J. F. Evatt, | Dublin, | 3252 |
| A. Lewis, | Dublin, | 3937 | T. R. King, | Edinburgh, | 3200 |
| R. E. Hogan, | Dublin, | 3915 | J. Creed, | Dublin, | 3149 |
| G. B. Sanders, | Dublin, | 3910 | A. W. Shedden, | Edinburgh, | 3145 |
| G. Andrew, | Dublin, | 3737 | R. Keith, | Aberdeen, | 3117 |
| A. J. Belemore, | Leeds S. of M. | 3728 | A. W. Bate, | Dublin, | 3035 |
| W. F. Ruttledge, | London, | 3683 | G. F. Churchill, | Dublin, . | 2965 |
| F. Powell, | London, | 3653 | T. A. Pickering, | Dublin, | 2915 |
| W. Robertson, | Gal. & Dub. | 3635 | J. F. H. Richardson, | London, | 2915 |
| G. Traynor, | Dublin, | 3598 | J. W. Purefoy, | Dublin, | 2858 |
| J. E. Fishbourne, | Dublin, | 3563 | H. Waghorn, | London, | 2770 |
| R. F. Tobin, | Dublin, | 3530 | W. Wakefield, | Glasgow, | 2635 |
| R. Jackson, | Birmingham, | 3500 | G. White, | London, | 2532 |
| J. A. Campbell, | London, | 3435 | G. R. O'Toole, | Dublin, | 2525 |
| J. J. Hanrahaw, | Gal. & Dub. | 3435 | J. C. Thorp, | Dublin, | 2293 |

## PUBLICATIONS RECEIVED.

Barker and Edwards, — Photographs of Eminent Medical Men. Nos. 1 and 2. By T. H. Barker, M.D., F.R.S. Edin., and Ernest Edwards, B.A. London, 1865.

Braithwaites, — Retrospect of Medicine. Edited by W. and James Braithwaite, M.D. London, 1865.

Cruise, — The Endoscope as an Aid in the Diagnosis and Treatment of Disease. By F. R. Cruise, M.D., etc. Dublin, 1865.

Cutler, — Notes on Spa. By Thomas Cutler, M.D. Brussels, 1865.

Davis, — Parturition and its Difficulties. By John Hall Davis, M.D., etc. Second Edition, revised and enlarged. London, 1865.

Gastaldi, — Lake Habitations and Prehistoric Remains in the Turbaries and Marl-Beds of Northern and Central Italy. By Professor Bart. Gastaldi. Translated and Edited by C. Harcourt Chambers, M.A. London, 1865.

Hecker and Hirsch, — Die Grossen Volkskrankheiten des Mittelalters. By J. F. C. Hecker and Dr August Hirsch. Berlin, 1865.

James, — Climate of San Remo, etc. By M. P. James, M.D. London, 1865.

Lee, — Baths of Switzerland and Savoy. By Edwin Lee, M.D., etc. London, 1865.

Lee, — Baths of Nassau. By Edwin Lee, M.D., etc. London, 1865.

Medical Sciences, Half-Yearly Abstract of the : January to June 1865.

Médecine et de Chirurgie Pratiques, Nouveau Dictionnaire de. Vol. III. Paris, 1865.

Odling, — Course of Practical Chemistry arranged for the use of Medical Students. By William Odling, M.B., etc. Second Edition. London, 1865.

Ritchie, — Contributions to assist the Study of Ovarian Physiology and Pathology. By Chas. G. Ritchie, M.D., etc. London, 1865.

Sheppard, — Bathing: How to do it, When to do it, and Where to do it. London, 1865.

Shinkwin, — Lectures on Hydrophobia. By T. C. Skinkwin, M.D., etc. Dublin.

Strange, — The Restoration of Health, or the Application of the Laws of Hygiene to the Recovery of Health, forming a Manual for the Invalid and a Guide in the Sick Room. By William Strange, M.D., etc. London, 1865.

Sutro, — Lectures on the German Mineral Waters, and on their Rational Employment. By Sigismund Sutro, M.D., etc. Second Edition, carefully revised and enlarged. London, 1865.

Tanner, — The Practice of Medicine. By Thomas Hawkes Tanner, M.D., etc. Fifth Edition, enlarged and improved. London, 1865.

# Part First.

---

## ORIGINAL COMMUNICATIONS.

ARTICLE I.—*Graduation Address to the Gentlemen who obtained the Degree of M.D. in the University of Edinburgh,* 1st August 1865, *delivered by* PROFESSOR SPENCE, *Promoter.*

GENTLEMEN GRADUATES,—It falls to my lot, as Promoter of the Medical Faculty, to offer you our sincere congratulations on your success in obtaining professional honours.

I shall best discharge the duty which devolves on me by being as brief as possible; but some things connected with the ceremony in which we have been engaged remind me that great changes have been taking place,—changes affecting those whom I address, which I can hardly pass unnoticed. Hitherto the duty of the Promoter has been to pronounce a valedictory address to those who, having attained the highest professional honours which the University could bestow, were never again to appear before the Senatus. To-day, I only bid farewell to some of you; for though all have passed their trials and obtained degrees, yet for those who come under the new statutes the highest honours are reserved for a future period, and we will again have the pleasure of meeting them when these honours are to be conferred.

In addressing you to-day, I feel you deserve to be congratulated with more than usual fervour on your success, as it has been achieved under no ordinary circumstances. You have pursued your studies and passed your examinations under all the annoyances and difficulties incident to a transition period of our profession; and lest, emerging from the struggle with a vivid recollection of changes in courses of study and methods of examination, and of modifications announced only to be re-modified, and doubts and difficulties of various kinds, you might suppose that modern medical legislation was but

"A mighty maze, and all without a plan,'

I venture to ask you, ere we part, to take a calmer look at it from the point of view you have attained.

When great changes or improvements are to be introduced into a system which has been long in practical operation, we must expect disturbance. By a series of adaptations, the old system,

however faulty, had been got to work tolerably well, and we had got accustomed to it; so that even when a better system is introduced, we feel the inconveniences of change. The passing away of the old things attracts more attention at such a time and is less pleasing than the coming advantages of the new. At the early period of such a change as our profession is undergoing, the cases of individual hardship are great, and often so numerous that the new regulations require to be relaxed to meet them. Such alterations are apt to be made hastily, and being afterwards found not to harmonize with other parts of the system, require again to be altered or modified; and this, together with the difficulty of drawing a line of distinction between one special case and another, leads sometimes to the appearance of a want of any guiding rule or principle. But we must take a wider survey to judge justly. Remembering that scarcely any great general good can be effected without some suffering, we must forget our own temporary annoyances and embarrassments in the good to be effected; we must compare, as a whole, the state of the medical profession as it existed prior to the passing of the Medical Act, with the system that is now being inaugurated. Formerly there was no joint or uniform action, no regulating power, no sensorium—if I may so speak—transmitting guiding power to and receiving reflexly the sensations of the medical body; there was no uniformity in the requirements of the different licensing boards for titles apparently the same, no protection for their licentiates or graduates, nothing to prevent the veriest quack assuming a title he had no right to, nor any means by which the public could readily ascertain who were and who were not properly qualified practitioners; and there was no equality of rights to practise even amongst those who were properly qualified; for until a recent period no man, however qualified otherwise, could practise as a general practitioner in England unless he were a licentiate of the Apothecaries' Company. Now what is the case? What has already been accomplished? The medical profession has now a representative legislative body —the General Medical Council. The great principle of the new system is uniformity: one common standard of education for all entrants to the profession, leaving to those who desire to do so to take its higher honours as you have done; equality of rights to practise in all parts of the British dominions to all properly qualified practitioners; protection, by registration, of all titles legally recognised. Not, indeed, the power that some would desire, to prevent the public employing irregular practitioners; but what I for one think enough—protection to the profession against the fraudulent assumption of its titles, by affording a means to the public to ascertain, if it choose to do so, who are properly qualified, by reference to the medical registers as published from year to year. In carrying out the details of the new system, the great difficulty has been to legislate for a uniform plan of education and examination.

This is only what might have been anticipated, when we remember how various were the requirements of the different boards, and how conflicting are the opinions on these subjects; and it is the undecided legislation in regard to these which causes most of the annoyances which teachers and students alike feel at the present time.

Medical education and the method of examination best suited to test candidates form the problem of the day. Some important advances in the right direction have been already made. A preliminary examination is now imperative before the student can begin his medical career; and arrangements have been made by which he may pass his examinations in the collateral sciences of botany, natural history, and chemistry, at an early period of the curriculum, so as to leave his mind and time free for strictly medical studies. What still requires to be done is, to decide upon and arrange the course of purely professional study. It is most important that this should be settled speedily and definitely; but I believe that, to be well done, it must be done by looking at the question as a whole, not by piecemeal legislation, but by a thorough consideration and reconstruction of the curriculum, in relation to the extended nature of medical science. I have no intention of entering into details; because, though I hold strong convictions, derived from some experience in medical education, I feel I have no right to take advantage of my position here, to state opinions probably differing from those of some of my colleagues, nor yet to detain you further than to say, that my views point, 1st, To re-arranging the periods or terms of study; 2d, To regulating the order of attendance on the different courses required,—because, as some of these subjects are means to an end, I think they should be studied first, so as to prepare the student to follow intelligently the courses of instruction on the great practical subjects of medicine and surgery; and, 3dly, That consideration should be given to the question, whether, by dividing some of our classes into junior and senior divisions, as is done in other faculties, the teaching might not be simplified and much repetition saved.

I cannot, however, leave the subject of medical education without adverting to the danger of extreme views in regard to the relative value of different kinds of instruction. Some thirty years ago, the licensing boards, desirous of raising the status of the profession, by increasing the education required of candidates, added class after class, many of the courses being collateral sciences, till the pathway of knowledge was all but impassable from the luxuriant overgrowth. Now the recoil from that system of over-lecturing has carried public opinion to the opposite extreme, in favour of what is termed practical instruction. If names always correctly indicated things, there would be few dissentients from the opinion, and assuredly I should not be one of them. If, however, it be meant, as is often insinuated, that systematic lectures are useless as a means of conveying practical

instruction, or that the only thing the student requires is self-instruction, with such aids as he receives in the dissecting-room and hospital, then I object to the term of practical instruction in that sense as only partially correct, and to the idea given to the student as "a delusion and a snare,"—a snare all the more tempting that it is usually baited with the name of some great man in our profession who has risen to eminence in spite of disadvantages. Such exceptional cases may be always found, but they are exceptions, not rules for general legislation.

There is no more dangerous error than trying to elevate some subjects by depreciating others, instead of looking at all the parts of education as one combined whole, for then even the less practical subjects are seen to have their place and use. The poet, when comparing knowledge to the building of a temple, says truly,—

> "Nothing useless is or low,
>     Each thing in its place is best;
> And what seems but empty show
>     Strengthens and supports the rest."

A great deal depends on the method of teaching. Lectures can be made a means of practical instruction, if they be made demonstrative and suggestive rather than exhaustive. Again, in the purely practical departments, we must also beware of over-teaching, helping the student too much, instead of merely superintending and aiding him in difficulties. The great power in self-taught men is *self-reliance*, derived from their having had to master difficulties, to observe for themselves, and make the knowledge so thoroughly their own that its application comes intuitively; and it should be the aim of all teaching to foster such habits of observation.

The nature of examinations for professional qualifications or honours, and the method in which they should be carried out, are evidently subjects of great importance. Examinations are the tests of the student's attainments, and the guarantees to the public of his fitness for entering on the practice of his profession.

The general principles on which examinations should proceed are, I think, simple, and may be classed under three heads:—*First*, They should have a relation to the means of instruction afforded to the student. We have no right to expect more from him than he is taught; and therefore it seems clear to me, that equal means of imparting instruction should be demanded from all recognised teachers, in order that the examinations for the same qualification may be uniform. *Secondly*, In arranging the order in which the different subjects of the curriculum are to be examined on, those sciences which form the foundations of medicine and surgery should be taken in the earlier examinations; whilst the practical departments of medicine and surgery should constitute the subjects for the final examination, which testifies to the fitness of the candidate to enter upon practice. *Lastly*, Examinations ought to be complete and testing in proportion to the value of the qualification to be awarded,

so as to maintain that value. Tests for university degrees and honours should be thorough, and their requirements high. The honour of our University and the interests of you its graduates alike demand this; for, depend upon it, the public look to the guarantee; and every candidate who manages to obtain a degree without being sufficiently fitted for it, whilst it does himself no real good, diminishes the value of the honour in public estimation, and so does injury to all who possess it. Whilst, however, I think examinations should be searching, and that we have a right to expect from the candidate such a knowledge of great general principles and their application, and also of ordinary surgical manipulations, as shall enable us to certify that he is fit to practise intelligently and safely, we have no right to expect him to be possessed of that peculiar tact in diagnosis and treatment of disease which experience alone can give. Any medical man, who reflects how gradually he has himself acquired this kind of practical knowledge, will best estimate the folly of expecting it from a candidate at his examination.

A considerable number of those whom I now address have, under our new statutes, obtained not only a medical but a surgical degree, that of " Master of Surgery :" the title is an old and honourable one revived; but let us remember, that it was held in estimation formerly just because it was an honour awarded after a searching practical examination—" the master-proof." We should see to it, that in granting this surgical degree now, that, to use the words of the famous French surgeon, Dionis, " we do nothing to diminish its ancient splendour," but that we do our best to give it the same high value in public estimation which the medical degrees of this University have always held.

There is one thing, before I leave the subject of examinations, in regard to which I am anxious to disabuse the minds of students, because I feel it operates injuriously on their appearances before examiners,—I mean, an unfounded dread of the nature of examinations; and let me say frankly of examiners, I have now had the honour of examining at all the medical boards in this city, and I can most confidently state, that whilst the gentlemen who compose these boards properly act under a strong feeling of the responsibility which their duty involves, I have never witnessed anything but the greatest fairness and consideration towards the candidates; and even when, in the interest of the public and the profession,—ay, and of the true interests of the candidate himself, it became their duty to remit, it was always evidently an unpleasant necessity, which all would have liked to avoid if possible; so that if examiners be not infallible, at least, believe me, that " even their failings lean to mercy's side."

I come now, Gentlemen, to that part of my duty which requires me to take a view of your professional prospects, and to tender you advice as to how these prospects may be best advanced.

There is an anecdote told of the great John Hunter (with what truth I know not), that, coming to his lecture one evening, and looking round on the crowded benches, he was so struck by the thought of what was to become of so many medical men, that, instead of the usual introductory "Gentlemen," he greeted his astonished audience with the involuntary exclamation — "God help you." Let us hope the prayer was heard. At least this we know, that though our profession has increased tenfold since then, somehow it has flourished. The medical profession cannot, indeed, be held up to you as one in which great wealth is to be acquired. It is one whose members must expect to meet with difficulties and anxieties almost unknown in any other profession, and these difficulties and anxieties greatest at the outset. But whilst it would be impossible to say what may be the success of any one man, as circumstances may do much to advance one or retard another; yet, looking back on the position of most of the medical men I have known, I would say our profession affords a fair average of success in life, and possesses so much of intrinsic interest for those who follow it, that I have found few who would be willing to change it for any other. If, indeed, you are ambitious of fame, I can hardly bid you hope to reap that dubious harvest here, although, even as affecting the general and political interests of society, the influences of our profession are beginning to be recognised, and that not only in matters sanitary. One of our modern essayists, a shrewd and keen observer of men and things, in an article on "Doctors," says, "It would be a very curious and a very subtle subject for inquiry, to investigate the share of doctors in the political education of society. The men who go everywhere, mix with all ranks and gradations of men, talk with each of them on the topics of the day, learning how class and condition influence opinion and modify judgments, must gain an immense insight into the applicability of any measure, and into its bearings on the different gradations of society. With this knowledge, too, they must be able to disseminate their own ideas with considerable power, and enforce their own opinions by arguments derived from various sources, doing these things not through the weight and power of a blind obedience, but by force of reason, by the exercise of a cultivated understanding, aided by especial opportunity." I quote this, Gentlemen, to show how much of general influence you may exert; but, apart from these things, the medical profession offers scope enough for the aspirations of any well-constituted mind in its own proper field,—a field wide as the prevention or relief of human suffering.

In regard to advice, I shall confine myself to a very few subjects, and treat them shortly.

I understand that what our French neighbours call a "specialité," in the addresses of surgical professors, has been the subject of matrimony. Whether medical men should marry early or wait

till they had attained a position?   To me it seems that the subject, however interesting for discussion, is scarcely fitted for advice.

> "Can love be controlled by advice,
> Will Cupid our maxims obey?"

I suspect not, for we are told, "Love rules the camp, the city, and the grove;" and, as even academic groves have no exemption from his sway, I shall trouble you with no counsel of mine, but leave the interesting topic to some fairer advocacy.

In regard to professional matters, I trust I need say little to inculcate the necessity for continued exertions in advancing your professional knowledge. It is, however, in the earlier years of your professional life that you will have most time to spare, and most energy to give to scientific investigations; and it is now also that you should lay the foundations of habits which in your busier after-life you will find of incalculable value. Amongst these, I would especially allude to the habit of methodical observation, and taking notes of the cases coming under your care; this, like other habits, will become a second nature, and prevent you becoming mere routine practitioners. Before settling in practice, you should, if possible, visit other schools of medicine, and note and contrast their teachings and practice with what you have learned and witnessed here; and though I believe you will learn by contrast in many things to value more highly the school where you have studied, yet you will surely add to your stock of knowledge, and enlarge your ideas; for I suspect that the man who can travel from Dan to Beersheba, and say that all is barren, must himself be afflicted with mental sterility. There is another important habit I would urge you to cultivate early and maintain through life—I mean a wise economy of time. It has been said that the men who have amassed the largest fortunes were not those who made them by some lucky ventures, but those who, knowing the power of "littles," looked after the pence as well as the pounds; and it would be well if we all looked after and usefully improved the odd half-hours we so often squander, on the plea that there's little use in beginning to do anything for so short a time. Amongst the miscellaneous sayings of the dying laird of Dumbiedykes, there was one worldly-wise advice he bequeathed his son, which, I think, has a moral application:—"Jock," said the dying laird, "whan ye hae naething else to do, *ye may aye be stickin'* in a tree; it'll be growin', Jock, whan you're sleeping." So I would say to you: in your leisure hours, when you have nothing else to do, you may always be sowing some seed of thought, or doing something which, by its own inherent vitality, will grow almost as imperceptibly, and with as little labour on your part, as the tree; and which in due season will flourish and bear fruit as certainly. Do not suppose, however, that I mean you to be always working at professional subjects. On these, as your proper business, your energies should

be mainly concentrated; but absolute concentration, or rather limitation, of the mind to one subject is not a healthy state of matters, —it is unfavourable to extended views or vigorous action; it is to the mental powers what selfishness is to the moral feelings, and, like it, tends to contract, not to expand. The leisure hours spent in the cultivation of a healthy literature, or in enjoying the beauties of nature or art, are hours well spent, for they enlarge and elevate the mind, cultivate the taste, and tell advantageously even on your professional pursuits.

It would be strange if, when considering how we may best improve time, I omitted to press on your consideration the highest and the truest of all knowledge,—that which is profitable not only for time, but eternity. It would, I say, be a strange omission at any time, but strangest of all when addressing those whose profession is to bring them constantly into contact with the sufferings of our frail humanity, and who, in the sickroom and by the deathbed, will be called to witness what a " summing up at a discount" this earthly life would be but for the grander hopes of the life beyond.

It has been well said, " that no science is complete which does not include the knowledge of the living God ;" and experience has often proved that when that is wanting nothing else can fill the void. One of the most eminent philosophers of modern times, whose whole career was a brilliant success, has, in his " Last Days of a Philosopher," left us this on record :—" I envy no quality of mind in others, no genius, wit, or fancy; but if I could choose what would be most delightful, and I believe most useful to me, I should prefer a firm religious belief to every other blessing; for it makes life a discipline of goodness, creates new hopes where all earthly hopes vanish, and throws over the decay, the destruction of existence, the most gorgeous of all lights, calling up the most delightful visions, where the sensualist and the sceptic view only gloom, decay, and annihilation." Yes, Gentlemen, with all your gettings, get understanding,—that true knowledge of God which will be not only a regulating principle, but a living power pervading all your daily life and actions, and which, far from hindering, will act as the most mighty impulse to energetic work ; for no diligence in business can compare to that of the man whose fervent zeal and willing labour have their springs deeper than the ordinary excitements to human action in the never-failing motive—" serving the Lord."

ARTICLE II.—*On the Mortality of Childbed as affected by the Age of the Mother.* By J. MATTHEWS DUNCAN, M.D., F.R.S.E., Lecturer on Midwifery, etc., etc.

IN describing my investigations of this topic I may be allowed to be somewhat brief and elliptical, because many reflections and limitations given in my paper on the mortality of childbed as affected by the number of the labour, are obviously applicable to this discussion also. As in that paper, so I shall here, first consider how age affects a special kind of mortality in childbed, that from puerperal fever, or metria.

### The Relation of Age to the Mortality from Puerperal Fever.

On this subject, as well as on all that follows, important information is to be found in a letter addressed by Dr Farr to the English Registrar-General, and published in the appendix to that officer's seventeenth annual report:

"What (says Dr Farr) is the danger of death by childbirth among women of different ages who bear children during the year? This is a different question; which is of practical importance both in medical science and in the business of life-insurance. The defect in the English schedule, which, as yet, contains no column for the ages of the parents of the children registered, renders it impossible to answer this question with precision. It will, however, be useful to obtain an approximate answer; and this we have been able to give by determining the probable proportion of women who bear children at each age, from the Swedish returns, and by applying the fraction expressive of this proportion to the English women living in 1851 at the corresponding age, the probable number of them who became mothers every year is determined. The total number thus determined for the year 1851 is 609,845; while the actual average number of the births in the seven years by the returns was 603,045. It is thus evident that the estimate differs to no great extent from the facts, and it may be assumed that the births corrected for twins, triplets, and still-born children, in England, would represent nearly 609,845 child-bearings."

The following table, extracted from the data supplied by Dr Farr, shows the mortality from puerperal fever in four decenniads :—

TABLE I.—*Showing the Mortality of Child-bearing Women from Puerperal Fever, in England, at Four different Ages.*—(*Farr.*)

| AGES. | 15–24. | 25–34. | 35–44. | 45–54. |
|---|---|---|---|---|
| Child-bearing women, . . . | 107,440 | 328,720 | 166,140 | 7545 |
| Deaths from puerperal fever, | 298 | 486 | 256 | 12 |
| Per-centage, . . . . . . | ·277 | ·148 | ·154 | ·163 |
| Or, one in every . . . . . | 360 | 676 | 649 | 628 |

The large figures in this table give great value to the result, that while child-bearing women aged from 15 to 25 do die of puerperal fever in a proportion far exceeding that of women at any other age, the child-bearing women aged from 25 to 35 are carried off by the same disease in the lowest proportion compared with all others. Puerperal fever mortality at its lowest among the lying-in aged from 25 to 35, rises on either side of this age, but it rises far more quickly and highly as age decreases than as age advances.

It would be unphilosophical to draw from this table even a presumption as to the influence of age on puerperal mortality, until careful consideration has been made of all the influences besides age, which may have a bearing on it. Now, as far as I know, the paramount influence interfering with deductions from this table as to the influence of age is that of the number of the labour. Of the influence of primiparity, both Dr Farr, and Dr Tyler Smith (Manual of Obstetrics, chap. xlviii.), have had some degree of appreciation. But Dr Hugenberger has, in some data he has published, actually separated the primiparous from the multiparous, with a view of eliminating this great influence. I here produce the tables of Hugenberger, re-arranged for uniformity's sake :—

TABLE II.—*Showing the Mortality of Primiparæ, of different Ages, from Puerperal Fever, in the Midwives' Institute of St Petersburg.—(Hugenberger.)*

| AGES. | 15–18. | 19–22. | 23–26. | 27–35. | 36–45. |
|---|---|---|---|---|---|
| Child-bearing women, . . . | 147 | 859 | 711 | 495 | 41 |
| Deaths from puerperal fever, | 7 | 25 | 22 | 39 | 4 |
| Per-centage, . . . . . . | 4·76 | 2·91 | 3·09 | 7·88 | 9·75 |
| Or, one in every . . . . . | 21 | 34 | 32 | 13 | 10 |

TABLE III.—*Showing the Mortality of Multiparæ, of different Ages, from Puerperal Fever, in the Midwives' Institute of St Petersburg.—(Hugenberger.)*

| AGES. | 18–22. | 23–26. | 27–35. | 36–53. |
|---|---|---|---|---|
| Child-bearing women, . . . | 503 | 1410 | 2967 | 903 |
| Deaths from puerperal fever, | 11 | 29 | 74 | 27 |
| Per-centage, . . . . . . | 2·18 | 2·05 | 2·49 | 2·99 |
| Or, one in every . . . . . | 46 | 48 | 40 | 33 |

These tables are interesting, and seem to show that Hugenberger felt the necessity, in the study of the bearing of age on puerperal fever mortality, of separating primiparæ from multiparæ. Any special results which might be drawn from them, I think little worthy of consideration, in deference to the much larger and more valuable data which I adduce, and on account of the extraordinary mortality which the tables reveal.

It may with truth be said that, to make a perfectly satisfactory comparison of the mortalities of women of different ages, it is necessary to compare with one another masses of women of different ages in each successive pregnancy. I know of no data for this purpose. Hugenberger's data of primiparæ are a poor instalment, and my own Edinburgh and Glasgow data are equally insufficient, and I do not think it necessary to encumber this paper with them.

I have, however, ventured to increase the value of Farr's data, with a view to the question of the influence of age, by the following method. In Table IV. the result is given.

TABLE IV.—*Showing the Mortality of Child-bearing Women from Puerperal Fever in England, at Four different Ages, corrected for Primiparity.*[1]

| AGES. | 15–24. | 25–34. | 35–44. | 45–54. |
|---|---|---|---|---|
| Child-bearing women, . . . | 107,440 | 328,720 | 166,140 | 7545 |
| Deaths from puerperal fever, corrected for primiparity, } | 194½ | 399½ | 256 | 12 |
| Per-centage, . . . . . . | ·181 | ·121 | ·154 | ·159 |
| Or, one in every . . . . | 552 | 823 | 649 | 629 |

The correction for primiparity is made because the puerperal fever mortality after first labours is at least double the puerperal fever mortality of all other labours taken together. In order to remove entirely, or almost entirely, the disturbing influence of primiparity, then, it is necessary to turn out of the data one-half of the deaths of primiparæ. The number of puerperal fever deaths of primiparæ at different ages is got by determining their probable proportion from the Edinburgh and Glasgow mortality of 1855.[2]

[1] It is to be remarked that this and the following tables corrected from Farr's data, give results for different decenniads that may be compared only with one another. The table would not give actual values even were Farr's data actual values, which they are not. Correction has been made only in the line of deaths, by taking away one-half of the deaths of primiparæ. This makes the table read as if a table of multiparæ. This proceeding, being simpler, has been preferred to another, which might have been followed, namely, to extract from the mothers the whole primiparous by estimate, and to extract from the deaths those of primiparæ, and compare the remaining multiparæ and deaths of multiparæ.

[2] TABLE V.—*Showing the Mortality from Puerperal Fever of Primiparæ in Edinburgh and Glasgow in 1855.*

| AGES. | 15–19. | 20–24. | 25–29. | 30–34. | 35–39. | 40–44. |
|---|---|---|---|---|---|---|
| No. of primiparæ, . . . | 331 | 1859 | 1007 | 354 | 134 | 33 |
| Deaths by puerperal fever, . | 2 | 14 | 7 | 3 | ... | ... |
| Per-centage, . . . . | ·604 | ·753 | ·695 | ·847 | ... | ... |
| Or, one in . . . . | 165 | 133 | 144 | 118 | ... | ... |

The preponderance of primiparæ at the earlier ages renders this correction necessary, and I only regret that the smallness of the data prevents us from ascribing to the correction a high value.

It may with truth and with some cogency be said that Farr's table should be farther corrected for the increased mortality accompanying ninth and subsequent pregnancies which fall into the more advanced ages. I do not attempt this correction, because it cannot, with the means at my disposal, be done satisfactorily. But the omission of this correction will, comparatively, cause little inaccuracy in the results drawn from the table; for births in ninth and subsequent pregnancies are proportionally few, and the average age of women in ninth and subsequent pregnancies is above 37 years, an age before which the injurious influence of elderliness appears to have already shown itself.[1]

It has, lastly, only to be remarked that reference to Table IV., corrected, as it is, for primiparity, shows results still closely resembling in general features those derived from the uncorrected table. Though similar in general features, there is evidently great difference in the numerical variations in the two tables (I. and IV.), and I think there can be no doubt that the last table (IV.) gives an approximation to a view of the influence of age far more faithful than the first (I.)

### *The Relation of the Age of the Mother to the Mortality accompanying Parturition.*

The first table which I shall adduce under this head is extracted from the data of Dr Farr, already referred to. The calculations, as made by Dr Farr, give the mortality according to age, but, since primiparous females are included in the lists, they are of little value as indicating the influence of age. I have, as in the table of puerperal fever deaths, corrected Dr Farr's data for primiparity, and in the penultimate line given the per-centages; which may be

[1] TABLE VI.—*Showing the Number of Children born in First and subsequent Pregnancies in Edinburgh and Glasgow, in 1855, and the Average Ages of the Mothers in each successive Pregnancy.*

| No. of Pregnancy. | No. of Children. | Average Age of Mother | No. of Pregnancy. | No. of Children. | Average Age of Mother. |
|---|---|---|---|---|---|
| 1st, | 3722 | 24·6 | 11th, | 152 | 39·2 |
| 2d, | 2893 | 26·2 | 12th, | 61 | 40·0 |
| 3d, | 2534 | 27·6 | 13th, | 34 | 41·7 |
| 4th, | 1982 | 29·9 | 14th, | 11 | 42·4 |
| 5th, | 1543 | 31·5 | 15th, | 6 | 42·7 |
| 6th, | 1221 | 32·9 | 16th, | 2 | 48·5 |
| 7th, | 848 | 34·9 | 17th, | 2 | 41·5 |
| 8th, | 611 | 36·1 | 18th, | 1 | 40·0 |
| 9th, | 425 | 37·5 | 19th, | 1 | 48·0 |
| 10th, | 222 | 38·8 | | | |

held as showing, when compared one with another, an approximate estimate of the influence of age on the mortality of parturition.

TABLE VII.—*Showing the Mortality of Child-bearing Women in England at Four different Ages, and the same corrected for Primiparity.*

| AGES. | 15–24. | 25–34. | 35–44. | 45–54. |
|---|---|---|---|---|
| Child-bearing women, . . . . . | 107,440 | 328,720 | 166,140 | 7545 |
| Deaths, . . . . . . . . . | 718 | 1397 | 1051 | 66 |
| Per-centage, . . . . . . . . | ·668 | ·425 | ·633 | ·883 |
| Deaths corrected for primiparity, . | 473 | 1216 | 1033 | 66 |
| Per-centage, . . . . . . . | ·440 | ·369 | ·621 | ·875 |
| Or, one in every . . . . . . | 227 | 270 | 160 | 114 |

Here the large figures give a proportionate value to the results. The women aged from 25 to 34 have the fewest deaths among them, namely ·369 per cent. They are more fortunate than the very young women aged from 15 to 24 by ·07 per cent. But the seeming influence of this youthfulness in aggravating the danger of women is slight when compared with that of advancing years, the decenniads following that from 25 to 34 showing a mortality increasing in a far higher ratio.

The next table which I adduce is made from data furnished by Dr Collins' Practical Treatise.

TABLE VIII.—*Showing the Mortality of Child-bearing Women in the Dublin Hospital during Dr Collins' Mastership.*

| AGES. | 15–19. | 20–24. | 25–29. | 30–34. | 35–39. | 40–44. | 45–49. | 50–54. | Total. |
|---|---|---|---|---|---|---|---|---|---|
| No. of mothers, . | 762 | 4862 | 5309 | 3817 | 1210 | 397 | 22 | 6 | 16,385 |
| Deaths, . . . | 10 | 38 | 41 | 41 | 26 | 4 | ... | ... | 160 |
| Per-centage, . . | 1·312 | ·781 | ·772 | 1·074 | 2·148 | 1·007 | ... | ... | ·976 |
| Or, one in every | 76 | 128 | 129 | 93 | 46 | 99 | ... | ... | 102 |

It is not corrected for primiparity, and I bring it forward for its own value, and because it gives the data for periods of five years. It will be observed that its results agree in the main with those derived from the data of Dr Farr. But a further step is attained by the five-year divisions showing that youthfulness is influential chiefly below 20 years, and increasing age not until 30 years are passed. These results are, in my opinion, however, somewhat modified by the data which follow.

The next table contains only multiparæ. Primiparæ are excluded; there is therefore no correction to be made. Further, it is not estimated; the figures all show actual values.[1]

---

[1] To complete the view of the mortality of child-bearing women in Edinburgh and Glasgow in 1855, I here give the mortality of the primiparæ. It is not placed in the text because of the smallness of the figures compared with those

TABLE X.—*Showing the Mortality of* Multiparæ *at different Ages, in Edinburgh and Glasgow in* 1855.

| Ages. | 15–19. | 20–24. | 25–29. | 30–34. | 35–39. | 40–44. | 45–49. | Total. |
|---|---|---|---|---|---|---|---|---|
| Multiparæ, . . | 45 | 1829 | 4030 | 3496 | 2273 | 807 | 92 | 12.572 |
| Deaths in ditto, . | ... | 13 | 21 | 39 | 20 | 8 | 2 | 103 |
| Per-centage, . . | ... | ·711 | ·521 | 1·115 | ·879 | ·991 | 2·174 | ·819 |
| Or, one in every . | ... | 140 | 192 | 89 | 113 | 100 | 46 | 122 |

It is worthy of remark that this table of actual values shows a minimum of mortality at the age of from 25 to 29 years.

The last table to be given is a composite one, but appears to me to be of value with reference to the present topic. It is made up as follows:—

TABLE XI.—*Showing the comparative Mortality of Child-bearing Women at different Ages.*

| Ages. | 15–19. | 20–24. | 25–29. | 30–34. | 35–39. | 40–44. | 45–49. | 50–54. |
|---|---|---|---|---|---|---|---|---|
| Mothers (Collins). | 762 | 4862 | 5309 | 3817 | 1210 | 397 | 22 | 6 |
| Deaths ⎰ corrected for pri-miparity, ⎱ | 10½ | 59 | 89 | 91½ | 52¾ | 15 | 1 | ... |
| Per-centage, . . | 1·3 | 1·2 | 1·6 | 2·3 | 4·3 | 3·7 | 4·5 | ... |
| Or, one in every . | 72 | 82 | 60 | 42 | 23 | 26 | 22 | ... |

With the number of women confined at different ages in the Dublin Hospital under Dr Collins, are compared the deaths at different ages, recorded by Drs Collins, M‘Clintock and Hardy, and Johnston and Sinclair. These deaths have been collected with considerable care to insure a close approach to their true number. All the deaths were among women delivered in the same hospital, and they are compared with a set of women also delivered in it. The deaths are corrected, as in former tables, for primiparity. The resulting per-centages are of course of value only when compared with one another, and in this respect they appear to me to be very valuable.

of multiparæ, and because the table of multiparæ is more like the others in the text.

TABLE IX.—*Showing the Mortality of* Primiparæ *at different Ages, in Edinburgh and Glasgow in* 1855.

| Ages. | 15–19. | 20–24. | 25–29. | 30–34. | 35–39. | 40–44. | 45–49. | Total. |
|---|---|---|---|---|---|---|---|---|
| No. of primiparæ, | 331 | 1859 | 1007 | 354 | 134 | 33 | 4 | 3722 |
| Deaths of ditto, . | 4 | 24 | 13 | 8 | 1 | ... | ... | 50 |
| Per-centage, . . | 1·208 | 1·291 | 1·291 | 2·260 | ·746 | ... | ... | 1·343 |
| Or, one in every . | 83 | 77 | 77 | 44 | 134 | ... | ... | 74 |

The smallest mortality is seen to be in the age 20–24; and the increased mortality from greater youthfulness is in the next quinquennial period less than the corresponding increase on the other side from greater age.

Looking over these tables, one cannot doubt that the result of Farr's data, showing 25 to 35 as the age of smallest mortality, may be considered as justly supplanted by the results of the tables showing quinquennial periods. In all of these the smallest mortality is found to be under 30 years of age. Of the tables showing quinquennial periods, given in the text, the 8th and 11th give 20–24 as the safest age for parturition, while the 10th gives it as 25 to 29; and we may guess with considerable assurance that the age of minimum mortality from parturition is at or near 25 years.

The following are the chief conclusions deducible from the whole exposition :—

1st, Youthfulness has less influence in producing mortality from parturition than elderliness.

2d, From the earliest age of child-bearing there is a climax of diminishing puerperal mortality, succeeded by an anti-climax of puerperal mortality increasing till the end of child-bearing life.

3d, The age of least mortality is near 25 years, and on either side of this age mortality gradually increases with the diminution or increase of age.

4th, Above 25 years puerperal mortality increases at a much higher rate than it increases at corresponding periods below 25 years.

5th, Though it is not deducible from anything in this paper, it is too interesting to omit noticing that the age of greatest safety in parturition coincides with the age of greatest fecundity, and that during the whole of child-bearing life, safety in parturition appears to be directly as fecundity, and *vice versa*. (See Transactions of Royal Society of Edinburgh for 1864, p. 488.)

---

ARTICLE III.—*Case of Traumatic Rupture of the Uterus; Gastrotomy thirty Hours after the Accident.* By JOHN MOIR, M.D., F.R.C.P. Ed., Consulting Physician, Edinburgh Royal Maternity Hospital, etc., etc.

RUPTURE of the uterus is an accident fortunately of not very frequent occurrence, but one which, when it does happen, involves so much risk to both mother and child, that it is a point of the first importance to ascertain and carry out the practice most likely to secure the safety of one or both of them.

In reporting a case which lately came under my care, I have no intention of entering on the subject at large, referring such as are

desirous of studying it more fully to a monograph on the Causes, Symptoms, Pathology, and Treatment of this accident, by Dr Trask of New York, in the American Journal of Medical Science for the years 1848 and 1856.

Dr Trask, in this admirable and exhaustive paper, has collected the history of upwards of 400 cases; and after shortly giving the opinions of various writers as to its appropriate treatment, remarks that there is among them on this subject a great variety as well as contrariety of sentiment.

After careful consideration, Dr T. has adopted the conclusion that, in cases where the fœtus has passed into the abdomen through the rent of the uterus, the correct principle of practice is not to leave the patient to the efforts of nature, but to accomplish delivery. He says, " We have shown, we think conclusively, that non-interference affords a patient a far inferior chance to any other course that can possibly be proposed. We cannot believe that any one can read the history of the several cases detailed, in which the patient was abandoned, and compare the success with that following artificial delivery, without being convinced of the superiority of the latter course." And further on he says, " We shall find also, by referring to the histories of most of those who eventually recovered, that their existence was for months, and in some cases for life, one of suffering; and that with a fistulous opening through the abdominal parietes, or between the peritoneal cavity and the vagina or rectum, with the constant passage of the offensive detritus of the putrefied fœtus, existence must have been anything but desirable. There is nothing to induce us to follow this course,—reason and experience are against it,—*gastrotomy*, then, is the only course which we shall be justified in adopting."

Dr Trask maintains and adduces statistics to prove, that delivery should be effected as early as possible after the rupture has occurred; and, further, gives the following tabular summary of all the cases he has reported, showing the relative success of different modes of treatment :—

Gastrotomy saved 16, lost 4, or 20 per cent. lost.
Turning, etc.    „    23, „    50, or 68·5        „
Abandoned        „    15, „    44, or 75         „

Of the 417 cases of rupture reported by Dr T., there are 20 of traumatic origin, of which the following is a summary view :—

In 4 occurring in the second, fourth, sixth, and seventh months of pregnancy, in consequence of a severe fall, the remains of the fœtus were eventually discharged several months after the accident, either at the umbilicus or partly per anum. The patients recovered.

In 1 at seven and a-half months, the consequence of a blow, portions of the fœtus were discharged several days after the accident, per vaginam, and, in a few days more, the remainder through an opening in the abdomen. The patient recovered.

In 2 at the seventh and fifth months, where the cause was a fall from a tree, one had a living child thirteen months after the accident, and shortly after the remains of a fœtus were discharged from an abscess. The other was long ill,—for five years had great irritation of the bladder; and, about twenty years after, expelled a urinary calculus, the nucleus of which was a fœtal bone.

In 3 cases at the full time the women were tossed by bulls, the abdomen and uterus were ripped up, and the children escaped through the rent. Two of these recovered, the other died.

In 3 also at the full time, two of them the effects of a fall, and the other of a severe blow, gastrotomy was performed, and the patients recovered.

Of 6 at or near the full time, caused by a blow or fall, *two* were left undelivered and died, one thirty-six hours, the other fourteen days after the accident: in one of these the motions of the infant were felt in the abdomen for some time. The parts were found gangrenous on dissection. A *third* was delivered with the forceps six hours after the accident, and died in a few hours. A *fourth* was in a state of great prostration for five days. The os uteri was then forcibly dilated and the child extracted, soon after which the mother died. On dissection, a second child was found among the intestines, and the parts were all gangrenous; in the fifth and sixth, labour came on a few hours after the accident and was duly accomplished, but the patient sank soon afterwards.

The last of these cases is one where, at the seventh month, the patient was jammed by a carriage against the wall, and violent pain and flooding immediately supervened. She languished about five months and died. On dissection, the putrid fœtus was found in the peritoneal cavity, with evidence of violent inflammatory action.

The following case is, as to its *cause*, similar to the one last mentioned, and in its termination equally unfortunate, though the treatment was different.

Mrs Ross, 38 years of age, pregnant for the third time, and about 8½ months advanced in pregnancy, was, on the afternoon of Thursday the 30th March, walking through a narrow court in which she resided, and in doing so had to pass behind a horse and cart which were standing there. When in the act of so doing, the horse suddenly became restive and backed, crushing her with very considerable violence between the hinder end of the shaft and the wall, the shaft striking her on the right side of the uterus, her left being towards the wall.

She almost fainted, was immediately raised up, and carried into her own house, where it was ascertained that there was very considerable hæmorrhage per vaginam, which continued for some time so profusely, that, besides what was gathered off the bed in clots, there was a large pool of blood on the floor, which had run through two mattresses. She was very faint on being taken home.

An opiate draught and hot fomentations to the abdomen were ordered by a medical man who saw her soon after the accident.

At midnight, when Dr Robert Moir, who had attended her during her two previous confinements, first saw her, she was complaining of great pain in the epigastrium, had frequent vomiting, countenance was very pallid and expressive of great suffering, pulse weak, about 125. There were no external marks of violence, but a considerable amount of effused fluid was felt in the lower part of the abdomen. On a vaginal examination, the os uteri was found closed, no part of the child could be felt, and the hæmorrhage was now very slight. One grain of opium was given every hour, and continued till 11 next forenoon (Friday the 31st). The pulse had then become slower and fuller, about 95. Still some pain over the abdomen and vomiting; the amount of abdominal fluid increased, os uteri still closed, and no hæmorrhage. Opium to be continued.·

It was about 11 o'clock of the same evening that I saw her first, in obedience to an anonymous but very urgent message sent to Professor Simpson, requesting his attendance on the poor woman, and which was handed over to me owing to his absence from town. On Dr Black, Professor Simpson's assistant, and myself proceeding to the court, we had the greatest difficulty in finding out the patient, which when we did she was in bed, very pale; pulse 120, of moderate strength; abdomen very greatly distended, and a firm mass lying across the epigastric region. On careful examination, I felt satisfied that this must be the fœtus, but its limbs and general contour were not so distinct and well defined as they are said to be in cases of ruptured uterus, caused doubtless in this case by the quantity of effused fluid. The same cause rendered it rather difficult to make out the contracted uterus; but with a little care, its presence was ascertained in the usual position and about the usual size after delivery.

Chloroform was then administered, and on examination per vaginam, the os uteri was open, and passing two fingers through the os and cervix, a transverse laceration was felt towards the right side and back of the organ, the promontory of the sacrum being plainly distinguished through it. All doubt as to the nature of the case was now removed, it being evident that the uterus had been ruptured, and that the fœtus was lying in the abdominal cavity. A message was at once despatched to my namesake Dr Robert Moir, who had seen her about midday, but who had been all evening, and still was, engaged at a midwifery case, as also to Drs Alexander Simpson and Keiller.

On their arrival, all present were satisfied that the only chance for the poor woman was to open the parietes of the abdomen, and remove the child and secundines, as well as the effused fluid; and the case being explained to the patient and friends, their consent was at once obtained. The patient being placed under the influence of chloroform, I proceeded to open the parietes of the

abdomen in the mesial line, commencing some inches above the umbilicus.    So soon as the opening was made, as much fluid escaped as more than filled a very large-sized tin basin, consisting partly of clots and bloody serum, and also most probably the liquor amnii.    The body of the fœtus, with its back towards the parietes, was lying across the epigastric region; it was seized by the thighs and removed, as also the cord and placenta.    The abdomen was carefully sponged out to remove all fluid, especially clots, and was then closed with wire sutures.

On the patient awaking out of the chloroform sleep, she expressed herself as feeling much more comfortable and freer from pain than before.    She had had about 26 grains of opium from the previous night, this was ordered to be continued if she did not sleep.

On Saturday morning, 1st April, when I saw her at 9, she reported that she had had some sleep and felt very easy; the pulse was much more steady, and had fallen to little above 100; the skin was natural and tongue clean; no abdominal tension, and but little pain; urine drawn off with catheter.

About 1 P.M., the pulse had risen to above 120; abdomen was more painful and becoming distended.    In the evening and about midnight, these symptoms became still more aggravated, the abdomen becoming extremely tympanitic, and she gradually sank till 7 on Sunday morning, the 2d April, when she died, about 60 hours after the rupture of the uterus, and 30 after the operation.

On analyzing the 20 cases stated above, we find that if we exclude the four first mentioned, which happened in the early months or previous to quickening, that there are 7 occurring from that period up to the full time in which the infant, as in this last case, escaped into the peritoneal cavity; and that of these *one* at $7\frac{1}{2}$ months, left to the efforts of nature, recovered; that in *three* death was the result of the same treatment; and that in *three* others, gastrotomy was performed and the patients recovered.

As to the propriety, therefore, of this operation in the case of Mrs Ross, and thus removing the dead infant, secundines, and a quantity of coagulated blood from the cavity of the abdomen, I suppose there can be scarce any difference of opinion, as if left there they must have become decomposed, and caused a fearful death without the least hope of recovery; whereas their removal gave the poor patient a chance, however slight, of surviving this untoward accident.    The only point as to which there may be a difference of opinion is, whether it might not have been better to have delivered the poor woman sooner than was done.    But it must be remembered that immediately subsequent to the accident, and for some hours after, there was, according to all accounts, such a state of collapse that if the operation had been then performed, it would most probably have proved fatal; and that though in the morning, or about fourteen hours after the accident, she had rallied very much, and the hæmorrhage had ceased, so that

the operation might have been then performed, yet I do not feel at all convinced that the few hours' delay added much, if anything, to the danger.

The friends would not permit of any examination of the parts after death.

Article IV.—*Remarks on a Case of Heightened Reflex Action treated by Calabar Bean.* By H. N. MacLaurin, M.D., R.N., Greenwich Hospital.

As the following case appears to me to throw light on some points in nervous pathology and therapeutics, I venture to bring it before the public with a few remarks.

*History.*—David Ritchie, æt. 46, seaman, was admitted to the Infirmary, Royal Hospital, Greenwich, 1st December 1864, under the care of Dr Smart, to whom I am indebted for permission to publish the case. He stated that he had suffered for about twelve months from fits; that the attacks were now becoming milder; that about six months ago he had become paralyzed in the left side; and that he had now regained nearly perfect use of his leg and arm.

*3d December.*—The following report was made:—A fit occurs several times during the day; the consciousness is not lost; the limbs become suddenly rigid; the head is drawn towards the left shoulder, and the patient recovers in a few moments. He had a paroxysm while being spoken to. · Ordered a hot bath and a purgative.

*5th December.*—Has had about ten fits during the night; he is perfectly aware when they come on. Ordered a draught containing fifteen grains of bromide of potassium and three grains of iodide of potassium thrice daily. Under this treatment he improved a little, but not materially. He was allowed to leave the Infirmary on 28th February 1865.

As the fits recurred with great violence, he was re-admitted on 10th March. He now suffered from considerable loss of power of the left arm and leg. The fits recurred six or seven times a-day. In each paroxysm the limbs became straight and rigid, the head was drawn to the left shoulder, and there was twitching of the left eyelid, cheek, and angle of the mouth. The eyes were turned upwards, and to the left. There was no stoppage of the respiration, nor foaming at the mouth. The colour of the face was only very slightly heightened. There was no loss of consciousness, although the power of speech was suspended by the spasm. The paroxysms lasted about half a minute. He was ordered bromide of potassium in scruple doses, without the least beneficial effect; valerianate of zinc was equally inefficacious. As he had had tapeworm when in China, about seven years ago, he was ordered male fern, and after-

wards turpentine, in the idea, that possibly his illness might depend on the presence of parasites in the intestines, but without any good result.  He was brought under the influence of mercury; and he took opium.  Lastly, he took belladonna till the physiological effect of dryness of the fauces and dilatation of the pupil was very strongly marked.  Instead of being better, he was much worse. He had lost all power over the left arm and leg; the hand and foot were turned in at the wrist and ankle.  There was a constant twitching of the left side of the face, involving the eye and the angle of the mouth.  Every six or eight minutes he was thrown into a state of tonic spasm in the manner already described.  The slightest external irritation brought on a fit.  Thus, if he were lifted or touched, or even spoken to, tetanic rigidity came on almost at once.  Each fit lasted about a minute or a minute and a half.  He slept very little.  He was rapidly losing intelligence. He was quite unable to feed himself, or to perform any of the offices of nature without assistance.  He could only swallow liquid food.

As everything else had failed, it was determined on 20th April to try the Calabar bean (*Physostigma venenosum*).  The dose was at first small, but it was gradually increased to four grains, three times daily.  The preparation employed was a solution of the alcoholic extract in glycerine, of such strength that each minim of the solution contained four grains of the bean.  It was prescribed as follows,—R Liq. physostigmat, m. iij.; glycerinæ; aquæ font. ā ā ʒiss.  S. ʒi. ter die.

In describing the result of this remedy it may be as well to divide the effects into physiological and therapeutical.  I do not mean to assert that there is any real difference between the two; but it is convenient to consider, under the one head, those effects of the medicine which do not appear to be essential to the cure, and, under the other, those which form part of the curative process.

The principal physiological results which we had to look for were,—1st, Diminution of the rate of the pulse; 2d, Contraction of the pupil; and, 3d, Epigastric sensation.

1st, The rate of the pulse was generally a little diminished for some time after taking a dose.  Thus, on 21st April, at 11 A.M., the pulse was 68.  He took three grains of the bean, and in five minutes the pulse was reduced to 58.  It remained at this point for two hours, when it returned gradually to its former frequency. But the reduction in the number of beats was on no occasion such as to cause alarm.

2d, The pupil was never observed to be unusually contracted at any time after taking a dose.  The strong solution, however, acted perfectly as a myotic when applied to the conjunctiva.

3d, That indescribable epigastric sensation to which Dr Fraser has drawn attention in his excellent paper on the Calabar bean,[1] was experienced by the patient on several occasions, and with varying

[1] Edinburgh Medical Journal.

intensity. Thus, on one occasion, forty minutes after taking three grains, he began to complain of a peculiar feeling in the epigastrium, which passed off in an hour and five minutes after it came on. On 28th April, after he had been taking twelve grains daily for three days, the feeling became so unpleasant that the remedy had to be omitted for four days. Generally speaking, however, he suffered very little from this feeling. In fact, after 2d May, he scarcely ever mentioned it.

The therapeutical results of the remedy were exceedingly gradual, but quite uninterrupted in their course, and in the highest degree satisfactory. For the first two days no improvement was noticed, except that the patient slept rather better at night. After this, however, the interval between the attacks was noticed to be a little longer, and the fits themselves were less severe. The intellect became clearer, and he was able to read. In a day or two more (29th April) he could be spoken to without a fit coming on. By-and-by he could be fed or lifted out of bed without recurrence of spasm. Simultaneously with the diminution of tonic spasm there was lessening of twitching, and some return of power in the left arm and leg. On 10th May he began to take solid food, and he was able to get out of bed and perform all offices for himself. He required, however, the use of a crutch; after this date he had no recurrence of fits. By 17th May he was able to dispense with the use of a crutch, and to walk about with a stick. On 21st May he gave up his stick, but he had still some weakness of the left leg and arm. He continued to take the remedy in four-grain doses three times daily till 27th May, when, at his own request, he was discharged from the Infirmary. He was then quite free from fits; he had little or no twitching of the face; his intellect was as clear as ever, and he had to a great extent recovered the use of his left limbs. Since that time he has taken no medicine, but he has gone on getting better; so much so, that he says he hopes to be able to go to sea again. Of this, however, there is but little likelihood.

*Remarks.*—In reference to this case the first question which presents itself is,—What was the nature of the disease? In endeavouring to find an answer we must attend to the twofold nature of the question. It implies, in fact, two inquiries:—*1st,* What was the nature of the function or functions primarily impaired? *2d,* What organ or set of organs was the seat of disease? We find that, in this patient, functional disorder of three kinds presented itself. There was violent tonic spasm of the voluntary muscles, dependent evidently on heightened reflex action. He could not be touched without falling at once into a kind of tetanus. The respiratory muscles were not, however, affected. The spasm confined itself mainly to the muscles of locomotion and expression. There was no involuntary ejaculation of excreta. It is true that those co-ordinated movements which are subsidiary

to convenient micturition and defecation were, in great measure, beyond the patient's control.   But we had no reason to believe that the hollow muscles were themselves attacked by spasm like the voluntary muscles.

In addition to this spasm there was also loss of voluntary power over the left arm and leg.   In the intervals of the fits the patient could move his right extremities.   The left extremities were in these intervals immovable and flaccid, the hand and foot being bent inwards.   During the fit, however, the limbs of both sides became equally rigid.   The tendency of the head and of the eyes to move towards the left may be mentioned in connexion with the paralysis of the limbs of the same side.   The third disorder of function was obscuration of the intelligence.   This, though quite perceptible at the height of the disease, was at no time very strongly marked, and soon passed off as the patient began to recover.

Of these three functional disorders, the most important—whether we judge from the history of the case or from the relative promi-nence of the symptoms—was undoubtedly the heightened reflex action.   It was with this that the patient's sufferings began.   This was the primary and essential feature of the disease.   The other functional derangements were secondary and consequent to it.

The second division of our question is, What organ or set of organs was implicated in this case?   To what part of the nervous system is the disease to be referred?   No one, I suppose, will deny that we had to do with a central affection; on this supposition only can we explain the presence of hemiplegia.   Whether or not that central affection depended on primary peripheral irritation is another question.   When the patient was under treatment, his disease con-sisted undoubtedly in heightened irritability of some central nervous organ.   Hence the application of the slightest external stimulus was immediately followed by violent reflex spasm.   Now the spasm which constituted what may be called the peripheral expression of the central disorder implicated the whole of the voluntary motor tract.   Every nerve of a voluntary muscle, from those of the orbit to the sacral plexus, was involved.   Hence there are only two theories possible.   Either, 1st, There was simultaneous heightening of the excitability of all the nervous centres which constitute the bond of union between the centripetal and the centrifugal nerves of the voluntary motor system; or, 2d, There was heightened excita-bility of some central nervous organ which has the power, so to speak, of irradiating reflex action to different parts of the body. For example, there was spasm of the muscles supplied by the *motor oculi*: this must either have depended on a stimulus from the optic nerve, propagated through a very excitable local centre, or on a stimulus from any centripetal nerve conveyed through what may be called a general centre, and thence distributed to all nerves of voluntary motion, and, among others, to the *motor oculi*.   Now, it is highly improbable that there was any special irritation of the local

centre corresponding to the optic nerve. For we find that neither
in the course of the disease nor in that of the cure was the action of
the iris affected. If the optic nerve centre had been affected we
should expect it to be in some way shown by the implication of the
iris. And we can hardly imagine that Calabar bean should remove
a supposed irritation of the optic nerve centre without ever producing
in the slightest degree its own specific action as a myotic. Further,
it may be stated generally as being highly improbable that a state
of great excitement should exist simultaneously and unvaryingly
among so many different local nervous centres as are involved in
voluntary motion. There can be but little doubt that the real seat
of irritation was the medulla oblongata. It has been well shown by
Pflüger, Schröder van der Kolk, and others, that general convulsions
almost always depend on irritation of this organ. It seems to
possess the power of a general centre, implicating all parts of the
body in its disorder.

I have thus shown that it is highly probable that our case was
one of irritability of the reflex function of the medulla oblongata,
producing spasm of the voluntary muscles. It would be interesting
to inquire how far it resembled other affections, such as hydrophobia,
trismus, etc., which also depend on heightened irritability of the
medulla. Too much space would, however, be necessary in order
to go thoroughly into all the facts and arguments bearing on the
question. It may be asked, What are the local organic changes,
central or peripheral, on which this disorder of the medulla depends?
To this, I fear, we can give as yet no answer.

Having stated my opinion of the nature of the disease, it now
remains for me to explain my views of the principles of its treatment.
We may either endeavour to attack the supposed primary local
organic change, which lies at the root of the disease, or we may try
to control the functional excitement which constitutes the outward
appearance of the disease. The first of these methods is objection-
able, because we can only guess at what the local change may be.
It was supposed that the disease might depend upon syphilis, and
the patient was accordingly treated with mercury and iodide of
potassium. But this tentative method failed to give any relief.
We do not know what was the organic local change which corre-
sponded to the disease. If we did, we have no remedy to which we
can confidently look to act on such change. Remedies bearing on
a supposed peripheral cause, such as entozoa, failed equally to
produce any beneficial effect.

Our treatment must therefore be directed to the second indication,
viz., the controlling of the excited reflex function. This opens up
the great question. What means do we possess of influencing
reflex action? That some remedies have the power of heightening
reflex action has been long known. The most direct and powerful
of these is strychnia. But almost all of the narcotic remedies pro-
duce a similar effect, though in a much less marked degree. Opium,

belladonna, and many others, possess the power of heightening reflex action when introduced into the economy. Theoretically, then, such remedies would be objectionable in the case I have given; and it was actually found that, after the administration of belladonna, the patient became worse instead of better.

On the other hand, the list of remedies which possess the power of depressing reflex action is not so long. To the researches of Fraser, already referred to, we owe our acquaintance with by far the most trustworthy of them,—the Calabar bean. According to his views this remedy exercises a powerful depressing influence on the functions of the spinal system. It has the great advantage, that the quality of the drug can always be tested by its topical action on the iris.

In consequence of Fraser's statements, it was deemed proper to give the remedy a trial in this case. There can be no doubt that its results were in the highest degree satisfactory. I have very little doubt that it would produce equally satisfactory results in cases of trismus, tetanus, and hydrophobia. I am not quite so sanguine as to its beneficial effects in epilepsy, but I think it would be, at all events, worth a trial.

ARTICLE V.—*On Acute Yellow Atrophy of the Liver.* By T. GRAINGER STEWART, M.D., F.R.C.P., Pathologist and Special Assistant-Physician to the Royal Infirmary, and Lecturer on General Pathology, Surgeons' Hall, Edinburgh.

(*Read before the Medico-Chirurgical Society, 5th July* 1865.)

THE following case is worthy of attention, both on account of its rarity and of the light which it seems fitted to throw upon the disputed question of the nature of the affection of which it affords an example.

Mrs H., æt. 35, was admitted to the Royal Infirmary, almost moribund, on 11th February 1865. She was sent to one of Dr Warburton Begbie's wards, but died very soon after admission. Her history was imperfectly ascertained, as she was comatose at the time of her admission; but from inquiry among her friends who had been with her before she went to the hospital, I have ascertained the following facts:—She was in the sixth month of her fifth pregnancy, and was leading an unhappy life, in consequence of domestic quarrels; but it was not known that any special disagreement had preceded her illness. She was in good health until a fortnight before her death, but then complained to her neighbours of a strange feeling of uneasiness which she felt and feared, but could not define. She gradually became worse, and began to vomit yellow matter like the yolk of an egg. She then also

became jaundiced, and this gradually deepened. Her bowels were constipated. She never was drowsy or delirious, nor did she vomit blood nor pass it at stool until the morning of 11th February, when she became much worse, vomited a large quantity of blood, became very drowsy, and could not be roused. In the afternoon, Mr Furley was called to see her, and, by his direction, she was removed to the Infirmary. The movement roused her, and she became for a little time conscious. On admission to the Infirmary she was shivering, and complained of cold. She vomited blood almost incessantly. She passed her fæces in bed; they were dark-coloured and fluid. She made water, but it could not be collected for examination. About eight o'clock, she became delirious, violent, and very noisy. This continued until midnight, after which she was comatose. Labour commenced about eight o'clock; the waters came away about nine. She was delivered of twins, just as she was dying, at seven o'clock on the morning of the 12th.

*Autopsy.*—The body was examined thirty-one hours after death. It was moderately well nourished. The *skin* was icteric and somewhat dusky. There was considerable hypostatic congestion, but no subcutaneous extravasation of blood. The *muscles* were dry; the blood dark and fluid. *All the internal organs* were jaundiced. There were *patches of extravasation* under the visceral layer of the pericardium, particularly over the left auricle, and towards the upper part of the left ventricle. The *heart* was moderately contracted, contained no clot, and very little blood. The muscular substance was somewhat pale; the valves were natural. The *aorta* was natural. The *lungs* were congested and somewhat œdematous, particularly at their lower and posterior parts. The *liver* was reduced to one-half its natural size; but, except being somewhat flattened, it retained its ordinary form. It weighed 1 lb. 7½ oz. Its surface was not shrivelled. Beneath its capsule and throughout its substance there were numerous ecchymoses, and small patches of an ochre yellow colour, but the mass of the organ was of a dark reddish brown hue. The outlines of the lobules were not recognisable. The *gall-bladder* was contracted, and contained a little grey inspissated mucus.

On *microscopic examination* of a scraping from a cut surface of the liver, a large quantity of debris and fatty matter, with cells in different stages of alteration, some full of oil globules, and containing a few bright ochre-yellow granules, and others full of dense granular matter, not fatty, were found. All the cells were considerably enlarged, and denser than natural. No natural cell was observed. The amount of bile pigment found both in the cells and debris was less than is usual in such cases.

On examining sections, it was found that the cells in the outer part of the lobules were almost completely destroyed, while those towards the centre were larger, denser, and more opaque than natural, and the amount of oil was greater towards the margins,

where the destruction of cells was taking place.    The system of
vessels and the fibrous stroma of the organ were not destroyed.
It was easy to make sections, and the sections were easily washed
without their giving way.    In the sections a considerable amount
of bile pigment was seen scattered about.

Some demonstrations were made of the blood in the liver, but
neither in it nor in the substance were any crystals or balls of
tyrosin or leucine observed.

The *spleen* was enlarged, weighed 8 oz., was soft and pulpy.
There were some points of extravasation in its substance and
beneath its capsule.

The *suprarenal bodies* were somewhat enlarged and partially
disorganized, but their microscopic structure was not ascertained.

The *kidneys* were somewhat enlarged, weighed together $10\frac{1}{2}$ oz.
Their cortical substance was dense and pale.    The capsule was
easily stripped off.    On microscopic examination, the tubules,
both straight and convoluted, were seen to be full of exudation,[1]
and presented the appearance of having been very successfully
injected with some dark matter.    The Malpighian bodies and
vessels stood out clear and transparent among the tubules.    With
a higher power the vascular structures appeared natural.    The
dark matter occupying the tubules was found to be composed of
exudation into and between the cells.    Very few tubules remained
healthy; in some the epithelium was swelled, thickened, in a state
of cloudy swelling, and here and there it was loaded with fat granules.
In some parts the outlines of the renal cells could not be made out;
the tubules were full of a dense homogeneous granular matter,
containing numerous oil globules.

The *œsophagus* was natural; the *stomach* was distended, and
contained a good deal of dark uncoagulated blood.    Its inner
surface was coated with slimy mucus; its walls were thickened, and
in the mucous coat there were numerous catarrhal ulcers, mostly along
the greater curvature, and on the anterior and posterior walls.    Extra-
vasation of blood existed about the bases of some of the ulcers.

The *intestine* contained almost no bile, but some altered blood,
and a good deal of hard nearly clay-coloured fæces.    The Peyerian
patches and solitary glands were swelled and prominent, particularly
those about three feet from the lower end of the ilium.    The large
intestine was natural.

---

[1] *Exudation.*—I employ this convenient term as one which is in general use,
and generally understood as describing the condition referred to in the text,
but do not regard it as an accurate term.    The idea upon which it was
originally founded, viz., that certain elements of the blood were poured or
sweated out of the vessels, in consequence of changes in the vessels or in the
blood, is quite untenable.    The view now generally held, and which I have
adopted, is, that the tissues attract to themselves certain elements, and in this
way the appearance referred to is produced.    But though the theory of its
production be altered, the descriptive term need not be changed; I think,
therefore, that we may continue to call this matter exudation.

The *pancreas* was natural.

The *mesenteric glands* were somewhat enlarged.

The *uterus* was large, about seven inches in length, moderately contracted; the cervix was long, and a plug of bloody mucus projected from the os.

Both *ovaries* were much scarred, and each contained a distinct corpus luteum.

The *Fallopian tubes*, particularly their fimbriated extremities, were congested.

The *skull* was natural. There was a little subarachnoid effusion. The brain was firm, somewhat congested throughout; it weighed 2 lb. 6½ oz. The ventricles were not dilated. There were a few small cysts in the choroid plexus.

The *bladder* contained almost 16 oz. of dark amber-coloured urine, which was carefully removed and sent to Dr Arthur Gamgee who kindly analyzed it, with the following result:—

" The urine was of an intensely jaundiced hue, and exhibited in a very characteristic manner the reaction of bile pigment. Its reaction was acid. It was divided into two portions—A and B.

" The portion A, measuring 200 c. c., was precipitated with neutral and basic acetate of lead, and the fluid filtered. The lead precipitate was reserved for the analysis of the bile acids.

" The filtrate was treated with a stream of sulphuretted hydrogen gas, in order to precipitate the excess of lead, and the clear and almost colourless filtrate thus occasioned was concentrated at a very gentle water-bath heat, and then placed aside in a cool place. At the end of twenty-four hours an abundant crystallization had taken place. The crystals, when examined under the microscope, were found to consist of the most characteristic needles and tufts of tyrosin tinged of a light yellow colour; they were separated by filtration, and dissolved in boiling water. On cooling, the water deposited a nearly snow-white mass, composed of beautiful needles of tyrosin; these were again crystallized from a solution in boiling water, and then dried. In the process of drying they contracted very much. When strongly heated they burned away completely, without leaving a trace of ash; they exhibited in a most characteristic manner the chemical reactions of tyrosin.

" The fluid from which the tyrosin had been separated was evaporated to a syrupy consistence, and set aside for some days. When examined, it was found to contain, in addition to much tyrosin which had separated, most characteristic masses of leucine.

" The precipitate which had been obtained by precipitating the urine with acetate of lead was suspended in water, and a stream of sulphuretted hydrogen passed through it. The fluid was filtered and evaporated at a gentle heat. The residue was dissolved in water, and tested by Pettenkoffer's test for bile acids, but none were found.

" The smaller portion of urine, B, was employed to determine the

presence and quantity of the more usual urinary constituents, and the results of the analysis are tabulated below.

" The points which specially call for notice are the following :—1*st*, The urea was determined by Liebig's method.   As tyrosin is equally with urea precipitated by nitrate of mercury, the results of the analysis are obviously not perfectly correct, the urea and tyrosin having in fact been estimated together.   As the tyrosin appears to have been present in very large quantity, it may be assumed that the amount of urea was actually very small.   **2*d*,** The amount of uric acid could not, from the excessively small quantity of urine which was obtained, be estimated; only traces of it, however, existed.   **3*d*,** The urine contained absolutely no chlorides, only the faintest trace of sulphates, and the earthy phosphates were absent.   The only salts present were in fact alkaline phosphates. This fact, which was brought out by the qualitative analysis, was also borne out by the quantitative; for the amount of phosphoric acid which was found, when calculated as phosphate of potash, is almost identical with the amount of ash as found by direct ignition."

*Table showing the Result of Analysis of Urine.*

| | |
|---|---|
| Specific gravity of urine at 60° Fahr.,   .     .     . | 1018·82 |
| Reaction acid. | |
| Total solids in 100 c. c.,   ..   .   .   .   . | 2·194 |
| Urea and tyrosin in 100 c. c.,   .   .   .   . | 1·80   gramme. |
| Leucine, uric acid, and colouring matter in 100 c. c., | ·1415   ... |
| Albumen,   .   .   .   .   .   .   .   . | ·0116   ... |
| Salts,   .   .   .   .   .   .   .   . | ·2525   ... |
| Phosphoric acid,   .   .   .   .   .   . | ·204   ... |

This case affords a very good example of the acute yellow atrophy of the liver.   But there are three points which I should like to bring under the notice of the Society :—1*st*, The state of the urine as ascertained by Dr Gamgee; 2*d*, The peculiar affection of the kidneys; and, 3*d*, The evidence which the case affords as to the nature of the disease.

I. *The State of the Urine.*—The points which appear to be most important are,—1*st*, That notwithstanding the presence of a large amount of abnormal ingredients, the *total solids* were considerably below the natural quantity, and thus the functional activity of the kidneys was shown to be diminished; 2*d*, That *leucine and tyrosin* were the chief abnormal ingredients, as is usual in this disease ; 3*d*, That a little *albumen* was present; 4*th*, That notwithstanding the extreme jaundice, *no bile acids* were found,—a fact which, at first sight, seems to confirm Dr Harley's view that these constituents are awanting in cases of jaundice from suppression ; but this point cannot be deemed important, when we consider that only Pettenkoffer's test was employed, a test which chemists do not now consider satisfactory; 5*th*, That the *urea* was probably much diminished in quantity, and only traces of *uric acid* were found, while in most febrile and hepatic

affections these elements are found in excess; 6th, That the *chlorides and earthy phosphates* were entirely absent, and that only the faintest trace of *sulphates* was found.

It is evident that some of these altered conditions of the urine depended upon the state of the kidneys and some upon that of the liver and the system generally. There can be little doubt that the leucine and tyrosin resulted from changes in the liver, and that they were eliminated from the kidneys along with the water, at a time when the renal epithelium had become incapable of separating from the blood the natural solids of the urine. The absence or diminution of some of the elements—as the chlorides, earthy phosphates, and sulphates—may have depended upon the state of the system generally, but almost certainly the diminution of the urea depended upon the peculiar condition of the kidneys, for Frerichs has found that in this disease its quantity is rather increased than diminished.

It is to be regretted that the only urine obtained for analysis was that found in the bladder after death. During the time that she was in the Infirmary she made water, but in bed and involuntarily, and it could not be collected. It is very probable that, had we been able to examine the urine for some days before death, we might have found, as did Frerichs in some of his cases, a progressive diminution of the urinary solids, coincident with changes in the kidneys. It seemed impossible that kidneys in the state in which this woman's were found could be capable of any secretion at all ; and, from this consideration, I think that the urine which was in the bladder had been secreted some time before, but retained in consequence of the torpid state of the nervous system. I have seen at least one case in which suppression of urine had occurred, in the course of pyæmia, and in which the kidneys were much less affected than they were in this instance.

But though we cannot in the meantime speak positively upon these points, it is evident that in every case of this disease the urine should be carefully examined, and that in treatment constant attention should be paid to the state of the kidneys.

II. *The Affection of the Kidneys.*—The state in which we found the kidneys specially deserves attention. A very copious exudation occupied the uriniferous tubules, and the epithelium was affected in the same way as the cells of the liver. Some were swelled, dense, opaque, granular, some extremely fatty, and some had broken down and disappeared,—a series of changes exactly corresponding with those of the liver. But for the firmer support which the stroma of the kidney and the walls of the tubules affords, we might expect a rapid wasting of the kidney like that which occurs in the liver.

The complete identity of the appearances met with in the two organs satisfies me that they were really affected in the same manner, that the morbid processes were identical, and that we cannot regard the renal affection as a consequence of the hepatic.

III. *The Evidence which this Case affords as to the Nature of the*

*Disease.*—Various opinions have been held as to the nature of this affection. They may be referred to three classes:—

1. Some conceive that from an increase in the amount, and perhaps a change in the quality of bile, the hepatic cells are dissolved, and so the acute atrophy is induced. This condition has been referred by Rokitansky to an excess of bile in the portal system, by Henoch and Van Dusch to changes in the bile ducts. But I think that this view has been disposed of by Frerichs, who points out,—1st, That no accumulation of bile precedes the acute atrophy; 2d, That even if such an accumulation did precede, it could not occasion the atrophy, because in cases in which bile is retained no solution of the cells of the liver takes place, and he has found by experiments that hepatic cells may be immersed in bile for whole days without undergoing solution.

2. Others conceive that the acute yellow atrophy is a result of a blood disease, and that the destruction of the hepatic cells, as well as the hæmorrhages which are often associated with it, depend upon the marked weakening of the heart's action and the rapid increase of the peripheric metamorphosis of matter.

This theory is, I think, right in so far; but before entering upon it, let me refer to the third.

3. It has been held by some to depend upon a diffuse inflammation of the gland : the wasting of the gland elements being a result of fatty degeneration consequent upon exudation into this substance.

This view has been maintained by Bright, Engel, Wedl, and Bamberger, and has been supported also by the eminent Professor of Clinical Medicine in Berlin, Professor Frerichs, but with a certain reservation ; for while he says that, " according to his experience, an exudation process constitutes the starting-point of the disease," yet " he has some hesitation in identifying the destruction of the hepatic cells with fatty degeneration ; because in the case of other glands, such as the kidneys, fatty degeneration of the epithelium does not produce such a rapid and general destruction of the cells, and especially because in acute atrophy of the liver the fat can only be seen deposited in the circumference of the lobules, whilst the destruction of the cells is found to extend so far as the central vein."[1]

Of the fact that the destruction of the hepatic cells is a result of exudation, and that through a rapid fatty degeneration, I have no doubt. The state of the cells in different parts of the lobules put this beyond question ; for in the centres they were dense, solid, and full of exudation, at the margins they had completely disappeared, leaving only the vascular network and the stroma, and between those two zones there was an intermediate ring of extreme fatty degeneration.

The first part of Frerichs' difficulty is very well met by the facts observed in the case under consideration, for the epithelial cells of

[1] A Clinical Treatise on the Diseases of the Liver, by Professor Frerichs. Syd. Soc. Translation. Vol. i. p. 229.

the kidney were in the same state as those of the liver. It thus appears that a similar exudation thrown out into the cells of those two organs leads to the same destruction in both. And his second difficulty seems to me to disappear, when we observe that a regular series of changes existed from the centre to the periphery of the lobules.

It is interesting to observe, that while in most cases the disintegration of cells commences at the margins and spreads towards the centre of the lobule, Dr Handfield Jones[1] observed one in which the destruction commenced around the hepatic venous radicle and spread outwards; and in his case it would appear that the destruction of the cells also resulted from an exudation process. We may assume, then, that the destruction of the cells is a result of an exudation process, but we have further to inquire as to the origin of this process. Is it a local affection, commencing in the liver and secondarily affecting the kidneys, the spleen, the intestine, and the blood? or is it a blood disease independently operating on all these organs?

The latter view seems distinctly indicated by the case which we have recorded, as well as by many others already on record.

The following considerations seem to me to point distinctly to this conclusion:—1. The blood was dark and fluid, and the muscles were dry as they are in typhus fever and other blood diseases. 2. The spleen was soft and pulpy as it is in many febrile blood diseases. 3. The fact that the kidneys and the liver were affected by a peculiar and identical morbid process indicates that they were influenced by a common cause, that cause situated in the blood, and a form of fever poison. 4. The appearance, amount, and effects of the exudation being different from what we see in simple inflammation, either of the liver or kidneys, indicates that some peculiar matter was present in the system altering the ordinary processes. 5. The facts that this disease occurs so often during pregnancy, and that it seems to be induced by depressing mental emotions, indicate that it is of a constitutional origin. From these considerations, I think that we cannot avoid concluding *that this peculiar affection is a blood disease, and that it leads to atrophy of the liver, by diffuse exudation into the hepatic cells, which is followed by a rapid fatty degeneration.*

This case is also fitted to draw special attention to the fact, that the blood poison may act on the cells of the kidneys as well as those of the liver, may destroy their structure, and arrest their functional activity as it does to those of the liver. There are very few cases of the disease on record in which the kidneys were so seriously involved as in the present; but Frerichs mentions that a granular state of the renal epithelium is by no means uncommon in cases which, like the present, occurred during pregnancy. What may be the import of the renal complication we cannot in the meantime determine, but it is evident that it must seriously increase the dangers of the disease.

[1] Diseases of the Liver, by George Budd, M.D. 2d Edition, p. 256.

ARTICLE VI.—*On certain Positions of the Head as a Cause of Syncope.* By J. SMITH, M.D., F.R.C.S., Surgeon-Dentist to the Royal Infirmary, etc.

*(Read before the Medico-Chirurgical Society, 5th July 1865.)*

SYNCOPE as a result of pressure on the internal carotids is a physiological experiment long familiar to medical men. I can find no allusion, however, to the possibility of producing pressure on the vessels of the neck with such a result, merely by certain positions in which the head may be placed.

The occurrence of cerebral symptoms on the occlusion or obstruction of any of the larger arteries leading to the brain has been recorded by various authorities. Dupuytren[1] relates several cases where death occurred from cerebral affections attributed to tying the carotid. Twelve[2] out of the fatal cases in Dr Norris's table died of symptoms referable to the brain, and which were apparently such as to suggest atrophic softening due to the operation. Kuhl[3] of Leipzig describes convulsions and other cerebral symptoms following in a case where he tied both carotids, within twenty-seven days. But, without further allusion to the ultimate or more remote effects, we find the immediate result of arresting the circulation through the carotids—if this occur in both at once—to be syncope.

John Bell,[4] after quoting Valsalva, Van Swieten, Pechlinus, Lower, Drelincurtius, and others, as experimenting on animals by impeding the cervical circulation, makes the following remarks regarding the human subject :—"The carotid arteries, says Valverda, being tied up or anyhow obstructed, the person grows stupid and falls presently into a profound sleep." Again, after referring with somewhat of a sneer to some cases in which pressure on the carotids was alleged to have been remedially employed by producing stupor, he adds, "If what Dr Parry says be true, that in lean people—in women at least—we can, by reclining the head back, compress the carotids entirely against the forepart of the neck with the finger and thumb, why then need we have fear of hæmorrhages of the nose, wounds about the jaw, cutting the parotid gland, or operations about the tonsils or tongue?"

What I would here advance, however, is, that by simply bending the head backwards, we can, without pressure of the finger and thumb, diminish or impede the circulation in all cases, not only through the internal carotids, but through the vertebral arteries; and, further, that in some persons, by widely opening the mouth and thereby bringing the angle of the lower jaw nearer the cervical

[1] Lesions of the Vascular System: Sydenham Society: pp. 53, 56.
[2] American Journal of Medical Sciences, vol. xiv. p. 22 ; and Holmes' Surgery, vol. iii. p. 498.
[3] Chelius' Surgery.
[4] Anatomy of the Human Body, p. 89.

vertebræ, while the head is in the position just described, such
effect may be increased owing to the parts within and behind the
angle of the jaw compressing, in certain cases, the internal carotids
against the muscles anterior to the spine.  The corollary being,
that by such causes of disturbance of the circulation in the vertebrals
and internal carotids, syncope may be produced.

In order to fortify any conclusion I might venture to draw from
my own anatomical knowledge of the parts concerned, I submitted
my views on the matter to Mr Turner of the University here,
and I have the satisfaction to know that, on examining in the
recent subject as well as in dry preparations, the relation of parts
at the base of the skull, and the changes of position produced by
the movements mentioned, he coincides with the opinion I am
inclined to entertain.

On examining the arteries of the neck *in situ*, the· readiness
with which the circulation through the vertebrals as well as the
internal carotids, could possibly be in this way interfered with,
appears obvious.  The internal carotid enters the foramen in the
petrous portion of the temporal bone at a point in the base of the
skull scarcely anterior to a line drawn transversely in front of the
articulating processes of the occipital bone.  The carotid foramen
is about half an inch nearer the median line than the internal
boundary of the glenoid cavity.  The artery consequently lies
immediately within the posterior border of the ascending ramus of
the lower jaw.  Before entering the skull it rests upon the trans-
verse process of the atlas, having the thickness of the rectus capitis
anticus muscle interposed between it and that vertebra, and, among
other structures, it has lying in front of it a portion of the parotid
gland.  This gland, with these other structures, occupies a deep
recess between the posterior edge of the ramus of the lower jaw
and the anterior margin of the mastoid process and sterno-mastoid
muscle.  This,[1] the pterygo-pharyngeal space, undergoes con-
siderable variations in size, produced, firstly, by the position of the
head; secondly, by the movements of the lower jaw.  On the
chin being made to approximate the sternum, the ramus of the
jaw glides backwards over the sterno-mastoid muscle, almost
obliterating the space inferiorly.  In this manner its contents
become compressed, and as these include the internal carotids, the
circulation through these vessels is impeded.

Now it has been shown[2] that arteries *in situ* normally exist in
a state of tension.  For example, the head being on a level with
the body, a portion of the right common carotid artery of a man,
when exposed in its natural position, was found to measure $1\frac{4}{5}$
inch.  When carefully isolated and accurately divided after removal,
it measured only $1\frac{1}{5}$ inch, the tension on the right carotid being
thus represented by 3-10ths of an inch.  A limit, however, is set

[1] Ledwich's Anatomy, p. 249.
[2] Savory "On the Shape of Transverse Wounds of Bloodvessels," pp. 10, 11.

to this extensibility of arteries, for the calibre of the canal is found to diminish in proportion to its elongation. Bearing these facts in mind, it will be recollected that besides the cervical vertebræ admitting of more antero-posterior movement, especially extension, than any other part of the spinal column, the skull itself can also be to a very free extent raised or depressed upon the occipito-atlantoid articulation. In this way, then, on the head being thrown back, the internal carotids become elongated, and are stretched over the face of the rectus anticus; and if while in this position there be superadded to the already diminished calibre of the vessel such depression of the lower jaw as makes its angle approach nearer the spinal column, we have the parts connected with and lying behind the ascending ramus encroaching upon the pterygo-pharyngeal space, and by pressing upon the already tense artery, still further impeding, and in some cases it may be arresting, the flow of blood.

[1] The result of compressing both vertebral arteries might be anticipated to be more energetic than in the case of the carotids. In the vertebrals, any occluding effect likely to be produced by position of the head would appear liable to occur at that tortuous part of their course where they pass between the axis and atlas, and between the atlas and occipital bone; the artery, in this instance, being not only compressed, but as it were what is termed " kneed " across the edges of the bony structures among which it threads its way. The vertebral would thus appear to differ from the carotid in the mode of its obstruction, flexion of the vessel here aiding its occlusion. Extreme flexion has, as is well known, been pointed out[2] as capable of impeding the current of blood, through an arterial trunk, to an extent requiring special provision, in the vicinity of joints, for the vessel's safety—such as the diverticular channels always connected with arteries situated in such localities. In this manner the vertebrals would become shut up, as an elastic tube does on being abruptly bent, and, as has been already said, on the same principle as holds in the case of stopping the pulse at the wrist or ankle-joints by forcibly bending the knee or elbow; a principle on which the cure of aneurism has been suggested and carried out, and on which a considerable amount has been written by such authorities as Ernest Hart, Nunn, Moore, etc.

The occurrence of such tendency to faintness as might be produced in the manner above described, is in itself perhaps of no great moment, but it becomes of some importance when it is considered as a complication likely to arise, for instance during the admini-stration of chloroform. Here every difficulty and every risk in an occurrence of the kind is to an immense degree increased, and recovery rendered more precarious. In some minor operations such risks are all the more liable to occur. Added to the sitting

[1] See Sir Astley Cooper—Guy's Hospital Reports, vol. i. pp. 450, 654.
[2] Nunn, " Observations and Notes on the Arteries of the Limbs," p. 23.

posture, during which, notwithstanding its danger, most such operations under chloroform are performed, we have in those of dental surgery that combined reclination of the head, with depression of the lower jaw, forming a circumstance not to be lost sight of. We know that in dental operations practitioners are familiar with instances where syncope occurs altogether apart from chloroform, and without any assignable cause whatever,—such as the infliction of pain or even the feeling of timidity. Among several cases coming under my own observation, I may here mention one, which indeed first directed my attention to the subject of this paper. On the morning of 8th May 1865, a strongly-built young man applied at the Dental Dispensary here, and required to have his upper incisors examined. He was not to have extraction or any painful operation performed, so there was no cause for apprehension, and he certainly exhibited no signs of timidity. He was seated on a high-backed chair, with his head bent as far backwards as possible in order to facilitate the examination of his upper teeth. I was engaged in this occupation when I observed his breathing to be embarrassed, and on looking at him found he was insensible. I attempted to lay back the chair with him on it, but found, from a screen standing behind it, that there was not room for this; his head was during this time hanging back, and on my supporting it, he came round again, looking somewhat confused. I asked him what was the matter, and if I had been hurting him. He replied, " No, sir; but it was jist something that cam owre my een." Such cases are not uncommon in the practice of dentists, and their explanation in all probability lies in what is here assumed as a cause hitherto overlooked,—namely, the position of the patient's head and lower jaw at the time such syncope occurs.[1]

If, then, we can by attention to such points obviate, in however small a degree, the predisposition to syncope during operations of the kind, especially when performed under anæsthesia, it becomes a matter not only of interest physiologically, but of some importance towards ensuring the patient's safety.

Lastly, I need not suggest, that should the hypothesis here advanced be accepted as correct, it might explain still more important phenomena than mere cases of fainting as above described. The occurrence of death during intoxication, and even of death during sleep, might, without exceeding the bounds of possibility be in some cases accounted for on similar principles,—namely, to obstructed circulation through the cervical arteries resulting from the position of the head.

[1] Since the occurrence of this case I have had my attention drawn to several others of the same kind.

ARTICLE VII.—*Is the Dietary authorized by the Secretary of State, in the " Rules for Prisons in Scotland," sufficient to bring up the Physical Frame of an Adult Prisoner to the Standard of Health and Vigour, and maintain it there?* By Dr BONNAR, Cupar, Surgeon to the Fife County Prison.

IT has lately been my duty to investigate the question, why the criminal inmates of our local prison lost weight, in spite of roomy cells, fresh air, faultless drainage, and every attention to cleanliness and health which an experienced and excellent governor and painstaking matron could exercise.

In the course of my inquiries, it became necessary to analyze the authorized dietary according to the very useful and comprehensive table given by Dr Lyon Playfair in the January No. of *Good Words ;* and, as the question of food, in relation both to economy and work, as well as health, is occupying a very prominent place in the applied sciences of the present day, I beg to subjoin the results of my labours as an item towards solving the problem what kinds of food, and how much, it is necessary to provide in our prisons for sustaining the general health and condition of the prisoners, without indulgence on the one hand, or deterioration to the *physique* on the other.

I pass over the dietaries' appointed for those whose terms of imprisonment are under three days, inasmuch as, although the nutritive matter is much below that on which it would be possible to subsist in health for any lengthened period, yet three days' restraint is a punishment of so slight a character in itself, that, unless the *animal* is made in some measure to feel the penalty of crime, the mere confinement would be a very inadequate preventitive to its re-commission ; nor do I comment on those rates of diet prescribed as punishments to the refractory, for it is obvious that the prisoners themselves, and not authority, are responsible for their infliction or continuance.

I would confine my remarks and comparison to those *three* rates under Section 112 of the Rules, which are applicable to all prisoners who are confined for any length of time exceeding three days.

The table of authorized diet is as follows :—

TABLE I.

| First Rate. | | |
|---|---|---|
| Breakfast. | Dinner. | Supper. |
| 6 oz. of oatmeal made into porridge, with ¾ pint (15 oz. by weight) of skimmed or butter milk. | 1½ pint of barley-broth or pease-soup, with 6 oz. of wheaten bread ; or 2½ lb. of potatoes, with ¾ pint of skimmed or butter milk. | 1½ lb. of potatoes ; or 4 oz. of oatmeal made into porridge, with ½ pint of skimmed or butter milk. |

| | Second Rate. | |
| Breakfast. | Dinner. | Supper. |
|---|---|---|
| 8 oz. of oatmeal made into porridge, with ¾ pint milk. | 2 pints of broth or soup, with 8 oz. of wheaten bread; or 2½ lb. of potatoes, with ¾ pint of milk and 4 oz. bread. | As above. |
| | Third Rate. | |
| As above. | 2 pints of broth or soup, with 12 oz. of bread; or 2½ lb. potatoes, with ¾ pt. milk and 8 oz. bread. | 2 lbs. potatoes; or 6 oz. oatmeal made into porridge, with ½ pint of milk. |

In Section 113, rules are laid down as to the kinds and qualities of the various ingredients to be used in the manufacture of the mid-day meal. Two pints of broth must contain 4 oz. of barley, 2 oz. of marrow-bones or 1 oz. of meat, or ½ oz. of dripping, and a proportionate quantity of succulent vegetables. Two pints of pease-soup must contain 4½ oz. of pease, same proportion of meat as the broth, but about half the vegetables.

Potato-soup is permitted to be substituted for broth or pease-soup, 1 lb. of potatoes being reckoned equivalent to the 4 oz. of barley, the other ingredients remaining the same.

Barley-milk, containing to each two pints 4 oz. of barley, well boiled; and half-a-pint of milk may likewise be supplied instead of broth or soup.

Fish may be substituted for broth at the rate of 6 oz. of fish for every pint of broth.

Without complicating my illustrations, I shall take as the standard prison dietary that given in the above table, and analyze its constituents under each rate, and compare the results with the estimated amount of nutrition required for adults in various grades and under different circumstances, as detailed in Dr Playfair's very able paper on "The Food of Man in relation to his Useful Work."

TABLE II.

*Under the First or lowest Rate, we have in the 24 hours as follows :—*

| | When the Dinner or principal Meal consists— | | | | |
| | 1. Of Broth. | 2. Of Pease-soup | 3. Of Potato-soup. | 4. Of Potatoes and Milk. | 5. Of Fish. |
|---|---|---|---|---|---|
| Oatmeal, . . . . . . | 10 oz. | 10 oz. | 10 oz. | 10 oz. | 10 oz. |
| Skimmed Milk, . . . . | 25 oz. | 25 oz. | 25 oz. | 40 oz. | 25 oz. |
| Barley, . . . . . . . | 3 oz. | none. | none. | none. | none. |
| Succulent Vegetables, . . | 3 oz. | 1½ oz. | 3 oz. | none. | none. |
| Butcher-meat, . . . . | ¾ oz. | ¾ oz. | ¾ oz. | none. | none. |
| Wheaten Bread, . . . | 6 oz. | 6 oz. | 6 oz. | none. | 6 oz. |
| Fish (fresh), . . . . . | none. | none. | none. | none. | 9 oz. |
| Pease, . . . . . . . | none. | 3⅝ oz. | none. | none. | none. |
| Potatoes, . . . . . . | none. | none. | ¾ lb. | 2½ lb. | none. |

The amount of nutritive and heat-giving materials contained in the table are arranged under the following heads :—

TABLE III.

| | Flesh-formers. | Heat-givers. | | Starch equiva-lents of heat-givers. | Carbon. |
|---|---|---|---|---|---|
| | | Fat. | Starch or Sugar. | | |
| I. Where dinner is of broth, | oz. | oz. | oz. | oz. | oz. |
|    10 oz. of oatmeal contain . . . | 1·6 | 0·6 | 6·2 | 7·64 | 4·095 |
|    25 oz. of milk, . . . . . . | 1· | 0·5 | 1·15 | 2·2 | 1·383 |
|    3 oz. of succulent vegetables, . | 0·03 | ... | 0·36 | 0·36 | 0·183 |
|    3 oz. of pearl barley, . . . . | 0·24 | 0·06 | 2·22 | 2·36 | 0·16 |
|    ¾ oz. of butcher-meat, . . . | 0·14 | 0·1 | ... | 0·25 | 0·157 |
|    6 oz. of wheaten bread, . . . | 0·49 | 0·06 | 2·67 | 2·81 | 1·496 |
| Totals, . | 3·50 | 1·32 | 12·60 | 15·62 | 7·474 |
| II. Where dinner is of pease-soup, omitting the barley and extra vegetables, and adding pease, Totals, . | 4·19 | 1·33 | 12·17 | 15·22 | 8·551 |
| III. Where dinner is of potato-soup, Totals, . | 3·43 | 1·27 | 13·08 | 14·98 | 8·69 |
| IV. Where dinner is of milk and pota-toes, . . . . . . Totals, . | 3·76 | 1·44 | 17·31 | 20·19 | 10·895 |
| V. Where dinner is of fish and bread, Totals, . | 4·21 | 1·62 | 10·02 | 13·87 | 7·901 |
| Average of the above, . . . . | 3·81 | 1·39 | 13·03 | 15·97 | 8·702 |

Under this rate of diet, all prisoners undergoing sentences of imprisonment not exceeding two months, are kept, not being employed at hard labour; and even a hard labour sentence, if not prolonged beyond ten days, does not entitle the prisoner to any other.

As a change of food, in respect of the dinner, is directed for at least two days in each week, and as it is desirable to obtain an approximatively correct table for the purpose of instituting a comparison between the amount of food recommended by authority, and that deemed necessary for the efficient sustenance of the adult frame according to the latest observations, I have taken the average of the different kinds of diet analyzed above, as the measure of the amount of nutrition supplied, under the lowest rate, to each adult prisoner in the twenty-four hours.

Proceeding after the same method, we obtain for the *second* rate as follows :—

TABLE IV.

| | Flesh-formers | Fat. | Starch. | Starch Equiv. | Carbon. |
|---|---|---|---|---|---|
| | oz. | oz. | oz. | oz. | oz. |
| I. Where dinner is of broth (2 pints), Totals, | 4·14 | 1·52 | 15·61 | 19·10 | 10·293 |
| II. Where of pease-soup,        ″     ″ | 4·79 | 1·53 | 15·03 | 18·54 | 10·394 |
| III. Where of potato-soup,      ″     ″ | 4·04 | 1·46 | 16·25 | 19·58 | 10·581 |
| IV. Where of potatoes and milk, . .   ″ | 4·41 | 1·60 | 20·06 | 23·65 | 12·711 |
| V. Where of 12 oz. of fish with bread,   ″ | 5·08 | 1·92 | 12·15 | 16·62 | 9·527 |
| Average, . . . . . . . . | 4·49 | 1·61 | 15·82 | 19·50 | 10·701 |

And for the *third* or highest rate.

TABLE V.

|  | | Flesh-formers | Fat. | Starch. | Starch Equiv. | Carbon. |
|---|---|---|---|---|---|---|
|  | | oz. | oz. | oz. | oz. | oz. |
| I. Where the dinner is of broth, | . Totals, | 4·78 | 2·18 | 18·13 | 22·51 | 12·110 |
| II. Where of pease soup, . . . . | . ″ | 5·42 | 2·19 | 17·55 | 21·96 | 12·212 |
| III. Where of potato-soup, . . . | . ″ | 4·68 | 2·12 | 18·77 | 22·99 | 12·398 |
| IV. Where of potatoes and milk, | . . ″ | 5·06 | 2·26 | 22·48 | 27·05 | 14·527 |
| V. Where of fish and bread, | . . . ″ | 5·72 | 2·48 | 14·67 | 20·03 | 11·344 |
| Average, . . . . . . . . | | 5·13 | 2·25 | 18·32 | 22·91 | 12·518 |

Prisoners whose terms of sentence exceed two, but do not extend beyond six months, if not employed at hard labour, are put on the *second* rate; where they are so employed they are put on this regimen when their terms of imprisonment are upwards of ten, but not beyond sixty days.

The *third* rate is administered to all prisoners whose sentences exceed six months, if they are not employed at hard labour, and in all cases of hard labour above sixty days; also to all prisoners under sentence of transportation or penal servitude.

Special diet may, of course, be ordered by the surgeon in any case which may seem to him to demand deviation from the rules, but such exceptions are necessarily excluded from calculation in the present communication.

We have now to compare the results arrived at with those shown in the tables taken from Dr Playfair's paper before noticed. He divides diet into five degrees (p. 19) as follows:—

1. Subsistence diet, or the amount of nutrition furnished to convalescent patients in our hospitals, while yet they are unable to take active bodily exercise.  2. Diet in quietude, or that required for a very moderate amount of exercise.  3. Diet of adult in full health, such as is furnished to our soldiers in time of peace.  4. Diet of active labourers, or those doing a fair but not excessive amount of active work, represented by a walk of twenty miles during six days of the week.  5. Diet of hard-worked labourers.

The following table shows the comparative value of these diets, and those of the average rates of prison diet, in ounces:—

TABLE VI.

|  | Flesh F. | Fat. | Starch, &c. | Starch Equiv. | Carbon. |
|---|---|---|---|---|---|
|  | oz. | oz. | oz. | oz. | oz. |
| Subsistence diet, . . . . . | 2·0 | 0·5 | 12·0 | 13·2 | 6·7 |
| Diet in quietude, . . . . . | 2·5 | 1·0 | 12·0 | 14·4 | 7·4 |
| Diet of adult in full health, . | 4·2 | 1·8 | 18·7 | 22·0 | 11·9 |
| Diet of active labourer, . . . | 5·5 | 2·5 | 20·0 | 26·0 | 13·7 |
| Diet of hard-worked do., . . | 6·5 | 2·5 | 20·0 | 26·0 | 14·3 |
| Average of 1st rate prison diet, | 3·8 | 1·3 | 13·0 | 16·0 | 8·7 |
| Average of 2d       do., . | 4·5 | 1·6 | 15·8 | 19·5 | 10·7 |
| Average of 3d       do., . | 5·1 | 2·2 | 18·3 | 22·9 | 12·5 |

If we keep our attention fixed on the amount of carbon in the last column, and compare that of each of the three different rates with that of the five kinds of diet in the table, we shall observe that the *lowest* rate has a slight advantage over the diet of quietude, while it falls very considerably short of what is needed for sustaining the adult in good health. If the carbon here be represented by 100, the ratios of these divisions of diet will be 85 and 137 respectively.

In the *second* or middle rate of prison fare, if the carbon be compared with that of the same divisions, it will be found to be in the ratio of 100 to 69 of the first, and 111 of the second, being still below the required amount for the health-standard.

In the *third* or highest rate the ratio is, in respect of the healthy adult diet, as 100 to 95, being somewhat above the requirements of the latter; but in respect of the diet required for the active labourer, it stands as 100 to 109, again considerably below the mark, and as 100 to 114 in respect of that of the hard-worked labourer.

Below is a table of ratios, which shows at a glance the comparative values of the different rates of prison fare as compared with Dr Playfair's tables.

TABLE VII.—*Ratios.*

| PRISON DIET. | Subsistence diet. | Diet in quietude. | Diet of adult in good health. | Diet of active labourer. | Diet of hard-worked labourer. |
|---|---|---|---|---|---|
| Flesh formers : | | | | | |
| 1st rate, as 100 is to | 52 | 66 | 110 | 144 | 171 |
| 2d rate,        " | 44 | 55 | 93 | 122 | 144 |
| 3d rate,        " | 39 | 49 | 82 | 108 | 127 |
| Fat : | | | | | |
| 1st rate,        " | 39 | 77 | 138 | 192 | 192 |
| 2d rate,        " | 31 | 62 | 112 | 150 | 150 |
| 3d rate,        " | 22 | 45 | 82 | 113 | 113 |
| Starch : | | | | | |
| 1st rate,        " | 92 | 92 | 144 | 154 | 154 |
| 2d rate,        " | 76 | 76 | 118 | 126 | 126 |
| 3d rate,        " | 66 | 66 | 102 | 109 | 109 |
| Starch equivalents : | | | | | |
| 1st rate,        " | 82 | 90 | 137 | 162 | 162 |
| 2d rate,        " | 67 | 74 | 113 | 133 | 133 |
| 3d rate,        " | 58 | 63 | 96 | 113 | 113 |
| Carbon : | | | | | |
| 1st rate,        " | 77 | 85 | 137 | 156 | 164 |
| 2d rate,        " | 62 | 69 | 111 | 128 | 133 |
| 3d rate,        " | 53 | 59 | 95 | 109 | 114 |

Here it is seen that all the rates are considerably above the lowest, or subsistence diet, and also that of quietude; but the first rate of prison fare falls in the flesh-formers 10 per cent., in the fat 38 per cent., in the starch, starch equivalents and carbon, 44 and 37 per cent. below what is needed to keep the animal frame in full health, while the prisoner may be required to undergo a hard labour

sentence for the period of ten days without additional nourishment.

If we direct our attention to the second class fare, we shall see that while it is 7 per cent. in advance in flesh-forming substances, it falls 12 per cent. behind in the fat and 18 per cent. in the starch materials, and 11 per cent. in the estimated amount of carbon, below the diet required for the maintenance of the body in full vigour, and on this diet a prisoner may be kept for six months without, or sixty days with hard labour.

The third, or highest prison rate, shows a proportion of 18 pe cent. of the flesh-forming materials, and 4 per cent. in the starch equivalents, and 5 per cent of carbon, *above* the full health diet, but falls 8, 13, and 9 per cent. in these respects, below the active labourer's diet, and on this fare a prisoner may be kept to expiate a sentence however long, accompanied with hard labour.  ·

How does this affect the condition of the inmates of our various prisons? Do they lose weight as a general rule? A variety of local circumstances, such as the character of the population, rural or manufacturing, and its general social condition may influence the results in different parts of the country, but we believe that the statistics collected by Dr Thomson of the General Prison, Perth, from every jail in Scotland, in answer to special inquiries on the subject, will show that in respect of the first rate of diet, nearly *one-fourth* of the males, and about 17·5 per cent. of females lose weight; in respect of the second rate, 18·7 per cent. of males, and 12·4 per cent. of females fall off; and with regard to the third rate, 18·5 of the male, and 14·5 of the females deteriorate.

Many questions open up in investigating this interesting subject. Such as the influence of previous habits on the health and condition of prisoners; that of mental emotion, as evinced in first, in comparison with subsequent convictions; that of various employments as affecting the vigour and stamina of prisoners subjected to much the same routine in diet and exercise, etc. These, again, may be influenced, in some degree, by the tone and general character of the population of the district, as the simple and usually more highly moral[1] state of the rural, compared with the sterner and more vicious and depraved of our large manufacturing cities. But whatever may be the influence of these disturbing causes, we think,

---

[1] I take *morality*, here, in its widest sense, especially in those aspects which bear most materially on the health of the individual; such as the avoidance of dissipation, with its various animal excitements, late hours, irregular meals, and stinted rest, in which the rural population have such an obvious advantage over the depraved criminal class of our large towns.

I am fully aware that, in a certain respect, as borne out by the Registrar General, the morality of our rural friends is said to fall sadly behind that of the same class in our cities; but I take this as a clear evidence,—indeed, as the one exception which proves the rule,—that in tone and vigour of body they are ahead of their town contemporaries, and, accordingly, that the falling off in condition of such prisoners is a thing to be anticipated.

that as a foundation for all statistics, and for all calculations on the subject of prison dietary, we ought to assume that the latter is, at the lowest, sufficient to maintain the *physique* of the adult up to the mark of a healthy standard, which at present it is not.

For juveniles and females a ratio ought to be fixed suited to their requirements. They ought not, as at present, to be slumped with the male adult and subjected to the same rules, without modification. In general, I think them over-fed, and the statistics last alluded to show how few of the females comparatively lose weight under any of the different rates.

In conclusion, I think that if it be laid down as a recognised rule that prisoners are not to be pampered or over-fed, so as to make the prison an attraction to the idly or viciously inclined, the converse should hold good, that in no case ought the dietary to be so scanty and indifferent as in any degree to impair the *vis vitæ* a man may possess on his entrance there, but rather to repair the ravages which crime and its concomitants generally make on his frame, so that his punishment may make him for the future, not only a wiser, but in this, as well as in other respects, a *better* man. I conceive that he ought to be set at liberty better prepared, physically as well as morally, for fighting the great battle of life anew, and the very experience he may have had in his confinement, of the *animal comforts* of regularity and sufficiency, may form a strong inducement for him in after-life to prefer and strive after these in an honest way, instead of exposing himself to the uncertainties and privations of dissipation and crime.

---

ARTICLE VIII.—*The Chief Medical Schools of the Continent.* By WILLIAM RUTHERFORD, M.D. Edin., Assistant in the Physiological Laboratory, Edinburgh University.

A SHORT account of the chief Continental Medical Schools will doubtless prove serviceable to those who may purpose visiting them, and will, it is hoped, not be without interest to those who are prevented from so doing. Among students generally very vague notions prevail as to the " how, when, and where," of continental medical travel : they have unfortunately no directory for continental schools similar to that contained in " Churchill" for the schools of this country (a deficiency which, if supplied, would prove of the greatest service not only to students but to the medical men of this country generally, very many of whom are anxious to be informed, from year to year, of changes taking place in continental schools). Most students entertain the idea that to spend a year on the Continent would be an advisable thing to do. Some are fortunate enough to be acquainted with men who have visited the

various schools, and from whom they can obtain the requisite information concerning them; not a few, however, abandon the idea altogether, owing to their being insufficiently informed of the advantages to be derived from its being carried out; and many of those who set out start without any definite idea as to how they mean to spend their time, and, in consequence, lose much of it when at the school, from their not knowing who the best men are; others lay hold of a celebrated name, and visit the school with that one idea, so to speak, and not unfrequently discover, when too late, that there are other men there from whom they might have learned quite as much, perhaps a great deal more. Often enough too much time is thrown away in attempting too much at one school, running after everything and everybody, instead of singling out the specialties of each, and spending all one's time on them.

The number of students from this country at continental schools is very small, compared with what it would doubtless be were the importance of such travel sufficiently dwelt upon. During the past winter there were only five English medical students in Berlin, and in Vienna during the spring there were only six; in Paris, however, the number is usually considerably greater.

Undoubtedly the best time to visit the Continent is immediately after the regular curriculum has been completed; for if any permanent appointment be entered upon, it is usually impossible to procure the necessary time. The length of time occupied by the tour will of course vary with each individual; but it ought not, if possible, to be less than nine months; during which period the three chief schools—Berlin, Vienna, and Paris—may be very conveniently visited. The best manner in which to dispose of the time is to divide it equally between the three; and one ought to be careful to avoid falling into the snare of lingering too long at one, at expense of the time devoted to the others. One who intends becoming a pure surgeon may with reason spend longer time at Paris and Berlin than at Vienna, but for the general student an equal time at all is best.

There are many schools worthy of a visit; but though Tübingen, Heidelberg, Utrecht, Prague, and Lyons, have all prominent attractions, they are not by any means equal to Paris, Vienna, and Berlin, which are the resort of most visiters. As regards the time of year at which these three should be visited, it may be stated that Paris is best in winter and Berlin in summer, while Vienna varies but little in this respect. The two great German schools differ from each other in many respects: Berlin is more scientific; Vienna more practical, being in fact the great clinical school of Germany. In Vienna there are several large hospitals; but happily the men whom one goes to see are all to be found in one,—an hospital of enormous dimensions, forming in itself quite a small town. This centralization is a most convenient thing for the student; there is no running from one hospital to another several miles distant, as in

Paris and London. The hours of the various cliniques are also admirably arranged, one subject not interfering with another. In Paris they are nearly all crowded together between 8 and 11 A.M., so that it is impossible to attend a clinique on one subject without interfering with many others; whereas in Vienna there is a succession of cliniques from 7 A.M. till 5 P.M. In Berlin they are pretty well arranged; though, to be sure, Graefe and Frerichs lecture at the same hour, and thereby it is impossible to attend both on the same day. With existing arrangements, however, it would be possible to do a great deal more in Berlin, were some of her professors more punctual. Of course every one knows that a German seldom pretends to punctuality, and that their "*academical* hour" is twenty minutes after every other hour. It is quite delightful to see how thoroughly German students are in training, as regards the exercise of that most amiable quality, patience. I have repeatedly seen them await the arrival of a learned professor for a period varying from half-an-hour to an hour after the appointed time, and allow him to begin his lecture without an apology for the delay, without the faintest murmur on their part. In fact, German students are apparently a most tractable body in everything but politics. In class-rooms they are docile and passive, and are not given to knocking the dust off the floor, or other unwelcome demonstration of applause, such as an English professor is often troubled to control; and no such sinful thought as that of perpetrating a practical joke during the lecture seems ever to enter their heads; nay more, they attend their classes most assiduously, without the aid of cards or roll-call. Debating societies are not much patronized; and it is the custom of most to spend one or two nights weekly at one of their many friendly societies, where beer, tobacco, and those delightful part-songs form the cheer. "Affairs of honour" are common enough; the duellists seldom, however, give each other more than a scratch on the cheek, which is considered perfectly satisfactory for all honourable purposes.

Medical education is considerably cheaper on the Continent than in this country; but for the English visiter the schools are not alike in this respect. In Paris, if he be qualified, he can attend all the cliniques, excepting some of those on eye diseases, free of charge. In Berlin and Vienna, he will in general require to pay, but an introduction to the professor is frequently all that is required to procure admission to his clinique. Medicine and surgery are chiefly taught clinically: the systematic lectures on these subjects being in general but poorly attended. Clinical medicine is best taught in Vienna. Skoda's clinique is very good, but his instructions are more adapted to the beginner; whereas Oppolzer's is more fitted for the advanced student, and yet conducted with such admirable tact and skill that all can derive great benefit. Oppolzer has not written much, and is in consequence not so well known in this country as he ought to be, for he is a singularly accomplished physician, *facile primus* in

Vienna, and the best teacher of clinical medicine I could find in the three capitals. Frerichs and Traube, in Berlin, are both excellent. Traube is professor of auscultation and percussion, and holds his clinique at the bedside. Frerichs, who is professor of clinical medicine proper, adopts the rather unusual system of having each patient carried in bed into a large lecture-room. This plan enables everyone to see the patient comfortably, and hear what Frerichs says; but it does not admit of any but one or two obtaining a near view of the patient, or of using the stethoscope after the patient is brought in. A student is called out to examine the case, which, however, Frerichs almost invariably does for him. After a brief history has been read by one of his assistants, Frerichs remarks on the case, generally very briefly, but always to the point. The course is very interesting for those who care only for a sketch of a case; but the time occupied by each might be extended with advantage, for Frerichs, who is a very popular teacher, just says enough to make one wish he would say a great deal more. In Paris, the physicians make but little use of the enormous amount of matériel at their disposal. Trousseau, who has resumed his lectures on the actions and uses of medicines, and is no longer professor of clinical medicine, though he still retains his wards at Hotel Dieu, does not attempt to teach clinically, and is in consequence accompanied by but a handful of students. His lectures on materia medica, however, attract large crowds; for he is a very prince of orators, and can render his subject not only interesting but singularly attractive. Piorry confines himself almost entirely to percussion. Barth and Beau are excellent auscultators. Bouillaud still "thirsts for blood," and his clinique is in consequence most interesting to English students, many of whom pass through their curriculum without ever seeing a venesection. Perhaps, however, the most instructive medical clinique in Paris is Grisolle's, at Hotel Dieu; but I must say I never could find one at all equal to Oppolzer's in Vienna.

The best surgical schools are those of Paris and Berlin; Vienna is a long way behind either. In Paris there are at least six surgeons well worth visiting,—Velpeau, Nélaton, Maisonneuve, Giraldés, Civiale, and Desormeaux. Velpeau, notwithstanding his advanced age, still operates beautifully, and one occasionally sees good cases in his wards. Nélaton is most instructive, but difficult to catch as he allows his practice considerably to interfere with his attendance at the hospital. Maisonneuve's clinique is largely attended; he has usually a capital selection of cases; his dressings are good, and some of his instruments and operative procedures most ingenious. Giraldés is always interesting; he is one of the most learned and accomplished surgeons in Paris. A good deal may be learned from Civiale, who has always good urinary cases; and that most obliging gentleman Desormeaux is ever ready to show his bladder speculum or endoscope, an instrument likely to come into pretty general use.

Unfortunately, Chassaignac has been compelled by ill-health to resign his hospital appointment. Parisian surgery is characterized by great ingenuity in many of the measures adopted for the removal of disease, but it is unfortunately attended by a fatality to life truly deplorable. One sees much that is curious, but little that one would care to carry away and put in practice. Very different and far superior to that of Paris is the surgery of Berlin; there are to be found surgeons at once ingenious, careful, and successful, from whom much that is useful may be learned. For my own part, I have no hesitation in saying, that Berlin is a far better surgical school than Paris. The two principal surgeons are Langenbeck and Willms; the latter is unfortunately as yet unconnected with the medical school, and has no regular clinique; moreover, his hospital is in a suburb, so far removed from the central parts of the town that it is impossible to visit it often. It is delightful, however, to go through his many wards, and see such numbers of well-formed stumps and healthy-looking wounds. The dressings are much the same as those used in England. He has usually an admirable collection of in-structive cases, and is a dexterous and careful operator. Langen-beck, whom one has the pleasure of seeing for two hours daily, has the best surgical clinique I have seen in all these three schools; he has always something interesting to show, and his remarks display an amount of surgical nous and a depth of learning in all things medical seldom met with; he is perfectly conversant with the latest doings of other surgeons, and is ever ready to give their proposals a fair trial; he operates admirably; and though he is not so successful as Willms in amputations, in the performance of plastic operations and excisions he has an unparalleled success. During the summer he personally superintends an operative course, which, I have been informed by those who have attended both, is superior to those given in Paris; not, indeed, as regards the quantity of matériel, for that is impossible in a town so small as Berlin, when compared with Paris, but on account of the teaching. The courses in Paris, though conducted with great ability, are all under the care of young surgeons, whose experience cannot of course be compared with that of a surgeon like Langenbeck. Those, however, who mean to pay special attention to surgery would do well to take both courses; but, to enable them to do so, they must remember that the Berlin course is only held in summer, while those in Paris, though held during both winter and summer, are best in winter. Jungken is an old surgeon in Berlin worth visiting; his surgery, however, is considerably behind that of the present day. With Gurlt, professor of systematic surgery, one is unfortunately not much thrown into contact, as he has no wards; but I know no man more able and ready to give one all sorts of hints concerning not only the surgery of his own country but also that of every other.

The great school for obstetrics ought to be Vienna, for the

quantity of matériel is enormous, far surpassing, I believe, that of any other continental hospital; but the tuition is so desultory and unsatisfactory that but little can be learned. I have been repeatedly told by Germans that by far the best school for this subject is Prague.

For skin diseases the greatest of all schools are Vienna and Paris. Hebra at the former, Hardy and Bazin at the latter, are all three admirable teachers and exponents of their various opinions. Hebra and Hardy are especially interesting. Hebra's lectures abound with amusing anecdote, and are filled with acute observation and sound common sense; while Hardy is one of the cleverest, clearest, and most concise lecturers I ever listened to.

Syphilis, though it can be pretty well studied in Paris, can be more conveniently and better done in Vienna, under the singularly able tuition of that kindest and most accomplished of men, Professor Sigmund. One ought frequently to go with this gentleman, not only to his regular clinique, but round his wards, where he is unceasingly attentive to visiters; there I imagine the greatest sceptic could hardly fail to be convinced that mercury may be used in the treatment of syphilis without producing any of those injurious effects to which its abuse has given rise; and there are few who will accompany Professor Sigmund round his wards long, without agreeing with him, that mercury, when judiciously and carefully employed, is of the greatest service in the treatment of the above-mentioned disease.

The eye is well taught in all the three schools. Graefe in Berlin, Arlt in Vienna, Liebreich and Wecker in Paris, are all the best of teachers.

The best school for pathology is undoubtedly Berlin. The pathological institute of that school is admirably arranged, and includes two rooms for the performance of sectios, a histological laboratory, a laboratory for animal chemistry, and two class-rooms—one for lectures on general pathology, the other for those on morbid anatomy. Virchow, who superintends the whole, lectures four times a-week on general pathology, and twice a-week on morbid anatomy; his lectures are always singularly interesting and instructive, and form the best course on pathology given on the Continent. There are several sectios daily performed by Virchow's clever assistants, Drs Klebs and Cohnheim; moreover, the laboratories are always open, and a course of histology is conducted by Virchow himself; while the chemical laboratory is superintended and a course of lectures on animal chemistry given by Dr Kühne, an excellent chemist and accomplished physiologist and pathologist. In Vienna, one may see a greater number of post-mortems daily, but they are so hurriedly performed that but little advantage can be derived from them, and Rokitansky, though possessing a universal renown, is not successful as a teacher. In Paris, pathology

cannot be at all well studied; the Berlin school totally eclipses the Parisian.

Physiology may be well studied at all the three schools. In Berlin the electrical experiments on muscle and nerve, performed by Du Bois-Reymond's excellent assistant Rosenthal, are the chief attraction; in Paris, in Claude Bernard's laboratory, experiments on the salivary, pancreatic, and biliary secretions, as well as other vivisections, are mostly performed; while in Vienna, with Professor Brücke, that deservedly celebrated physiologist, histology is chiefly pursued.

Altogether, the teaching in these three great schools is excellent. As with all schools, both in this and every other country, at no single one can every subject be equally well studied, and the only remedy consists in visiting several good schools. German students show us a good example in this respect. Instead of confining themselves entirely to one school, they generally visit several in their own country, and select the best men in each. In Paris, moreover, students see a diversity of practice far exceeding even that seen by most students in London; for, instead of a separate school attached to nearly every hospital, as in the latter town, where students pass their years sometimes without ever entering another hospital, though it be but a few yards from their own, Paris students belong to one great general school, to which all the hospitals are equally open.

In comparing the schools of this country with those of the Continent, it may be stated generally that specialties may be best studied at the latter, while the three great practical departments of medicine can be best learned at the former.

In fine, it is to be desired that the intercourse between the medical schools of this country and those of the Continent may continue to increase; reciprocal benefit will be derived by all; for the importance of knowing thoroughly those languages in which the best medical literature is contained, of becoming acquainted with those men who are the champions of medical science, of hearing the authors of some of the most important theories in medicine express and support them with their own lips, of being enabled to judge of the relative merits of various methods of treatment, by seeing them put in practice on an extensive scale cannot be over-estimated. These, together with the importance of travelling, are considerations as important to the students and medical men of other countries as those of our own.

## INFORMATION REGARDING MEDICAL EDUCATION AND EXAMINATIONS.

We commence, as on former occasions, by giving the Recommendations of the General Medical Council, as agreed to at its last meeting.

*Recommendations of the General Medical Council, on the subjects of Preliminary Examination, of Registration of Medical Students, and of Professional Education and Examination.—1865.*

### I. PRELIMINARY EXAMINATION.

1. That testimonials of proficiency granted by the national educational bodies according to the subjoined list, may be accepted, the Council reserving the right to add to, or take from, the list.

  1. A Degree in Arts of any University of the United Kingdom, or of the Colonies, or of such other Universities as may be specially recognised from time to time by the Medical Council.
  2. Oxford Responsions or Moderations.
  3. Cambridge Previous Examinations.
  4. Matriculation Examination of the University of London.
  5. Oxford Middle Class Examinations (Senior).
  6. Cambridge Middle Class Examinations (Senior).
  7. Durham Middle Class Examinations (Senior).
  8. Durham Examinations for Students in Arts, in their second and first years.
  9. Durham Registration Examination for Medical Students.
  10. Dublin University Entrance Examination.
  11. Queen's University, Ireland, two years' Arts Course for the Diploma of Licentiate in Arts.
  12. Preliminary Examinations at the end of A.B. course.
  13. Middle Class Examinations.
  14. Matriculation Examinations.
  15. First Class Certificate of the College of Preceptors.
  16. " Testamur " granted by Codrington College, Barbadoes.
  17. Degree of Associate of Arts granted by the Tasmanian Council of Education, with a certificate that the Student has been examined in Latin and Mathematics.

2. That students who cannot produce any of the testimonials referred to in the first recommendation be required to pass an examination in Arts, established by any of the bodies named in Schedule (A) of the Medical Act, and approved by the General Medical Council.

3. That the examination in general education be eventually left entirely to the examining boards of the national educational bodies recognised by the Medical Council.

4. That no certificate of proficiency in general education, which does not affirm the proficiency of the candidate in Latin, be deemed a sufficient proof of preliminary education previous to the commencement of professional studies.

5. That the various educational and licensing bodies be requested to transmit to the Registrar of the General Council, returns, embodying any alterations which they may from time to time introduce into their courses of general study and examinations, which qualify for the registration of medical students; and that a copy of such returns be sent by the Registrar, as soon as convenient, to each member of the General Council.

## II. Registration of Medical Students.

1. That the registration of medical students be placed under the charge of the Branch Registrars.

2. That every medical student shall be registered at the commencement of his professional study; and not until he has passed the preliminary examination.

3. That each of the Branch Registrars shall open a Register of Medical Students, according to the subjoined form; and that application for registration be made by every such student within fifteen days after the commencement of professional study.

*Form of Application for the Registration of Medical Students.*

| Date of Registration. | Name. | Preliminary Examination in Arts, and Date. | Place of Study. |
|---|---|---|---|
|  |  |  |  |

4. That every person desirous of being registered as a medical student shall apply (in writing) to the Branch Registrar of the division of the United Kingdom in which he is residing, according to a form to be had on application to the several qualifying bodies, medical schools, and hospitals, and shall produce or forward to the Branch Registrar a certificate of his having passed a preliminary examination in Arts recognised by the General Medical Council, and of his place of study: whereupon the said Branch Registrar shall enter the said applicant's name and other particulars in the Students' Register, and the Registrar shall give him a certificate of such registration accordingly; and that each of the Branch Registrars shall supply to the several qualifying bodies, medical schools, and hospitals in that part of the United Kingdom of which he is Registrar, a sufficient number of blank forms of application for the registration of medical students.

5. That a copy of the Register of Medical Students so prepared by the Branch Registrars be transmitted to the Registrar of the General Council, who shall, under direction of the Executive Committee, prepare and print an alphabetical list of all registered students, and supply a copy of such authorized list to each of the bodies enumerated in Schedule (A) to the Medical Act.

6. That the several licensing bodies be requested not to admit to examination, after October 1869, any candidate for license or degree whose name does not appear on the authorized list of medical students, or whose name is not already on the *Medical Register*.

7. That the several Branch Councils shall have power to admit special exceptions to the foregoing regulations as to the registration, for reasons which shall appear to them satisfactory.

8. That the Branch Councils be desired to take means to make these regulations known to the medical students at the various medical schools.

## III. Age for License to Practise.

1. That the age of twenty-one be the earliest age at which a candidate for any professional license shall be admitted to his final examination; that the age shall in all instances be duly certified; and that a return of any exceptions to this recommendation allowed by the licensing bodies, together with the reasons for such exceptions, be transmitted to the Branch Council of that part of the United Kingdom in which they have been granted.

2. That no license be obtained at an earlier period than after the expiration of forty-eight months subsequent to the registration of the candidate as a medical student.

## IV. Professional Education.

1. That the course of professional study required for a license shall comprehend attendance during not less than four winter sessions, or three winter and two summer sessions, at a school recognised by any of the licensing bodies mentioned in Schedule (A) to the Medical Act.

2. That it be recommended to the several licensing bodies that the courses of instruction required by them be framed in such a manner as to secure a due share of attention, both to preparatory branches and to those more strictly connected with the practice of Medicine and Surgery; and that it be suggested accordingly to these bodies, that their regulations should be such as to prevent attendance upon lectures from interfering with hospital and clinical study.

3. That the Council will view with approbation any encouragement held out by the licensing bodies to students to prosecute the study of the natural sciences before they engage in studies of a strictly professional character.

## V. Professional Examination.

1. That those licensing bodies which have not already done so, be requested to furnish a statement of the dates of their examinations and of the modes in which such examinations are conducted, whether by written, oral, or practical examination, and of the length of time a candidate is under examination in each or all of these ways; and that the Registrar transmit these reports to the members of the Council, in order that they may be taken into consideration at the next meeting of the several Branch Councils.

2. That the professional examination for any license be divided into two parts; the first embracing the primary or fundamental branches of Medicine; and the second the branches directly connected with the Practice of Medicine and Surgery; that the former be not undergone till after the close of the winter session of the second year of professional study; and the latter or final examination, not till after the close of the prescribed period of professional study.

3. That the examination in Physics, Botany, and Natural History may be undergone at an earlier period than the first professional examination.

4. That the professional examinations be conducted both in writing and orally; and that they be practical in all branches in which they admit of being so.

5. That the professional examinations be held by the several licensing bodies, except in special cases, at stated periods, to be publicly notified.

6. That returns from the licensing bodies in Schedule (A) be made annually, on the 1st of January, and in the subjoined form, to the General Medical Council, stating the number and names of the candidates who have passed their first as well as their second examinations, and the number of those who have been rejected at the first and second examinations respectively; and that the Registrar forward a sufficient number of forms, with a notice for their being returned in due time.

*Form of Return of Examinations, and their Results.*

| PASSED. | | | | REJECTED. | |
|---|---|---|---|---|---|
| 1st Examination. | | 2d Examination. | | 1st Examination. | 2d Examination. |
| No. | Name. | No. | Name. | No. | No. |
| | | | | | |

7. That it be recommended to all the examining boards that they should require from every candidate for examination before them, a statement, signed by himself, whether he has or has not been rejected within three months by any of the examining boards included in Schedule (A) of the Medical Act.

8. That it is not desirable that any University of the United Kingdom should confer any degree in Medicine or Surgery, whether that of bachelor, doctor, or master, upon candidates who have not graduated in Arts, or passed all the examinations required for the Bachelorship in Arts, or the examinations equivalent to those required for a degree in Arts.

## VI. Supervision of Examinations.

That each of the Branch Councils, or such of their members as may be deputed by such Councils, shall, from time to time, visit the examinations, preliminary as well as professional, conducted by the qualifying bodies in their respective divisions of the United Kingdom, and report the results of their observations to the General Council.

---

On the Recommendations contained under these different heads we have a few remarks to make.

## I. Preliminary Examinations.

From its first institution the Medical Council has shown a laudable desire to promote the general education of those who propose to become members of the medical profession. A reform in this respect was most urgently required. In no other of the learned professions was there such a large number of members who could make no pretensions to a liberal education. Not merely was a large proportion ignorant of Latin, too many possessed a very imperfect knowledge of their own language. This unfortunate state of matters was due to various causes, among which must be mentioned the assistant system. Imperfectly educated lads were selected, at first to take charge of the surgery and dispense medicines; in course of time they were allowed to attend midwifery cases, and assist in the general practice; they married and had families dependent upon them; and not unfrequently, by the ill-advised leniency of some of the licensing bodies, were permitted to obtain qualifications, although their professional, and still more their general education was very deficient. The multiplication of provincial medical schools has operated in the same direction. The schools themselves were, in many instances, inferior; while the diminished cost of the education supplied by them attracted a class of students who could not have afforded the expense of pursuing their studies at a distance from home. In consequence of the operation of these and other causes, a class of men have been admitted as medical practitioners, who were by no means qualified to become members of a liberal profession. The result has been most unfortunate; for, as a whole, the medical profession does not occupy that social position to which it is entitled, and has failed to secure that general influence which it ought to possess. The first step towards improvement in these respects lies undoubtedly in raising the standard of general education among medical students; and this can only be efficiently done by the establishment of preliminary examinations. The only possible objection which could be made to this proposal was, that there was a danger of the standard being fixed too high, and that there might consequently be a difficulty in obtaining a sufficient number of medical practitioners to supply the wants of the country. This objection is

not altogether without weight, for there are many localities, particularly in Scotland and Ireland, where the prospect of remuneration is so inconsiderable as not to justify the outlay of a large amount of capital in general or professional education. At the same time it must be borne in mind that the requirements at the preliminary examinations are not such as should exclude lads who have profited from instruction at the grammar schools, or even at the parish schools of Scotland. Besides, in Scotland the facilities for attending college are such that the sons of men in comparatively humble circumstances can generally have the benefit of attendance for one or two sessions. But were the objection much more formidable than it really is, we would not be inclined on that account to lower the standard. We would rather see the establishment of a lower grade of practitioners, corresponding perhaps to the *Officiers de Santé*, than consent to the continued degradation of a whole profession.

The examinations in general education should certainly be left in the hands of the national educational bodies. This, we doubt not, will eventually be the case ; and the result will be much facilitated by the establishment of middle class examinations, which have proved successful in England, and more lately in Scotland. In the meantime, however, the General Council have agreed to accept certificates as to attainments in general education from any of the qualified licensing bodies. The sooner that this is done away with the better ; for the public will very justly regard with greater confidence the certificate of an independent body than of one which may be supposed to have an interest in increasing the number of medical students. The General Council is somewhat indefinite as to its requirements in general education. All that it absolutely requires is " proficiency in Latin." No doubt, a knowledge of Latin is the best single test of a liberal education, but the licensing bodies have themselves fixed on a higher standard. Besides a competent knowledge of English and Latin, the candidates are generally required to pass in one or more additional subjects, which may be selected at the option of the candidates from the modern languages, or from the natural or physical sciences.

We trust that the licensing bodies will do their best to keep up the standard of general education, and if they will act in concert they have the matter in their own hands. An important step has been taken by the Royal College of Physicians of Edinburgh, which has decided to institute a Scholarship of the annual value of Twenty-five pounds, tenable for two years. The candidates must be those who commence their medical studies in Edinburgh in November next, and the subjects of examination are Latin, Greek, Roman and Grecian Antiquities, and Ancient Geography. The examination for the first Scholarship is to be held on 23d and 24th of the present month.

## II. REGISTRATION OF MEDICAL STUDENTS.

Under this head a very important decision has been come to by the General Council; it is, that no one shall be registered as a medical student until he has passed the preliminary examination. This decision is most important, for upon it the anticipated improvement in general education mainly depends. When the candidate was not required to pass the preliminary examination previous to the commencement of his medical studies, he very commonly postponed it till the latest possible period,—in fact, till immediately before he

went in for his professional examination. Under these circumstances, the Examiners were placed in a difficult position. The candidate might have gone through his whole medical curriculum, and might be anxious at once to enter upon practice; and it certainly seemed hard that he should be liable to rejection for ignorance of non-professional subjects. There is no doubt, accordingly, that many were indebted for passing to the good nature of the Examiners. This difficulty will now be removed, and in future no one will be permitted to enter upon his medical studies until he has proved that he has received and profited by a fair general education. There will be no hardship in remitting to their studies those who by their failure have shown that they are not yet in a condition to enter upon special professional training. The various licensing bodies have all, we believe, agreed to the recommendation of the General Council. The regulation will not apply to those who commenced their medical studies previous to the approaching session, but henceforth it will be obligatory; and it is recommended that, after October 1869, no candidate for license or degree whose name does not appear in the *Medical Register* or in the authorized List of Medical Students shall be admitted to examination.

### III. AGE FOR LICENSE TO PRACTISE.

Few, we think, will be inclined to dissent from the recommendation, that twenty-one should be the earliest period at which a professional qualification should be granted. There may be exceptional cases, but, as a rule, men are not prepared to enter upon the responsibilities of practice until they have attained their legal majority. We do not, however, approve of the second recommendation under this head, "that no license be obtained at an earlier period than after the expiration of forty-eight months subsequent to the registration of the candidate as a medical student." This regulation would be in many cases attended with considerable hardship. Suppose a student to have passed his preliminary examination, and to have been registered as a medical student in October 1865, he would not be allowed to appear for his professional examination till October 1869, although he might have gone through the entire curriculum, and fulfilled the requirements of the Council as to attendance at medical schools by the previous April. It certainly seems to us unnecessary that a delay of six months should be interposed, and it can readily be understood that cases of great individual hardship might occur. We would therefore propose that, in the recommendation of the Council, the words "forty-two" should be substituted for "forty-eight."

Under the fourth and fifth heads, we have no special remarks to make.

### VI. SUPERVISION OF EXAMINATIONS.

The supervision of examinations, if efficiently carried out, cannot fail to be attended with good effects. It is most desirable that there should be as nearly as possible a fixed standard of medical proficiency, short of which no one should be allowed to receive a license to practise. But, in this country, in consequence of the number of licensing bodies, a feeling of rivalry necessarily grew up between them; and as these bodies are to a great extent dependent

for their income on the fees of their licentiates, it was for their interest to attract the greatest number of candidates. Consequently, a certain laxity in examinations at one time crept in; and, although we believe it has been to a great extent corrected, the carrying out of a well-devised system of supervision will prevent any dereliction from their duty on the part of any of the boards. The Scottish Branch Council carried out the plan during last summer, and the English and Irish Councils are preparing to do so too.

The following pages contain an abstract of the regulations of the various licensing bodies. We have thought it unnecessary to print the requirements of the different boards under the head of preliminary examinations. These requirements do not vary very widely, and the most recent information can at all times be obtained by addressing the Secretaries of the different bodies. Generally speaking, there are from two to four periods in the course of the year at which these examinations are held.

By the new statutes of the Scottish Universities, three medical degrees have been instituted, those, namely, of Bachelor of Medicine (M.B.), Master in Surgery (C.M.), and Doctor of Medicine (M.D.). The degree of C.M. is not, however, conferred on any one who does not at the same time obtain the degree of M.B. All candidates for the two first degrees are required to pass the full preliminary examinations, and to have been engaged in professional study for four years before being admitted to the final examination. The degree of M.D. may be conferred on any Bachelor of Medicine twenty-four years of age, who has been engaged, subsequently to his having received the degree of M.B., for at least two years in medical and surgical practice, provided that he is either a Graduate in Arts of a University, or that he has passed an examination in Greek and in Logic or Moral Philosophy, in addition to the other branches of a preliminary examination. Those, however, who had commenced their medical studies previous to the period at which the statutes came into force at the different universities, are entitled to graduate either under the system in force before that time, or under that now established, according as they may prefer to comply with the regulations in force before or after these dates. Consequently, those who began their medical studies previous to the dates mentioned below, may either take the degree of M.D. at the age of twenty-one, and without a preliminary examination, except in Latin; or they may obtain the degrees of M.B. and C.M.; in which cases, however, they will be required to pass the full preliminary examination. This alternative refers to candidates who began their medical studies before the following dates:—In Edinburgh, before the 4th of February 1861; in Glasgow, before the 1st of October 1861; and in Aberdeen, before the 5th of November 1861.

# COURSE OF STUDY REQUIRED BY THE VARIOUS BOARDS OF THE UNITED KINGDOM.

| | Age. | Anatomy. | Dissections. | Chemistry. | Practical Chemistry. | Materia Medica. | Physiology or Institutes of Medicine. | Surgery. | Practice of Medicine. | Midwifery. | Medical Jurisprudence. | Pathology or Morbid Anatomy. | Botany. | Natural History. | Practical Pharmacy. | Clinical Surgery. | Clinical Medicine. | Hospital Attendance. | Practical Midwifery. | Dispensary or Out-door Practice. | Vaccination. |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | Years | Mons. | Mons. | Mons. | Mons. | Mons. | Mons. | Mons. | Mons. | Mons. | Mons. | Mons. | Mons. | Mons. | Mons. | Mons. | Mons. | Mons. | Mons. | Mons. | |
| Edinburgh University, M.B. & C.M., | 21 | 6 | | 6 | 3 | 3 | 6 | 6 | 6 | 6 | 3 | 6 | 3 | | | 6 | 6 | 24 | { 8 mo, or 6 cas. | 6 | 6 |
| University of Glasgow, M.B. & C.M., | 21 | 6 | | 6 | 3 | 3 | 6 | 6 | 6 | 6 | 3 | 6 | 3 | | | 6 | 6 | 24 | do. | 6 | 6 |
| University of Aberdeen, M.B. & C.M., | 21 | 6 | | 6 | 3 | 3 | 6 | 6 | 6 | 6 | 3 | 6 | 3 | | | 6 | 6 | 24 | do. | 6 | 6 |
| University of St Andrews, M.D., | 22 | 12 | 12 | 6 | 3 | 6 | 6 | 6 | 6 | 6 | 3 | 6 | | | | | | 24 | | | |
| London University, M.B., | 21 | 6 | 15 | 6 | 1 crs | 1 crs | 3 | 6 | 6 | 1 crs | 3 | 6 | 3 | 3 | 1 crs | 2 yrs. | 2 yrs. | 4 yrs. | 20 cas. | 6 | 6 |
| University of Durham, M.B. & M.D., | 21 | 6 | | 6 | 3 ms | 3 | 6 | 6 | 6 | 6 | 3 | | 3 | | | | | | | | |
| Dublin University, M.B., | | | | | | | | | | | | | | | | | | | | | |
| Surgical Diploma, | | 18 | 18 | 6 | 3 | 3 | 18 | 18 | 6 | 6 | 3 | 6 | 3 | | | 18 | 18 | 18 | | | |
| The Queen's University of Ireland, M.D., | | 12 | 12 | 6 | 3 | 3 | 12 | 6 | 6 | 6 | 3 | | 3 | 3 | 3 mos. | 27 | 27 | 27 | 3 mo. | | |
| Royal College of Physicians, London, | 21 | 12 | 12 | 6 | 3 | 3 | 12 | 6 | 12 | 6 | 3 | | | | 3 | 12 | 12 | 24 | 20 cas. | | |
| Royal College of Physicians, Edinburgh, | 21 | 6 | 6 | 6 | 6 | 6 | 6 | 6 | 6 | 6 | 3 | 6 | | | 3 | 6 | 21 | 24 | 6 cases | | |
| King and Queen's Col. of Phys. Ireland, | 21 | 6 | 12 | 6 | 6 | 3 | 6 | 6 | 6 | 6 | 3 | | | | 3 | | 27 | 27 | 6 mo. | | Cert. |
| Royal College of Surgeons, London,* | 21 | 12 | 12 | 6 | 3 | 3 | 12* | 12 | 6 | 3 | 3 | | | | | 27* | 9* | 33 | | | Cert. |
| Royal College of Surgeons, Dublin, | 21 | 18 | 18 | 6 | 3 | 3 | 18 | 18 | 6 | 6 | 3 | | | | | 18 | 9 | 27 | | | Cert. |
| Royal College of Surgeons, Edinburgh, | 21 | 12 | 12 | 6 | 3 | 3 | 6 | 6 | 6 | 6 | 3 | | | | 3 | 6 or 12 | 6 | 24 | 6 cas. | 6 | Cert. |
| Faculty of Phys. and Surgeons, Glasgow, | 21 | 12 | 12 | 6 | 3 | 3 | 6 | 6 or 12 | 6 | 6 | 3 | | | | 3 | 6 or 12 | 6 | 24 | 6 cas. | 6 | Cert. |
| For Double Qualification by Royal College of Phys. and Surg. of Edinburgh, | 21 | 12 | 12 | 6 | 3 | 3 | 3 | 6 or 12 | 6 or 12 | 3 | 3 | 3 | | | 3 | 6 or 12 | 6 or 12 | 24 | 6 cas. | 6 | Cert. |
| For Double Qualification by Royal College Phys. Edinburgh, and Faculty of Physicians and Surgeons of Glasgow, | 21 | 12 | 12 | 6 | 3 | 3 | 6 | 6 or 12 | 6 | 3 | 3 | 6 | 3 | | 3 | 12 | 12 | 24 | 6 cas. | 6 | Cert. |
| Apothecaries' Hall, England, | 21 | 12 | 6 | 6 | 3 | 3 | 6 | 6 | 12 | 6 | 3 | 6 | 3 | | Appr. | 9 | 27 | 27 | 20 cas. | 6 | 9 mo. |
| " Ireland, | 21 | 6 | 12 | 6 | 3 | 3 | 6 | 6 | 6 | 6 | 3 | 6 | 3 | | Appr. | | 18 | 27 | 20 cas. | 6 | |

---

* Students from the Schools of Scotland are admitted to examination at the Royal College of Surgeons of England, if they have followed the course of study required by the regulations of the Royal College of Surgeons of Edinburgh. Students in Scotland, therefore, are not required to attend more than one course of Physiology, six months Clinical Surgery, six months Clinical Medicine, and twenty-four months Hospital.

ENGLISH POOR-LAW BOARD.—Candidates for the appointment of Medical Officer are required to be registered under the Medical Act, and must be legally qualified to practise both Medicine and Surgery, in virtue of Diplomas or Licenses granted by competent legal authority in England, Scotland, or Ireland.

Information respecting exceptions to these regulations under various circumstances, and other details as to the order in which, according to some Licensing Bodies, the courses should be taken out, etc, must be obtained by consulting the published Charts of the Colleges, etc. Students should apply to the Secretary to each Board which they intend to pass for a detailed copy of its Regulations.

# MEDICAL SCHOOLS OF SCOTLAND, 1865-66.

## WINTER SESSION.

| SUBJECTS. | UNIVERSITY OF EDINBURGH. | SURGEONS' HALL, EDINBURGH. | UNIVERSITY OF GLASGOW. | ANDERSON'S UNIVERSITY, GLASGOW. | UNIVERSITY OF ABERDEEN. | UNIVERSITY OF ST ANDREWS. |
|---|---|---|---|---|---|---|
| Anatomy, Systematic and Practical, with Demonstrations. | Professor Goodsir. | Dr Handyside. | Dr A. Thomson. | Dr George Buchanan. | Professor Struthers. | ... |
| Physiology, or Institutes of Medicine.* | Dr Bennett. | Dr Sanders. | Dr A. Buchanan. | Dr E. Watson. | Dr Ogilvie. | Dr Oswald Bell. |
| Chemistry, and Practical Chemistry. | Dr Playfair. | Dr Macadam. Dr C. Brown. | Dr T. Anderson. | Dr Penny. | Professor Brazier. | Dr F. Heddle. |
| Materia Medica and Therapeutics. | Dr Christison. | In Summer. | Dr Easton. | Dr Morton. | In Summer. | ... |
| Practice of Medicine. | Dr Laycock. | Dr Haldane.⁺ Dr Balfour. | Dr Gairdner. | Dr Cowan. | Dr Macrobin. | ... |
| Surgery. | Professor Spence. | Dr Watson.⁺ Dr Jos. Bell. Mr Annandale. | Prof. Lister. | Dr Macleod. | Professor Pirrie. | ... |
| Midwifery. | Dr Simpson. | In Summer. | Dr Pagan. | In Summer. | Dr Dyce. | ... |
| Natural Philosophy. | Professor Tait. | Mr Lees. | Professor W. Thomson. | Dr Taylor. | Professor Thomson. | Professor Fischer. |
| Natural History. | Professor Allman. | ... | Dr Rogers. | ... | Professor Nicol. | ... |
| General Pathology. | Dr Henderson. | Dr Grainger Stewart. | ... | ... | ... | ... |
| Clinical Medicine. | Drs Bennett, Laycock, and Maclagan. | Drs Sanders,‡ Haldane, Jackson, and J.M.Duncan. | Dr Bell and Dr Ritchie. | | Dr Kilgour. | ... |
| Clinical Surgery. | Professor Syme. | Dr Gillespie. | Dr A. Buchanan and Dr Lyon. | | Dr Keith. | ... |

* This course is equivalent to that given under the name of General Anatomy and Physiology in the English Schools. Special schedules are issued by the London Boards for their Scotch students, which should always be inquired for.

⁺ These are not conjoint courses, but separate ones by the gentlemen named.

‡ This is a joint course.

# MEDICAL SCHOOLS OF SCOTLAND, 1865–66.

## SUMMER SESSION.

| SUBJECTS. | UNIVERSITY OF EDINBURGH. | SURGEONS' HALL, EDINBURGH. | UNIVERSITY OF GLASGOW. | ANDERSON'S UNIVERSITY, GLASGOW. | UNIVERSITY OF ABERDEEN. |
|---|---|---|---|---|---|
| Practical Anatomy and Demonstrations. | Professor Goodsir. | Dr Handyside. | Dr A. Thomson. | Dr George Buchanan. | Professor Struthers. |
| Botany. | Dr Balfour. | ... | Dr W. Arnott. | Mr Hennedy. | Dr Dickie. |
| Materia Medica. | ... | Dr Scoresby-Jackson. | ... | ... | Dr Harvey. |
| Midwifery. | ... | Dr Keiller.* Dr Duncan. | ... | Dr Wilson. | ... |
| Medical Jurisprudence. | Dr Maclagan.† | Dr Littlejohn.† | Dr Rainy.† | Dr Leishman. | Dr Ogston.† |
| Comparative Anatomy. | Professor Goodsir. | Dr Handyside. | Dr A. Thomson. | ... | Prof. Nicol. |
| Histology. | Dr Bennett. | Dr Sanders. | ... | ... | Dr Ogilvie. Mr J. Thomson. |
| Practical Chemistry. | Dr Playfair. | Dr Macadam.* Dr Crum Brown. | Dr T. Anderson. | Dr Penny. | Prof. Brazier. |
| Natural Philosophy. | ... | Mr Lees. | ... | ... | Professor Thomson. |
| Natural History. | Professor Allman. | ... | Dr Rogers. | ... | Prof. Nicol. |
| Clinical Medicine. | Drs Bennett, Laycock, and Maclagan. | Drs Sanders,‡ Haldane, Jackson, and J. M. Duncan. | Dr Fraser and Dr R. Scott Orr. | | Dr Kilgour. |
| Clinical Surgery. | Prof. Syme. | Dr Gillespie. | Dr Morton and Dr G. Buchanan. | | Dr Keith. |

* These are not conjoint courses, but separate ones by the gentlemen named.
† Drs Rainy and Ogston deliver their courses only in winter. Drs Maclagan and Littlejohn give courses during both the winter and summer sessions.
‡ This is a joint course.

For additional Summer Courses on special subjects, see the Prospectus of each School.

# LIST OF HOSPITALS, DISPENSARIES, ETC., IN CONNEXION WITH THE MEDICAL SCHOOLS OF SCOTLAND.

## EDINBURGH.

ROYAL INFIRMARY, including LOCK HOSPITAL. Upwards of 560 Beds. Visits daily from 12 till 2 P.M. Physicians—Drs Bennett, Laycock, and Machgan, Professors of Clinical Medicine; Dr Simpson, Clinical Professor for Diseases of Women; Drs W. R. Sanders, D. R. Haldane, Scoresby-Jackson, Clinical Lecturers; Dr J. Matthews Duncan (on Diseases of Women). Pathologist, Dr Grainger Stewart.

Surgeons—Mr Syme, Professor of Clinical Surgery; Professor Spence; Dr Gillespie, Clinical Lecturer; Dr P. H. Watson. Assistant Surgeon, Mr Annandale. Assistant Surgeon to Clinical Wards, Dr Joseph Bell. Consulting Surgeon, Dr Dunsmure. Ophthalmic Surgeon, Mr Walker. Dental Surgeon, Dr John Smith.

CHALMERS' HOSPITAL FOR THE SICK AND HURT. 24 Beds for medical and surgical patients. Physician, Dr Halliday Douglas; Surgeon, Dr P. H. Watson.

ROYAL MATERNITY HOSPITAL. 36 Beds; 279 in-patients and 380 out-patients annually. Consulting Physicians—Drs Simpson and Moir. Physicians—Drs Thomson, Weir, Keiller, A. Wood. Consulting Physician, Dr Begbie. Ordinary Surgeon, Dr Dunsmure.

ROYAL HOSPITAL FOR SICK CHILDREN. 40 Beds; average number of out-patients, about 4000. Consulting Physicians—Professor Christison and Dr C. Wilson. Physicians—Drs Graham Weir, Keiller, and Moir. Extra Physicians—Drs Stephenson and Ritchie. Surgeon-Dentist, Dr Smith.

ROYAL PUBLIC DISPENSARY AND VACCINE INSTITUTION. About 11,500 patients annually. Medical Officers—Drs Spence, Pattison, Somerville, Wright, Sanders, Husband, Littlejohn, D. Wilson, Ritchie, Stephenson, Cairns, Linton. Physicians-Accoucheurs—Drs Keiller, Matthews Duncan, Wright, Pattison. Superintendent of Vaccination, Dr Husband. Visits daily at 2 P.M. Vaccination on Wednesdays and Saturdays at 12 noon.

NEW TOWN DISPENSARY. 7800 patients annually. Medical Officers—Drs J. Hunter, Dycer, Grainger Stewart, Joseph Bell, A. Dickson, John Duncan. Physician-Accoucheur—Dr Andrew Inglis. Superintendent of Vaccination, Dr J. Hunter. Visits daily at 2 P.M. Vaccination on Tuesdays and Fridays from 12 to 1.

ROYAL ASYLUM FOR THE INSANE. About 660 patients. Physician, Dr Skae. Lectures and Clinical Visits in summer.

EYE INFIRMARY, George Street. Surgeons—Benjamin Bell, Esq., F.R.C.S., and Dr Watson, F.R.C.S. Consulting Surgeon, Dr Hamilton. Open daily at 1 P.M.

EYE DISPENSARY, Cockburn Street. 1150 patients annually. Surgeons—Mr Walker, Dr Wilson. Consulting Surgeon, Dr Duncan. Open Monday, Wednesday, and Friday, at 1 P.M.

EAR DISPENSARY, Cockburn Street. Dr T. Keith. Tuesdays at 12. Average, about 20 cases daily.

EAR DISPENSARY. Dr Jackson. Mondays and Fridays, 11 to 12.

DENTAL DISPENSARY, Cockburn Street. Consulting Surgeons—Professor Goodsir, Professor Spence. Consulting Surgeon-Dentist, Mr Nasmyth, F.R.C.S. Ordinary Dentists—Dr John Smith, Dr Orphoot, Mr Hutchins, Dr Roberts, Dr Hogue. Daily, 9 to 10 A.M. Average number of patients 3000 per annum.

## GLASGOW.

ROYAL INFIRMARY. 600 Beds. Visits daily at 8.30 A.M. Physicians—Drs Scott Orr, W. T. Gairdner, Leishman, Steven. Fever Physician, Dr Perry. Physicians to out-patients—Drs M'Laren and James Stewart.

Surgeons—Drs Lyon, J. Morton, G. Buchanan, and Professor Lister. To out-patients—Drs Dewar and Macleod.

LOCK HOSPITAL. 47 beds. Medical Officers—Drs G. H. B. Macleod and D. Forbes.

LYING-IN HOSPITAL AND DISPENSARY. 24 Beds; in-patients 326, out-patients 353. Physicians—Drs J. G. Fleming, J. G. Wilson. House-Surgeon, Mr G. Gentle.

UNIVERSITY LYING-IN HOSPITAL AND DISPENSARY. In-patients about 750, out-patients about 2500. Physicians—Drs Pagan and Leishman. Assistant Physician, Dr S. Johnston Moore.

WESTERN PUBLIC DISPENSARY. Medical Officers—Drs Caughie, A. Wood Smith, J. Paton Watt, and John Barbour. Attendance daily.

ROYAL ASYLUM FOR THE INSANE. About 680 patients. Physician-Superintendent, Dr A. Macintosh.

EYE INFIRMARY. 24 Beds; 160 in-patients; 2497 out-patients annually. Consulting Surgeons—Drs Rainy and A. Anderson. Ordinary Surgeons—Drs W. Mackenzie, A. Anderson, W. Brown. Assistant Surgeon, Dr G. Rainy.

DISPENSARY FOR SKIN DISEASES. 1204 patients last year. Physician—Dr M'Call Anderson. Practical Courses in Diseases of the Skin and Ear are held during the Winter and Summer sessions. The Dispensary is open on Mondays and Thursdays at 2 P.M.

## ABERDEEN.

ROYAL INFIRMARY. Upwards of 280 beds. Visits daily at 12 o'clock. Consulting Physicians—Drs Dyce and Kilgour. Physicians—Drs Harvey, Smith, and Keith.

Surgeons—Messrs Keith, Pirrie, Kerr, Fiddes. Ophthalmic Surgeon, Dr Wolfe. Pathologist, Dr Beveridge. Dental Surgeon, Mr Williamson.

GENERAL DISPENSARY, LYING-IN, AND VACCINE INSTITUTION. Upwards of 6000 patients annually. Medical Officers—Drs Forsyth, Sutherland, and Christie; Messrs Smith, Paterson, and Fraser. Visits daily at 9.30 A.M. Vaccination every Wednesday at 3 P.M.

LUNATIC ASYLUM. Above 300 patients. Consulting Physician, Dr Macrobin. Resident Physician, Dr Robert Jamieson. Clinical Lectures in summer.

OPHTHALMIC INSTITUTION. 500 patients. Surgeon, Dr Cadenhead.

## ROYAL COLLEGE OF PHYSICIANS OF EDINBURGH,

### REGULATIONS REGARDING THE FELLOWSHIP AND MEMBERSHIP OF THE COLLEGE.

### I. *Of the Fellowship.*

1. No one shall be elected a Fellow of the College until he has been at least one year a Member thereof, and has attained the age of twenty-five years.

2. Every motion for the election of a Fellow shall be made at a quarterly meeting of Fellows by one of the Fellows present, and seconded by another; and this motion shall be determined by ballot at the next quarterly meeting of Fellows,—a majority of three-fourths being necessary to carry it in the affirmative.

3. If an urgent reason satisfactory to the Council be assigned, a Candidate may be proposed at an extraordinary meeting of the Fellows summoned for the purpose, and his petition may be balloted for at an extraordinary meeting of the Fellows specially summoned for the purpose; provided that the holding of this special meeting be agreed to by a majority of five-sixths of the Fellows present at the meeting at which the Candidate was proposed: provided also that not less than one week intervene between the two meetings, and that due notice of the intended ballot be given in the billets summoning the second meeting. The Candidate shall in this case pay to the Treasurer a sum of ten guineas in addition to the ordinary fees.

4. Every Fellow resident within five miles from the General Post-Office of Edinburgh shall, on his election, have his name placed on the roll of attendance, and shall pay the annual contribution, and be subject to all the laws of the College regarding fines. Fellows resident beyond five miles shall have the option of having their names on the roll of attendance or not; but if their names be on the roll of attendance, they shall pay the annual contribution, and be subject to fines.

5. Any Fellow may petition that his name be taken off the roll of attendance; which petition shall be determined by ballot at next quarterly meeting.

6. Any Fellow whose name is not on the roll of attendance may have it inserted by giving notice to the Secretary, who shall report to the next quarterly meeting; after which, the Fellow shall be entitled to all the privileges of the Fellowship, and may take his seat at the first meeting of the College.

7. Any Fellow leaving Edinburgh for a length of time, and omitting to petition to have his name taken off the roll of attendance, or wishing the same to be continued on it during his absence, shall be charged with his annual contribution and fines.

8. Fellows whose names are not on the roll of attendance shall not have the use of the library and reading-room, except in the cases specified in Laws 9 and 10.

9. Fellows whose names are not on the roll of attendance, on coming to reside in Edinburgh, or within five miles thereof, for a period not exceeding six months, may, with consent of the Council, be allowed the use of the library and reading-room.

10. Fellows not on the roll of attendance, who reside permanently in Edinburgh, or within five miles thereof, but are not engaged in practice, may, with consent of the Council, be allowed the use of the library and reading-room on payment of the annual contribution.

### II. *Of the Membership.*

1. Any Licentiate of a College of Physicians, or Graduate of a British or Irish University, with whose knowledge of medical and general science the College may be satisfied, may be admitted a Member of the College, provided he shall have attained the age of twenty-four years.

2. Every motion for the election of a Member shall be made at a quarterly meeting of Fellows by one of the Fellows present, and seconded by another; and this motion shall be determined by ballot at the next quarterly meeting,—a majority of three-fourths being necessary to carry it in the affirmative.

3. Every Member on the roll of attendance, whose address has been communicated to the Clerk, shall be summoned to attend all meetings of the Fellows and Members.

### III. *Of the Fees.*

1. The fee to be paid by a Member shall be thirty guineas.

2. When a Licentiate shall be raised to the rank of Member, he shall pay twenty guineas.

3. When a Member shall be raised to the rank of Fellow, he shall pay thirty guineas, exclusive of stamp-duty. The stamp-duty on the Fellowship payable to Government is £25.

4. All Candidates for Fellowship or Membership must lodge their Fees, and the amount of stamp-duty payable at the time to Government, with the Treasurer, previously to presenting their petitions.

---

## ROYAL COLLEGE OF SURGEONS OF EDINBURGH.

### ABSTRACT OF REGULATIONS FOR THE FELLOWSHIP.

1. No person shall be received as a candidate for the Fellowship who is not in possession of the diploma of the Royal College of Surgeons of Edinburgh, or of the Royal College of Surgeons of England, or of the Royal College of Surgeons of Ireland, or of the Faculty of Physicians and Surgeons of Glasgow.

2. No person shall be admitted as a Fellow who is under twenty-five years of age.

3. Every candidate for the Fellowship shall lodge with the President a petition for admission, and shall be recommended by two Fellows as proposer and seconder, of whom one at least shall be resident in Edinburgh.

4. Candidates for the Fellowship shall pay £25 to the College funds, including all fees. The money shall be payable to the Treasurer immediately after the presentation of the petition to the College.

5. The billets calling the meeting at which the petition is to be presented, shall intimate the name and surgical qualification of the candidate, his professional appointments, if any, and the names of his proposer and seconder.

6. The petition shall be considered at a subsequent meeting, to be held not earlier than a month after the first; and in the meantime, the petition, with the names of the proposer and the seconder, shall be hung up in the library; and the billets calling the second meeting shall contain an intimation in the same form as those of the first.

7. At the meeting for considering the petition of the candidate, the votes shall be given by ballot. Three-fourths of the votes are required to entitle the candidate to be admitted; and the number of those voting shall not be less than twenty.

---

## FACULTY OF PHYSICIANS AND SURGEONS, GLASGOW.

### ABSTRACT OF REGULATIONS FOR THE ELECTION AND ADMISSION OF FELLOWS.

1. A candidate for the Fellowship of the Faculty as a Physician must be a Doctor of Medicine of a University of the United Kingdom, or of a Foreign University recognised by the Faculty. A candidate for the Fellowship as a Surgeon must be a Licentiate of the Faculty, or a Fellow, Member, or Licentiate of one of the Royal Colleges of Surgeons of the United Kingdom.

2. A candidate must be proposed, in writing, by two Fellows, at an ordinary meeting of the Faculty. But no proposal shall be received until the candidate

shall have intimated, by letter, to the President, the medical or surgical quali-
fication (as the case may be), in virtue of which he desires admission to the
Fellowship. After having been proposed he shall submit to the Council all
necessary evidence of his professional qualifications, and of his being of
unexceptionable moral character.

3. A copy of the proposal shall be placed in the reading-room till the next
ordinary meeting of the Faculty; when, the Council having reported as to his
eligibility, and no reasonable ground for delay being shown, the Faculty shall
determine, by a ballot, whether or not the candidate shall be admitted as a
Fellow.

4. For the election of a Fellow, two-thirds of the votes given must be in
favour of his admission. An excerpt of the minutes of the meeting, giving the
result of the ballot, shall be sent by the Clerk of the Faculty to the candidate,
who, if elected, shall, at the next meeting of the Faculty or of the Council
thereof, be enrolled as a Fellow, upon making and subscribing the declaration
required by the Faculty.

5. The entrance fee (to be deposited at the date of his proposal) shall be
£50; but from this sum a Licentiate of the Faculty shall be entitled to a
deduction of whatever amount he may have already paid for his diploma.

6. A candidate residing beyond five miles from the Faculty Hall, on being
elected in the usual way, may, upon payment of £25 (subject in the case of a
Licentiate of the Faculty to deduction of one-half of the license fee) be
admitted a Fellow.

---

# ARMY MEDICAL DEPARTMENT.

## WHITEHALL YARD.

### QUALIFICATIONS AND EXAMINATION OF CANDIDATES FOR COMMISSIONS IN THE ARMY MEDICAL SERVICE.

1. Every candidate desirous of presenting himself for admission to the Com-
petitive Examination required for the Army Medical Service must be unmarried.
He must produce a birth certificate from the District Registrar, or a certificate
of baptism in which the date of birth is stated; or if neither of these can be
obtained, an affidavit from one of the parents, or from some other near relative
who can attest the date of birth, will be accepted. The certificate or affidavit
must show that the candidate is not above twenty-eight nor under twenty-one
years of age. He must also produce certificates of moral conduct and character,
one of them from the parochial minister if possible.

2. The candidate must make a declaration that he labours under no mental
or constitutional disease, nor any imperfection or disability that can interfere
with the most efficient discharge of the duties of a medical officer in any climate.
He must also attest his readiness to engage for general service immediately on
being gazetted.

3. The candidate must possess a diploma in Surgery, or a license to practise
it, as well as a degree in Medicine, or a license to practise it in Great Britain
or Ireland.

4. Certificate of registration in accordance with the Medical Act of 1858,
and certificate of character, must be lodged at the Army Medical Department
for examination and registry, at least one week before the candidate appears
for examination. The certificate of age must accompany this form when filled
up and returned.

5. On producing the foregoing qualifications the candidate will be examined
by the Examining Board in the following subjects :—Anatomy and Physiology;
Surgery; Medicine, including Therapeutics, the Diseases of Women and
Children, Chemistry and Pharmacy, and a practical knowledge of drugs. (The
examination in Medicine and Surgery will be in part practical, and will include

operations on the dead body, the application of surgical apparatus, and the examination of medical and surgical patients at the bedside).

The eligibility of each candidate for the Army Medical Service will be determined by the result of the examinations in these subjects only.

Candidates who desire it will be examined in Comparative Anatomy, Zoology, and Botany, with special reference to Materia Medica, and the number of marks gained in these subjects will be added to the total number of marks obtained in the obligatory part of the examination by candidates who shall have been found qualified for admission, and whose position on the list of successful competitors will thus be improved in proportion to their knowledge of these branches of science. The subjects for this part of the examination will be taken from the following books :—" Animal Kingdom," by W. S. Dallas, F.L.S. " Outlines of the Structure and Functions of the Animal Kingdom," by Rymer Jones; or " Cours Élémentaire d'Histoire Naturelle," par Milne Edwards. Lindley's " School Botany ;" Lindley's " Medical and Economic Botany ;" Henfrey's " Elementary Course of Botany." Candidates who may desire it may also be examined in the Elements of Physics and in Physical Geography. The following books are recommended for this purpose :—" Elements of Natural Philosophy," by Golding Bird and C. Brooke. " Physical Geography," by Mrs Somerville.

6. The examiners in London shall prepare a list in order of merit, with the marks affixed in the different subjects, to be transmitted to the Director-General, and communicated to the professors of the Army Medical School. If any candidate is found to be deficient in any particular subject, this shall be stated, in order that he may receive special instruction on the point at Netley.

7. After passing his preliminary examination, every candidate will be required to attend one entire course of practical instruction at the Army Medical School, before being admitted to his examination for a commission, on Hygiene, Clinical and Military Medicine, Clinical and Military Surgery, Pathology of Diseases and Injuries incident to Military Service. These courses to be of not less than four months' duration.

8. At their conclusion, the candidate will be required to pass an examination on the subjects taught in the School. The examination will be conducted by the professors of the School. The Director-General, or any medical officer deputed by him, may be present and take part in the examination. If the candidate give satisfactory evidence of being qualified for the practical duties of an army medical officer, he will be eligible for a commission as assistant-surgeon.

9. During the period of his residence at the Army Medical School, each candidate will receive an allowance of 5s. per diem with quarters, or 7s. per diem without quarters, to cover all costs of maintenance, and he will be required to provide himself with uniform (viz., the regulation undress uniform of an assistant-surgeon, but without the sword).

10. All candidates will be required to conform to such rules of discipline as the senate may, from time to time, enact.

---

## ARMY MEDICAL SCHOOL.

### ROYAL VICTORIA HOSPITAL, NETLEY.

All gentlemen who have been successful in the Competitive Examination, held twice a-year (February and August), at Chelsea, for appointments in the Medical Service of the Army, attend subsequently, at the Royal Victoria Hospital, a course of practical instruction in the duties they will have to perform in the Army. The course lasts four months, after which an examination is held to ascertain the progress made by each candidate. The lectures on Military Surgery include gunshot and other wounds; arrangements for the transport of wounded; duties of army surgeons in the field, during sieges, on

transports, etc.; and other special subjects. Those on Military Medicine refer to the tropical and other diseases of the British possessions and colonies, and to the losses, by disease in peace and war at home and abroad. The lectures on Hygiene comprise all duties relating to the examination of water, air, food, clothing, etc. of the soldier; his duties and exercise, and the circumstances affecting his health; the subjects of meteorology, statistics, and prevention of the principal diseases met with in the Army, on home or foreign service. The lectures on Pathology have reference chiefly to the scientific examination of tropical diseases, and of other complaints which the army-surgeon is especially called on to investigate. The candidates also attend the wards of the hospital to study the diseases of invalids under the Professors of Medicine and Surgery, the system of recruiting, and the modes of keeping the army medical returns and records. They are also called on to make post-mortem examinations, to operate on the dead body, and pass through courses of practical instruction in the laboratory on the modes of recognising the qualities and adulterations of food, and in the microscopic room on the modes of microscopic examination of morbid tissues and of adulterations of food, etc.

---

# NAVY MEDICAL DEPARTMENT.

### ADMIRALTY, SOMERSET HOUSE.

#### REGULATIONS FOR CANDIDATES FOR THE OFFICE OF ASSISTANT-SURGEON IN THE ROYAL NAVY.

A candidate for entry into the Royal Navy shall make a written application to that effect, addressed to the Secretary of the Admiralty; on the receipt of which application he will be furnished with the regulations and a printed form, to be filled up by him, to show if he possesses the required qualifications.

As vacancies occur, the number of candidates required will be ordered to attend at the Admiralty Office, bringing with them the requisite certificates, showing that they are fully qualified by age, professional ability, etc., when they will be examined by a board of medical officers, to be named by their Lordships.

That no person be admitted as an Assistant-Surgeon in the Royal Navy who shall not produce a certificate of being registered under the Medical Act, and a diploma from one of the Royal Colleges of Surgeons of England, Edinburgh, or Dublin, from the Faculty of Physicians and Surgeons of Glasgow, from Trinity College, Dublin, or from other corporate body legally entitled to grant a diploma in Surgery; nor as a Surgeon unless he shall produce a certificate from one of the said colleges, faculty, or corporate body, founded on an examination to be passed subsequent to his appointment of Assistant-Surgeon as to his fitness for the situation of Surgeon in the Navy; and in every case the person producing such diploma and certificate shall also undergo a further examination, touching his qualifications in all the necessary branches and points of Medicine and Surgery, both at the time of his entry and after serving three years to render himself eligible for Surgeon; and that previously to the admission of Assistant-Surgeons into the Navy, it will be required that they produce proof of having received a preliminary classical education, and that they possess, in particular, a competent knowledge of Latin; also,

That they are of good moral character; the certificate of which must be signed by the clergyman of the parish, or by a magistrate of the district.

That they have served an apprenticeship, or have been engaged for not less than six months in Practical Pharmacy.

That their age be not less than twenty years, or more than twenty-six years.

That they have actually attended a recognised Hospital for eighteen months subsequently to the age of eighteen, in which Hospital the average number of patients is not less than one hundred.

That they have been engaged in actual dissections of the human body twelve months, the certificate of which from the teacher must state the number of subjects or parts dissected by the candidate.

That they have attended Lectures, etc., on the following subjects, at established schools of eminence, by Physicians or Surgeons of the recognised Colleges of Physicians and Surgeons in the United Kingdom, for periods not less than hereunder stated; observing, however, that such Lectures will not be admitted if the teacher shall lecture on more than one branch of science, or if the Lectures on Anatomy, Surgery, and Medicine be not attended during Winter Sessions of six months each: — Anatomy 18 months; or General Anatomy 12 months, and Comparative Anatomy 6 months. General Surgery 12 months, or Military Surgery 6 months, and General Surgery 6 months. Theory of Medicine 6 months, Practice of Medicine 6 months; if the Lectures on the Theory and Practice of Medicine be given in conjunction, then the period required is 12 months. Clinical Lectures (at an Hospital as above) 12 months; on the Practice of Medicine 6 months, on the Practice of Surgery 6 months. Chemistry 6 months; or Lectures on Chemistry 3 months, and Practical Chemistry 3 months. Materia Medica 6 months. Midwifery 6 months, accompanied by certificates stating the number of Midwifery cases personally attended. Botany 3 months.

A favourable consideration will be given to candidates who have obtained the degree of M.D. at either of the Universities of Oxford, Cambridge, Edinburgh, Dublin, Glasgow, London, or Aberdeen; or who, by possessing a knowledge of the diseases of the eye, and of any branch of science connected with the profession, such as Medical Jurisprudence, Natural History, Natural Philosophy, etc., appear to be more peculiarly eligible for admission into the service.

By the rules of the service, no Assistant-Surgeon can be promoted to the rank of Surgeon until he shall have served five years (two years of which must be in a ship actually employed at sea), and can produce a certificate from one of the before-mentioned colleges, faculty, or corporate body; and it is resolved that not any certificate of examination from any of the aforesaid institutions shall be admitted toward the qualification for Surgeon, unless the certificate shall be obtained on an examination passed after a period of not less than three years' actual service; observing, that no one can be admitted to an examination for Surgeon, unless, as hereinbefore mentioned, he can produce a certificate, together with the most satisfactory proof, that he has performed, on the dead body, under the superintendence of a professor or teacher of known eminence, all the capital operations of Surgery, and is perfectly competent to perform any operation with skill and dexterity, and thoroughly acquainted with the anatomy of the parts involved in such operation; without which qualification no one hereafter can be promoted to the higher branches of the service; and whenever Assistant-Surgeons already in the service (whose professional education may not be in accordance with the above) obtain leave to study previously to their passing for Surgeon, they will be required, on their examination, to produce testimonials of their having availed themselves of the period of leave to complete their education agreeably to these regulations generally.

# Part Second.

## REVIEWS.

*A System of Surgery, Theoretical and Practical, in Treatises by various Authors.* Edited by T. HOLMES, M.A. Cantab. In Four Volumes, Volume the Fourth. London: Longmans: 1864.

WE have, on former occasions,[1] directed the attention of our readers to the earlier volumes of this work. We have now to congratulate the editor, the authors of the various treatises, and the publishers, on the successful termination of their important work. The original plan has been strictly adhered to, and the work must be considered a very complete account of every thing connected with the science and practice of Surgery.

The present volume treats of the diseases of the organs of digestion, of the genito-urinary system, of the breast, thyroid gland, and skin, together with an appendix of two hundred and fifty pages, embracing the surgical diseases of childhood, surgical fever, apnœa, parasites, venomous insects and reptiles, surgical diagnosis and regional surgery, hospitals, and surgical instruments and apparatus. Our limits forbid us to do more than glance at one or two of these subjects.

The first treatise in the volume "on the Surgical Diseases connected with the Teeth," is by Mr Salter, Surgeon-dentist to Guy's Hospital, and in the space of sixty pages contains a great deal of very interesting matter. Ordinary works on Surgery say but little regarding the diseases of the teeth, and although, no doubt, a particular mechanical knowledge is necessary to the dentist, these diseases are specially interesting, not merely from the importance of the local phenomena, but from their being very frequently associated with constitutional causes. The subjects chiefly treated of in Mr Salter's essay are alveolar abscess; painful and difficult eruption of the wisdom teeth; tumours of the gum and tooth pulp; abscess of the antrum; dentiferous cysts; alveolar and maxillary necrosis; hæmorrhage after extraction; and the application of obturators, etc., in fissures of the hard palate, or fistulæ of the antrum.

In speaking of the abscess of the antrum, an ingenious contrivance is described for facilitating the free discharge of the purulent matter; it consists of a plate fitted over the place which was occupied by the extracted tooth; through this plate a hole has been bored corresponding to the orifice into the antrum, and through this a short tube is carried just sufficiently long to enter the antrum. The orifice in the tube is closed by a cork, which can be removed when injection of the cavity is to be practised. The object of this

---

[1] See this Journal for October 1861, and December 1862.

contrivance is, of course, to keep patent an opening through which the purulent matter may drain away, while at the same time food is prevented from passing through the orifice in the socket into the maxillary sinus.

The chapter on diphtheria and croup is written by Dr Barclay, Physician to St George's Hospital. The account of the symptoms, diagnosis, and general treatment of the disease, is very good, but Dr Barclay's observations on the propriety of performing tracheotomy in these diseases are by no means satisfactory. In regard to the question of operating in diphtheria, he says, "it is by no means easy to give in few words a definite answer to this inquiry, or to lay down rules which may be sufficient to guide the practitioner in deciding on his course. If we turn to statistics, we find that the fatal termination is not averted to any great extent, although, in all probability, some lives have been saved by the operation which must otherwise have been lost." Had Dr Barclay been acquainted with Professor Spence's papers on tracheotomy in croup and diphtheria, he would have been able to give his readers better rules for the circumstances under which the operation should be performed.

The chapter on the surgery of the male urinary organs is by Mr Henry Thompson, and is clearly and ably written.

The first chapter in the appendix is on the surgical diseases of childhood: the authors are Mr Holmes, Mr Brodhurst, and Mr Shaw.

In conclusion, we can cordially recommend this work as a valuable addition to the library of the surgeon. Of course, it is not perfect, and on many of the points contained in it there is room for difference of opinion. But it has been carefully and conscientiously written, and is creditable to the surgeons of the metropolis.

─────

*Lectures on Orthopædic Surgery.* By Lewis Bauer, M.D. Delivered at the Brooklyn Medical and Surgical Institute. Lindsay and Blakiston: Philadelphia: 1864. Pp. 108.

The Board of Trustees of the Brooklyn Medical and Surgical Institute appointed Dr Bauer as one of their lecturers. The course of lectures on Orthopædic Surgery delivered under these auspices constitutes this work, and seems to be published almost verbatim. The author has succeeded in accumulating a very large amount of information, the result of much digestion of the labours of others, into a small space and in a very accessible form. The researches of the Webers on the mechanism and physiology of the spine, the theories of Baehring and Guerin, and the practice of Dieffenbach, Stromeyer, Langenbeck, Little, and Sayre, are largely drawn upon, but every quotation is most amply and conscientiously acknowleged; a very large number of less known

authors and isolated cases are also quoted; so that this little work really gives a very fair idea of the chief "lines of practice" of the orthopædic surgery of the past.

In such a digest of the past, from its very fulness and excellence, there is not much to review; some of Dr Bauer's own observations are at once more original and more challengeable. His pathology of hip-joint disease, as conveyed in the following sentences, will surprise most surgeons :—

"We are fully prepared to show that the etiology of hip disease has no connexion with tuberculosis, and that the existence of the latter itself has become totally questionable by more recent investigation."

The meaning of this second clause is obscure; it appears to deny the existence of tuberculosis, but a few sentences lower down we read,—

"In contaminated nutrition hip disease *may* originate, but we are firmly convinced *that the former is more often the result than the cause of the disease.*"

The italics are our author's. He then states his belief that inflammation of the ligamentum teres, the result of contusions or contrecoup, is the first if not the essential morbid change in hip disease, and that to it all the other changes are secondary.

The treatment of hip disease consists, according to Dr Bauer,—

1. In milder cases, by extension and counter extension, by splints, screws, pulleys, and weights.

2. When the pain is severe, and the joint filled with inflammatory products, to relieve the tension of the capsular membrane either, 1st, by twisting the joint; 2d, by subcutaneous paracentesis of the joint.

In the section on the treatment of spinal deformity, we have the descriptions and diagrams of various complicated instruments, yet the following sensible estimate of their value :—

"The *very best* mechanical contrivance is nothing more than a '*monitor*' to the patient, restraining undue motion of the spine, and slightly sustaining the superincumbent weight of the body. Stays are silly and reprehensible apparatus."

Yet, on the opposite page, we have a diagram of a "*cuirass*" made of soft iron and wire-webbing. There is much in a name.

The woodcuts are numerous, and almost curiously hideous, in their roughness and bad drawing; the style is conversational, occasionally stilted, and adorned with numerous Americanisms.

---

*The Surgery of the Rectum, being the Lettsomian Lectures on Surgery,* delivered before the Medical Society of London, 1865. By HENRY SMITH, F.R.C.S. John Churchill and Sons: London: 1865. Pp. 127.

THE most charitable critic would find this a hopeless case. Its very title a misnomer, this little pamphlet on certain of the diseases affecting the rectum, and certain plans of treatment, contains little that is novel, and much that is mistaken.

It consists of the three lectures listened to by the Medical Society of London, and, as the author tells us in the preface, was " so favourably received by a large and attentive audience that I hesitate not to publish them just as they were delivered." This fact may, in part, account for the slipshod style, which is so apt to deform what is written for the lecture-room, but the numbers and patience of the audience hardly deserved the third lecture. Its description, in the author's own words, is sufficient :—

" It will be perceived that the use of my improved clamp in the treatment of hæmorrhoids and prolapse of the rectum is fully explained, and, together with the record of my experience of the method recommended, forms the subject of the whole of the last lecture."

Now the instrument is already described in another work by the same author ;[1] surely a *rechauffé* of the description of an instrument, with thirty-seven detailed instances of its use, is not a lecture worthy of a large and attentive audience.

Lecture I. is on some points connected with fistula in ano. We may refer the reader, anxious for information on the question of how much ought to be cut, to the following sentences, quoted verbatim :—

" When the inner opening does exist, and in the situation referred to, I believe we need not carry the point of the bistoury further than this spot ; but it does not follow, that in those cases where an internal opening does not exist, and where the sinus runs some distance up the bowel, that we should in all cases limit our incision to that point where the internal opening is generally expected to be found."—P. 10.

" It is clear that in our operations we should confine our incisions as much as possible to the lower extremity of the bowel, and fortunately it is found by experience that the division alone of the external sphincter muscle in the direction of the fistulous sinus is sufficient to produce a satisfactory cure ; but, as I have before stated, there are exceptions to this rule."—P. 15.

And certainly he cannot complain that he has not a fair choice.

An account of a few cases in which stricture of the rectum complicated fistula in ano, prepares us for Lecture II., which is devoted chiefly to stricture of the rectum, simple and malignant, and to rectal polypi.

The contents of Lecture III. have been already mentioned : the tone of it will be best seen from the following sentences, which need no comment.

" It is not possible that either tetanus or pyæmia, the two most formidable results of the ligature, can occur after this operation, because the condition which produces the former affection does not obtain, viz., the presence of an irritating substance around the nerves for several days ; and pyæmia, or other inflammatory affections, will be sufficiently prevented by the exposed surface being deprived of its vitality and the veins being blocked up by the cauterization."—P. 102.

" It is impossible for any surgeon conscientiously to tell his patient that there is no danger whatever after the ligature ; but this may be, most truthfully stated, with regard to the operation by my improved clamp, if the most ordinary precautions are taken to prevent bleeding."—P. 104.

[1] Smith on Hæmorrhoids.   Third Edition, p. 116, etc.

*Clinical Surgery: on Tumours, and Tumours of the Breast, more particularly in Reference to their Diagnosis.* By THOMAS BRYANT, F.R.C.S., Assistant-Surgeon, etc., Guy's Hospital. Part V.: pp. 90. London: John Churchill & Sons.

THIS little work is the fifth of a series of Memoirs on Clinical Surgery on which Mr Bryant is at present engaged; and some of its predecessors have been already and favourably reviewed in these pages. Like all the works of this author, it is a carefully written and thoroughly sensible and practical treatise. A quiet thoughtfulness, and a desire to utilize as far as possible every surgical case, however trifling it might appear to an unobservant eye, characterize all Mr Bryant's surgical work.

The aim of these observations on tumours is primarily to aid in clinical diagnosis, and to record the method and results of treatment of various kinds of tumour. But Pathology is not forgotten: a chapter "On some Points in the Pathology of Tumours tending to illustrate the Subject of their Diagnosis" being prefixed, and bringing out briefly but very clearly certain important first principles in the form of a series of axioms, with a short commentary on each. To what school of Pathology Mr Bryant belongs will be clearly seen from the following quotation of the first two axioms:—

"As a leading pathological principle, it may, I think, be unhesitatingly asserted, *that all tumours, with the exception of the hydatid, are made up of one or more of the natural elementary tissues of the body, and that in no single example has any extraneous or new element been ever detected.*

"From this, therefore, a second leading principle may be fairly drawn,— *That all tumours partake of the nature of the part in which they are developed, and are more or less made up of the elements which naturally enter into its formation.*"

The first half of the book consists of a rapid sketch of the varieties of tumours (following the arrangement of Mr Paget) from a clinical point of view; reference is made to about 300 cases, and a few of the more interesting and remarkable are given in some detail.

From a peculiarity of arrangement which is necessary in a series of clinical reports, cancerous tumours receive a smaller share of attention than usual, the reason being that epithelial cancers of the lips, tongue, rectum, and penis are discussed under their separate organs; as also cancers of breast, testicle, and bones. Surely hydatid tumours in surgical seats are becoming more common of late years than they used to be: Mr Bryant records six cases all observed at Guy's within nine years, and all successfully removed. Three typical cases of recurring fibroid tumours are given, in all of which, however, the patient died after three, four, and seventeen operations respectively.

The second half is devoted to tumours of the breast, including a brief notice of abscess, acute and chronic, with an analysis of 102

cases. In the treatment of chronic abscess, the drainage-tube receives due commendation.

Various aids are given for the diagnosis of the simple adenoid and cystic tumours of the breast from the cancers. An analysis of 222 cases of cancer follows; 133 were operated on, of whom 9 died of the operation. As usual, no data are furnished as to the number of complete and *permanent* recoveries. The last case in the paper is interesting from its extreme rarity; a good example of what may be called the natural cure of scirrhus. A well-marked case in a state of ulceration, in the seventh year of its presence and the fourth of its ulceration, without any local treatment beyond water-dressing, " became much smaller, several pieces the size of nuts having fallen off, having apparently been destroyed by the contraction of their own fibres." Five years afterwards, by this process of gradual decay, the tumour had been reduced to the size of half a pea in the cicatrix; and a year after that report, the patient considered herself well, the local disease causing her no pain or inconvenience. A full description of this form of natural cure will be found in Maurice Colles' of Dublin excellent work on the Diagnosis of Cancer, also published by Mr Churchill.

*The Practice of Medicine.* By Thomas Hawkes Tanner, M.D., F.L.S., Fifth Edition, Enlarged and Improved. London: Renshaw: 1865.

Though this volume is called a fifth edition, it is so much enlarged as nearly to constitute a new work. Dr Tanner's " Manual of the Practice of Medicine" was very popular among students preparing for examination; the fourth edition was in 32mo, and consisted of about 700 pages. The fifth edition no longer retains the title of a Manual, and has expanded into a handsome octavo volume of upwards of 900 pages. The increased space at Dr Tanner's disposal has enabled him to give a more complete account of the symptoms of diseases generally, and also to introduce several new subjects; in particular we may notice a chapter of a hundred pages on the diseases of the female sexual organs. Of course the work is essentially a compilation, but the statements are generally accurate, and it will, we think, form a very fair text-book for the medical student.

# Part Third.

## PERISCOPE.

### PRACTICE OF MEDICINE.

#### ON THE TREATMENT OF URÆMIA. BY DR PETERS.

THE following are Dr Peters' conclusions with regard to the efficacy of different medicines in increasing or diminishing the quantity of urea:—

1. *Tartar Emetic.*—Bœcker took 2½ grains of tartar emetic daily for nine days; the urea was *lessened* about 75 grains in each twenty-four hours; the only alteration of any importance in the urine was the diminution of urea. Beigel gave two persons two grains every day for four days, and found the urea invariably decreased about 75 grains daily. Tartar emetic is well known to be very useful in acute cases of Bright's disease; but Ackermann found it to greatly increase the quantity of urea, and apparently proportionately to the size of the dose. It may prove useful both by preventing the formation of urea and increasing its elimination.

2. *Golden Sulphuret of Antimony* increases all the constituents of the urine, and that of urea over ninety grains daily. Parkes thinks that it either increases the elimination of all the constituents of the urine, or favours the metamorphosis of nearly all the tissues of the body, or both. He regrets that this remedy has almost fallen into disuse of late, and that it has been superseded by tartar emetic, especially in chronic diseases.

3. *Muriate of Ammonia* increases all the constituents of the urine except uric acid, which it slightly diminishes; the daily *increase* of urea was about 74 grains, a quantity which indicates a great augmentation of metamorphosis or elimination; as it lessens the uric acid, perhaps it converts this more rapidly into urea.

4. *Citrate of Ammonia.*—Prout noticed a great increase of urea in the urine of a dyspeptic patient while taking this remedy.

5. *Liquor Potassæ.*—In several cases the amount of urea was increased, and Parkes says this occurred so constantly as to lead to the inference that this alkali really augmented the *formation* of urea. The uric acid was unaffected. Day says, little is definitely known regarding the power of remedial agents in modifying the amount of urea, except that liquor potassæ has been decisively proved, by the experiments of Dr Parkes, to increase its quantity. He adds, the experiments of Bœcker, Beigel and others, on this subject, are too vague and uncertain in their results to call for special notice. But doctors will disagree.

6. *Nitrate of Soda.*—According to Schenck, it increases the water of urine and the urea for the first three or four days, when they both fall below the normal standard to an extent which balances the previous increase. It acts first as a diuretic, increasing the amount of urinary water, thus aiding the elimination of urea; but its influence is limited and temporary; it is a mere elimination, and the formation of urea in the system is not augmented. It increases the urinary water 2700 grains for three or four days, and the urea about 60 grains; then the water falls off 3000 grains below the previous increase, and about 1350 grains below the normal standard; the urea fell off about 100 grains below the increase, and 45 grains below the normal quantity. One and one-half ounce of nitrate of soda were required to produce the above effects.

7. *Nitrate of Potash.*—According to Beigel, three drachms given in twenty-four hours diminished the urea slightly in one case, and greatly in two cases. Hence, it either lessens the production of urea or prevents its elimination. Parkes, from one ounce given twenty-four hours, found it to act as a diuretic, and to cause the excretion of an excess of nearly 200 grains of urinary solids.

8. *Phosphate of Soda* diminishes the urea nearly 30 grains in one day when given in 225-grain doses; it is said to retard the digestive process extremely, and diminish the amount of nutriment entering the system. It is not a disintegrating agent, but rather the reverse; it lessens both the quantity of urea and that of the insensible perspiration.

9. *Acetate of Potash.*—Bœcker took three ounces of the Prussian solution of the acetate of potash in four days, and found the quantity of urea lessened about 15 grains; the urinary water about 440 grains. It lessens the urinary water, urea, and extractives, and the earthy phosphates in a remarkable degree, viz., to the extent of 22 grains per day. In another case in which the acetate of potash was taken for eleven days, the urea was diminished over 60 grains per day.

10. *Colchicum* lessens the quantity of urea and uric acid from one-seventh to one-quarter the normal quantity. Garrod found the uric acid generally, but slightly lessened. This remedy should prove either very useful or very injurious; in one case I thought it decidedly injurious, or rather discomforting, for it caused nausea and vomiting in very small doses. But in scarlatinous dropsy Dr Maclagan frequently found colchicum of much service, particularly when the urine became very scanty, and indications were given of approaching coma. He thinks that urea retained in the blood is the cause of the symptoms, and that colchicum causes its discharge. The same writer proposes colchicum in the advanced stages of Bright's disease as a means of depurating the blood.

11. *Belladonna* increases rather than diminishes the quantity of urinary water, urea, and extractives.

12. *Quinine.*—Ranke has made the important observation, that twenty grains of quinine lessens very greatly, viz., about one-half, the excretion of uric acid. It does not affect the quantity of urea. There was no increased excretion of uric acid after the effect of the quinine had passed off; hence, the formation of uric acid was probably absolutely lessened; as the quantity of urea was not increased, it was, of course, not converted into urea.

13. *Digitalis.*—In a case of cardiac dropsy observed by I. Vogel, 419 grains of urea were excreted before digitalis was given, and 755 grains per day subsequently; in another case the urea rose to 696 grains daily.

14. *Juniper Ointment* increases the urinary water to 114 ounces, and the quantity of urea from 339 to 622 grains.—*New York Medical Journal.*

### ON THE USE OF ERGOT IN CHOREA.

AT a meeting of the New York Obstetrical Society, the following remarks were made on this subject:—

Dr Jacobi related the following case he had recently treated. A girl, 10 years old, had been under his care for pneumonia, and afterwards for bronchitis, but has, during the past year, been healthy. Five weeks ago, she was brought to him suffering with intense chorea, not being able to walk or sleep, and hardly to swallow, and with no cessation in the movements during sleep. She was feverish, with hot skin, thirst, rapid pulse, etc. The spine was examined, and no excessive sensitiveness was detected in the lumbar or lower dorsal regions; but over the first dorsal vertebra, and increasing in severity on ascending the cervical region, there was tenderness. The chorea being due to irritation of the spinal cord and cerebellum, the cervical region was leeched, and ice applied for three or four days, when the girl began to improve. The ice was now continued for a week longer, and a strong purgative given with marked benefit. Afterwards, as the Doctor has seen good effects from ergot in spinal meningitis, he administered it in this case, and in large doses. At first, half an ounce of Squibb's fluid extract was taken daily, in combination

with sulphate of soda, and latterly but two drachms, with ten grains of quinine per day, continued for two weeks. The girl is greatly improved, being able to walk with assistance, and to eat and talk. Dr Jacobi remarked, that the majority of cases of chorea occur in girls between 6 and 11 years of age, and that it is generally impossible to trace the symptoms to any local affections, except to rheumatic disease of the heart. If no rheumatic or cardiac trouble can be found, the spine and cerebellum are examined, but generally with like result.

Dr Gonzalez Echeverria stated that he had seen a case in which there was pain in the cervical region of the spine, with most violent choreic convulsions. The patient died, and, on post-mortem examination, apoplexy in the spinal-gray substance of the cervical region of the cord was found. The Doctor also related a case of chorea, mostly located in the right limbs, in a pregnant woman he had seen in consultation with Dr W. H. Van Buren. The disease was preceded by sudden hemiplegia, occurring upon protracted lactation, but which nearly subsided, until the development of the choreic convulsions at the beginning of gestation. The sensibility of the limbs was evidently diminished, and the patient showed a tendency to cerebral congestion, which made once necessary the application of leeches to the back of the ears. The urine contained no casts, once or twice was slightly albuminous, but kept throughout its normal condition, with the exception of an increased quantity of phosphates. The state of the patient continuing to be alarming, and appearing to depend in a great degree on gestation, premature labour had to be induced at about the eighth month. This operation was skilfully carried through by Dr George T. Elliot. The nervous symptoms did not, however, materially abate. The patient was then put upon the use of ten grains of bromide of potassium, three times a-day; the remedy was soon discontinued on account of pain in the stomach, which the patient attributed to it. She was then directed to use ergotine gr. j., with quinine grs. ij., twice a-day, and to resume the bromide of pot. mixed with the tr. rhei., and carbonate of ammonia. Under this treatment she decidedly improved: the ergotine was carried up to grs. xij. a-day, and then discontinued, but the bromide of potassium has been kept up to the dose of thirty grains, three times a-day, with the carbonate of ammonia, and half an ounce of the infusion of calumbo. Cold bathing, application of ice to the spine, and the localized movement cure, were employed in addition to the above means. Uterine disease having been suspected all along, the patient at last consented to be examined with the speculum. Besides retroversion, a large ulcer of the neck and enlargement of the womb were found, together with leucorrhœa and the dysmenorrhœa already complained of by the patient. A local treatment has been instituted for these latter symptoms, and the patient's improvement has continued beyond the stationary point it seemed to reach with the above means. Let me add, that the child, now over a year, has grown very robust, and to this date free from any nervous derangement. It is true, that in this case ergot was not the only remedy employed, but I have used it mainly in other cases of chorea, with similar good results to those mentioned by Dr Jacobi. I generally prescribe the Aq. extract of ergot, or Bonjean's ergotine, in the shape of pills, combined with quinine and the extract of conium added, to prevent the pain which ergotine is apt to cause on the digestive organs. The largest doses of ergotine I have prescribed have been from eight to fifteen grains a-day, the latter dose having been employed with adults.—*New York Medical Journal.*

### OBSTINATE CHOREA TREATED WITH THE CALABAR BEAN.
#### BY DR JOHN W. OGLE.

Robert W., aged 14, was admitted into St George's Hospital with decided and persistent chorea, for which, during the course of several months, the preparations of steel, antimony, zinc, arsenic, and valerian were tried, in addition to suitable generous diet. He remained eleven weeks in the hospital, and was

to have returned home in very much the same state as when he came in, when permission was given to me by Dr Page, whose patient he was, to try the use of Calabar bean.  Having obtained a number of the beans, I got Messrs Bullock and Reynolds to make me a tincture or alcoholic solution.  Of this I gave four drops for a dose thrice daily, increasing it gradually during the course of nine weeks until he took eighteen drops three times a-day.  During the whole of this period no change was observed in the pupils of the eyes, but the boy, without the use of further medicine, recovered almost entirely the power over his muscles, so that he left the hospital and went home into Hertfordshire about the 6th or 7th of April almost quite well, the want of control in certain movements being only occasionally perceptible.  He was requested to return to the hospital if any relapse occurred, but we have seen nothing of him.

I have now an obstinate and violent case of chorea (in a young woman) in St George's Hospital which I am treating with Calabar bean.

I have recently been trying the Calabar bean upon an out-patient with very severe so-called *paralysis agitans*, but I have not obtained (as, indeed, I hardly expected I should) any desirable result.  Indeed, no result was obtained either as to power of controlling the movements of the muscles, or as to the action of the pupils of the eyes.  The form of preparation used was the alcoholic solution supplied to the hospital by Bell, of the strength of one drachm of the bean to one ounce of rectified spirits of wine.  Of this, twenty drops were given, diluted, thrice a-day, and increased, by ten drops at a time, to one drachm in the course of six weeks.  For one month he took one drachm doses thrice a-day, and then it was increased to eighty drops.  This he took for three weeks, and then he took one and a-half drachm doses for two weeks.  At the end of that time, no effect being produced either on the muscles of the limbs or the eyes, I discontinued the remedy.

I have given the details of this unsuccessful case to show what doses of the Calabar bean might be given with impunity.—*Medical Times and Gazette.*

### HEMIPLEGIA ON THE RIGHT SIDE, WITH DEFECT OF SPEECH.
#### BY DR HUGHLINGS JACKSON.

A WOMAN, 66 years of age, was considered to be well up to October 1862. She was one day sitting for her photograph, when all at once she called out that she feared she was losing the use of her right side.  She fell off her chair, and did not speak a word for a month or more, and she never spoke properly afterwards.  Her tongue was " swollen," and for months she did not eat solid food, but swallowed fluids well.  For several days after the attack she did not put her tongue out.  When she began to talk she said but few words, and seldom called anything by its right name.  She not only called " dinner" " supper"—words having some connexion—but called " dinner" " chair," etc. It was impossible to get to know what she wanted, either by words or signs. She could not write with the left hand, but made meaningless marks on paper.

When Dr Jackson saw her, two years after the attack, there was total paralysis of the right arm and leg, and a defect of articulation so great that he could never make out anything she said.  This was not due to inability in the muscles of the tongue, lips, and palate, as she moved these parts well.  Her friends could sometimes make out what she said, and could understand what she meant by signs.  She did not come to the hospital for a year before her death, which occurred two years from the attack.  Before death she suffered from extreme œdema of both legs, and for some months passed her time in bed. She died slowly from exhaustion.

*Autopsy.*—There was granular disease of the kidneys.  The heart had probably been hypertrophied, but at the post-mortem both ventricles seemed nearly equally thin and rotten; in the brain were found the remains of an extensive apoplexy; on opening the lateral ventricles there was seen on the left side a large yellowish walled cavity the size of a walnut, which seemed to affect equally the corpus striatum, the thalamus opticus, and the adjoining hemisphere.  It

was found that it extended downwards to the Island of Reil, but the convolutions were unbroken ; the cavity extended under the thalamus, and into the crus cerebri ; it had not broken into the descending cornu, nor did it encroach on the corpora geniculata or corpora quadrigemina. The frontal convolutions seemed healthy, but they were not examined by the microscope.

In this case, with hemiplegia on the right, there was defect of talking, although the tongue was not paralyzed. The disease, however, was far too extensive to help us to determine anything precisely as to the seat of speech. The case has, however, this value, that it shows that what may appear at first glances to be a mere defect of articulation is often due to disease of parts far above the ninth nerve or its nucleus. The probability is, that the convolutions near the corpus striatum have to do with guiding muscles in articulation. Although the frontal convolutions appeared healthy, so extensive disease of the motor tract must have interfered with the exercise of their functions.—*Medical Times and Gazette.*

### ON ILL-SMELLING FEET. BY PROFESSOR HEBRA.

In this affection, while the rest of the body shows no unusual amount of transpiration, the feet (and sometimes the hands also) exhibit an extraordinary amount of sweating, accompanied by a most unpleasant odour. In former times this excessive secretion was looked upon as an advantageous circumstance, freeing the blood of a certain amount of " acrimony" which might have given rise to disease ; while various affections, the origin of which was obscure, were attributed to its suppression. For this reason, so far from seeking to suppress this secretion, it was sought to encourage it, and to recall it when it had disappeared. That it can be safely and effectually treated, however, the Professor is convinced from the results derived from his numerous cases.

First, a few words on the appearance of the parts. The feet and hands of these persons feel remarkably cold, without they themselves being conscious of a deficiency of warmth. In slight degrees of the affection the points of the fingers and toes are alone cold, but in worse cases the whole palm of the hand and sole of the foot, the back of the hand and foot also not being warm, although they do not present that icy moisture felt on the other parts. On looking closely the parts affected are found to be of an intense bluish-red colour, numerous small, shining droplets of sweat appearing at their surface. Moreover, the epidermis of the toes themselves, from long maceration in the secretion, presents a white, wrinkled appearance, like that produced by long continued maceration in warm water. In exaggerated cases portions of this macerated epidermis separate, leaving excoriated surfaces, which greatly impede locomotion. With these conditions becomes produced the intolerably fetid smell which renders the person a nuisance to all around him.

Finding that there really was no effectual remedy known for this distressing condition, Professor Hebra determined to do his best to investigate the nature of the anomaly in secretion, and to discover the means of relieving it. The first question he put to himself was, whether the smell was inherent to or derived from some condition external to the economy, and he soon came to the conclusion in favour of the latter view. In repeated instances the shoes and stockings of persons suffering from stinking feet were taken away from them, and the feet carefully cleaned with soap and water by means of a nail brush. They were then put to bed, warmly covered up, and freely supplied with warm and diaphoretic drinks until a free transpiration was secured. The feet were carefully enveloped in gutta-percha paper, or other waterproof material, so as to prevent the passage of the sweat. The sweat of the entire body, though having its acid odour, did not manifest anything of the stinking character ; but when the waterproof coverings of the feet were left unchanged the penetrating stink was gradually produced until it predominated. The shoes, which had been kept away from these persons, retained for weeks the filthy odour. The expression " stinking foot-sweat " is, in fact, an incorrect one, the proper one

being "stinking shoes produced by an excessive production of sweat." The influence of stout, thick shoes as a proximate cause of the odour is seen in the facts that copious sweating of the hands is not attended by ill-smell, evaporation not being impeded, and that persons going barefoot and women who wear thin shoes are seldom liable to it.

The indications for treatment are leaving off the shoes which have induced the stink, and the application of means which have been found by experience capable of diminishing the excessive secretion. In slight cases it suffices to dust the insides of the stockings with some simple powder—such as lycopodium, alum, or even common flour. If this simple means fail, the following may be employed with certainty of success :—Some diachylon is to be gently melted over a fire, and then an equal weight of linseed oil is to be added, so as to form a homogeneous ointment. This is to be spread on linen, in which the foot, having been thoroughly washed and dried, is to be completely and exactly enveloped. Where the toes come into contact, shreds of lint covered with the ointment are to be interposed. So wrapped up, the foot is to be covered with a stocking and a light shoe, well open at the instep. At the end of twelve hours the application is to be removed, and the foot is to be well rubbed by means of a dry towel or one of the powders mentioned before, but neither washed nor bathed. It is then to be covered up with the ointment again. This procedure will be required to be repeated, according to the intensity of the evil, during eight or ten days, but the patient meanwhile is enabled to go about his ordinary occupations. After this time the ointment is to be left off, but friction by means of pulverulent substances is to be continued a while longer, and the ordinary shoes worn. After some days, brownish-yellow portions of epidermis, about half a line in thickness, separate from the affected parts, leaving a white, clean, healthy epidermis behind. It is only after this separation has taken place that washing the feet or the use of a foot-bath is to allowed ; and for some time afterwards the pulverulent substances should still be rubbed into the foot. In this way, at the end of from fourteen to twenty-one days, the foot-sweating either disappears for ever, or at least for one or more years. In quite exceptional cases, to secure this end, a repetition of the above procedure for a second time is required; but then it is invariably successful. Professor Hebra has, during fifteen years, employed in several hundred cases this with success, and without the least attendant disadvantage.—*Allgem. Wiener Med. Zeitung.*

[M. Stanislas Martin (*Bull. de Therap.*, t. lxv. p. 143) observes that some of the applications employed for removing this disgusting infirmity are not always harmless, the arrest of transpiration having in some cases been followed by neuralgia, disturbance of the digestive organs, etc. The diffusion of the abominable stink may be effectually prevented by placing a sole containing a layer of powdered charcoal either between the foot and the stocking, or between the latter and the shoe. A paste, composed of forty parts of powdered charcoal, forty of water, and fifteen of gum, should be thickly spread over a piece of filtering-paper, flannel, felt, etc., stretched over a board or pasteboard. The paste is then covered over with another piece of paper, which is to be smoothed with the hand so as to remove all asperities. The whole is submitted to compression during an hour, after which the water is to be allowed to evaporate. When quite dry, the sole may be cut out of the required size. Being so cheaply made, these soles can be changed once or twice a-day, if required.]—*Medical Times and Gazette.*

# Part Fourth.

## MEDICAL NEWS.

### PROCEEDINGS OF THE EDINBURGH OBSTETRICAL SOCIETY.

SESSION XXIV.—MEETING VIII.

22d *March* 1865.—Dr CHARLES BELL in the Chair.

#### I. MOULDED SULPHATE OF COPPER.

*Dr James Sidey* showed specimens of sulphate of copper which had been moulded into the shape and size of the ordinary sticks of nitrate of silver, and which could easily be used with a quill. Sulphate of copper could be moulded in this manner to any required shape, by being powdered and mixed with a fourth part of borax. It was often difficult to get a nice piece of ordinary sulphate of copper to use in some cases; and Messrs Smith, chemists, of Duke Street, had kindly made several. Dr S. was not certain who had first suggested the borax, but it had appeared in one of the foreign journals, and as some of the Fellows might not have seen the method, he thought it right to make it better known. The mixture required the addition of a few drops of water.

#### II. OBSTETRIC HISTORY OF A PATIENT WITH A PELVIC TUMOUR.

*Dr Moir* stated, that on the 29th February he had been summoned to see a woman who had been brought to the Maternity Hospital by Dr Kirk of Bathgate. Dr K. had seen her for the first time that morning, on her being taken in labour, and having on examination ascertained the existence of a large pelvic tumour, which he felt satisfied would render necessary more than usual assistance, he resolved on bringing her to town.

Dr Kirk had been previously informed, by her former medical attendant, of the existence of this tumour—which had been first discovered about eight years ago when Mrs S. was in labour; that it was then about the size of a small lemon, and so interfered with the passage of the child through the pelvis as to cause its death; that in the subsequent labour the result was the same, the tumour having increased considerably in size; and that since then it had progressed at even a speedier rate. Mrs S. is 42 years of age, and has had ten labours, including the present one.

On examining the patient, Dr Moir found the whole of the back part of the pelvis filled up by a hard tumour, permitting only of the passage of the flattened hand between it and the pubes. He found the os uteri opening and the breech presenting, and thought it advisable at once to bring down the feet. The pains not being strong, and the os not sufficiently dilated, it was thought proper not to hurry the delivery, and in the meantime to summon the other physicians of the Maternity, to see the case. Accordingly, Drs Thomson, Keiller, Alex. Simpson, Dr Kirk of Bathgate, and some others, were shortly at the Hospital; and on consultation with them, it was agreed that the attempt should be made to deliver the child by the feet, and to break down the head in the course of the labour, should that be found necessary for the delivery of the patient. Accordingly, the os uteri being now quite dilated, Dr Moir proceeded with the delivery. There was no *very great* difficulty experienced in bringing the body through the pelvis—considerably more in bringing down the arms; but after this the resistance to the passage of

the head was something fearful. Care was taken that the os uteri was slipped over the child's head, and that the latter was directed so that its narrow diameter should correspond with the narrowest one of the pelvis, and as it was advanced a little, that the chin was well brought down by inserting two fingers in the mouth; but it required the united efforts of both Dr Moir and Dr Keiller for a considerable time to bring it down through the pelvis. The head, on delivery, was much flattened,—one of the parietal bones being forced inwards and fractured; the child was, of course, still-born; it weighed 6½ lb., and measured 17 inches.

The mother progressed favourably, without any untoward symptom, and was able to return to Bathgate in a fortnight after delivery. Before leaving, she was examined by Professor Simpson and Dr Moir, and the tumour found, as previously described, apparently osteo-sarcomatous, growing from the sacrum, and filling up the posterior part of the cavity of the pelvis.

Dr Moir begged to remind the members that about two years ago he had reported a similar case, to which he had been called by a medical man some miles from town, but where the osteo-sarcomatous growth was not so large. It was a head presentation, and turning was had recourse to in the hope of giving the child a better chance. The delivery was a very difficult one and the child was still-born,—though, after a long time employed in resuscitating it, it recovered, and is now a fine healthy child. The lady has not been pregnant since.

*Professor Simpson* had seen this patient (Mrs S.). It was a case of osteo-sarcoma fixed to the upper part of the sacrum. There was a cast in his museum of a similar case which occurred in Dublin, where the question of Cæsarean section was raised. Embryulsio, however, was performed after the woman had been some time in labour, but the patient died. In the present case a question might be considered,—Should the patient again become pregnant, what ought to be done? He thought that if the tumour was found not to have increased in size, then premature labour should be induced; but if it was larger than at present, then Cæsarean section would be preferable to running the risk of the same issue as in the Dublin case. This class of tumour fortunately was of very slow growth.

*Dr Bruce* put the question, whether, in a case of this nature, we knew Cæsarean section to be absolutely necessary, would we be justified in producing abortion at an early period, so as to save the risk to the mother's life?

*Professor Simpson* remarked that the question was a difficult one. He thought he might do it once, but would scarcely feel himself justified in performing it time after time.

### III. TUMOUR OF THE UTERUS—SLOUGHING—DEATH FROM SEPTIC POISONING.

*The Secretary* showed a specimen of a fibroid tumour sent by Dr Trail of Dunfermline. There had been an effort of the uterus to expel the mass through the os, the tumour had begun to slough, and the patient sunk under symptoms of septic poisoning.

### IV. POLYPUS OF UTERUS BECOMING CANCEROUS.

*Dr Bryce* of Dalkeith stated that he had lately had a case where a polypus had taken on a cancerous nature. The cancer had first manifested itself in the mamma, and secondarily in the uterus.

*Professor Simpson* remarked, that such a change in the nature of the polypus was exceedingly rare.

### V. ON ABORTION.

*Dr Charles Bell* read a paper on this subject, which will be found at page 120 of the August number of this Journal.

12th April 1865.—Dr CARMICHAEL, Vice-President, in the Chair.

### 1. DEATH OF FŒTUS FROM TORSION OF CORD.

*The Secretary* exhibited a specimen, furnished by Dr Moir, of a case where death of the fœtus had taken place from torsion of the cord. The patient was delivered about the seventh month. All along the movements had been very feeble, and had been unfelt for a week previous to delivery. The fœtus, of small size, presented a large umbilical hernia and had malformation of the hands; the right having only four digits, and the left five, but one was nearly amputated. The cord was long in proportion to the size of the fœtus, and when the placenta was suspended by holding up the child, it revolved rapidly, showing the great extent to which torsion had taken place. The twisting in the funis was not continuous, as is generally the case, but at intervals of an inch or two throughout the whole extent. Between the parts, the gelatine of Wharton was present as in natural cases, the whole cord thus presenting a beaded appearance. In the placenta there were several nodules of old effused blood, indicating a diseased condition of that organ.

### II. CASE OF AMAUROSIS DURING PREGNANCY.

*Dr Murray* read the following account of the case :—

On 19th February, I was asked to see Mrs G., æt. 24. On calling, I found her very much swollen, labouring under intense albuminuria, and six months advanced in her first pregnancy. Her appearance is very pale and anæmic; but she says she always enjoyed good health. Along with the swelling there has supervened a great dimness of vision, amounting almost to blindness, which renders it impossible for her to distinguish between different objects. She can walk from one part of the house to another, but with difficulty; can only distinguish between one person and another by means of the voice; is unable to engage in any of her household duties, such as cooking; and fails to distinguish between the letters of the largest print. Nothing remarkable is seen externally, except that her sclerotic has a very pearly appearance,—indeed it contains no vessels at all. Thinking it was one of those cases depending on the condition of albuminuria, I advised the administration of diuretics, acetate and bitartrate of potash, with the effect of diminishing the swelling, but with no improvement as regards vision.

Three weeks afterwards, on 2d March, patient was delivered of a putrid child, after which the swelling completely disappeared. No improvement of vision.

About a fortnight after delivery, 15th March, as she continued in much the same state, I thought it advisable to have an ophthalmoscopic examination, in which I was kindly assisted by Dr Argyll Robertson, when the following condition of matters was found, which sufficiently accounts for all the symptoms present :—The retina in both eyes is the seat of numerous small extravasations of blood, which occur more immediately in the neighbourhood of the macula lutea. The vessels of the retina are congested and tortuous; there are exudations of lymph into the retina, and between it and the choroid; they are abundant in the immediate vicinity of the entrance of the optic nerve. She was ordered two-grain doses of iodide of potassium three times a-day. No improvement of vision has yet taken place.

Ten days afterwards she was again submitted to an examination by the ophthalmoscope. The appearance presented is now considerably altered; the extravasations of blood have entirely disappeared; the vessels are still tortuous and congested, but less so than on the former occasion; the veins also appear somewhat interrupted in their course (a sign of considerable significance), while the effusions of lymph previously noticed are markedly diminished in size. The margins of the optic discs (entrance of optic nerves) appear somewhat indistinct and ill defined, more especially on the side corresponding to the macula lutea.

*12th April.*—To-day I saw the patient, and I found no improvement as regards vision. An examination was made with the ophthalmoscope, when the following condition of matters was displayed. In the right eye the veins were congested in the region of the macula lutea, and there was a small amount of atrophy with a little pigment. In the left eye the veins were very much congested. Between the macula lutea and the entrance of the optic nerve several small spots of extravasated blood were seen, but no deposit of pigment. All over the fundus engorged veins were very visible posteriorly—tortuous and distended. To the outer side of the macula lutea there was also a series of white spots, resulting from atrophy of the choroid. In the centre of these last spots was a dark appearance of the fundus, as if it had been the seat of effusion of blood. There were also atrophic spots on the outside of the macula lutea. Patient is still continuing the treatment, with iodide of potassium and tincture of muriate of iron, but with no very favourable result. I very much fear that, after the organic lesions which have taken place, not much can be expected.

*Dr A. R. Simpson* remarked, that this case showed the value of the ophthalmoscope in enabling us to distinguish the different forms of amaurosis during pregnancy. Great difficulty always attended the form where extravasation had taken place, especially when it had become organized.

### III. NOTICE OF A WOMAN, WHO HAD BEEN DEPRIVED OF BOTH ARMS AT BIRTH.

*The Secretary* stated that he had received a letter from Dr Fothergill of Morland, in which he (Dr F.) stated that there was living in his neighbourhood a woman without arms. Her account of her loss is, that the medical man amputated them before her birth in order to deliver her mother.

*Dr Keiller* doubted the accuracy of the woman's statement, and in this case was inclined to regard the case as one of natural intra-uterine amputation. It would be interesting to get the full particulars of this case.

---

### SESSION XXIV.—MEETING X.
### 26th April 1865.—Dr WILSON in the Chair.

#### I. FATAL CASE OF ACCIDENTAL HÆMORRHAGE.

*Dr Young* read the following notes of the case :—

Mrs A., æt. 40 or 41, was rather of a corpulent habit, and had long enjoyed excellent health. I was called to visit her at 6 A.M. on the 22d February, when I found that about a pint of blood had escaped in the chamber utensil, and a considerable quantity in the bed. On examination, I had some difficulty in even discovering the os uteri, and found some exertion necessary in the introduction of one finger, it being almost of cartilaginous firmness, and so far from the cervix being thin, the length of this callous canal could not be less than one inch. She has felt no pains. She was only in the eighth month of pregnancy, or rather had not completed the eighth month.

After remaining with her for about an hour, I ruptured the membranes with a blunt-pointed probe, about nine inches long, and encased in a small-sized male catheter made straight, having the farther end open, which I had formerly employed for the purpose of inducing premature labour till a safer and better method was introduced. I had previously tried to rupture the membranes in the present case with a pointed piece of whalebone, but found that the membranes yielded before it, as they not unfrequently do when firm and not distended by pain. No good resulted from the rupture; and finding the hæmorrhage continuing, and the os uteri as rigid and undilatable as at first, I introduced into the os a sponge-tent, but which, after remaining a considerable time, produced no effect in dilating it. Finding the hæmorrhage rather increasing than otherwise, and the utter hopelessness of dilating the os, and the patient's strength giving way, I considered it necessary to have the advice and assistance of Professor Simpson, but who being unable from indisposition

to leave his own house, kindly sent Dr Alexander Simpson. After mature consideration of the whole circumstances, and after making a farther trial of larger sponge-tents, he agreed with me in thinking that the only chance of life for the patient was to make two lateral incisions through the os and cervix of the uterus; this was done about 10 A.M., after which I had little difficulty in gradually, and as gently as possible, dilating the os and introducing the hand, and effecting delivery by turning. There being, however, still too much discharge, we injected iced water into the uterus, which had the effect of greatly modifying the discharge. The pulse being weaker and the patient faint, the supply of wine was continued; and by noon, when we left, she had considerably recovered, but complained of pain, though not severe, in the left iliac region.

Dr Simpson saw her again with me at 2 P.M., and found her going on well, the pain not severe, and the pulse 110. She continued to progress favourably till the third day after delivery, when the pulse rose to 120, and the pain had increased, with a slightly tympanitic condition of the abdomen. She had already taken a considerable quantity of opium, with small doses of calomel, fomentations of hot water; poultices with turpentine were also employed. She was ordered nutritious soup, and next day wine. She died on the fifth day.

*Post-mortem examination thirty hours after death*, conducted by Dr Jolly, demonstrator of anatomy, who remarks as follows:—" Abdomen very tympanitic. Adipose matter to the depth of an inch and a-half. On opening abdomen intestines found enormously distended with flatus. There was no effusion into peritoneal cavity, and no signs of peritonitis, with the exception of slight injection of the recto-vesical pouch.

" *Uterus.*—No adhesions. Normal in appearance externally; perfectly free and movable in pelvis; the os and cervix had been laterally incised on the left side to the extent of an inch. The mucous membrane of cervix had a slightly dark sloughy appearance; other organs not examined. The post-mortem examination revealed no evident cause of death. No inflammation visible in any organ in the abdomen. On one spot of the colon, about the size of a shilling, there appeared a little coagulable lymph. Two fibrous tumours existed in the uterus, evidently of long standing, about the size of a nut,—one on the anterior wall of the uterus, and one behind. The incision of the os and cervix, and increase of the aperture by a slight tear, did not extend beyond the junction of the cervix with the upper part of the vagina."

Hæmorrhage, whether accidental or unavoidable, is always a source of anxiety to the practitioner, and sometimes occasions no small amount of perplexity. It is, to say the least, a grave occurrence. Knowing as we do that unless checked a majority will die undelivered,—and I know few more painful positions in which an accoucheur can be placed than when his patient dies undelivered,—I have no doubt that one principal cause of fatality is unjustifiable delay, there being often great reluctance to a "forced" delivery, where the os uteri is in an undilated state, even though soft and yielding, which fortunately is the case in most instances, especially after the first birth. The usual advice in works on midwifery is to wait till the os has assumed a condition favourable for the introduction of the hand. And certainly there is not in general much difficulty in effecting this in cases of severe hæmorrhage; but it does occasionally happen that the os, notwithstanding the flooding, and that even in a second or third child, is so firm as to require the continued exertion of the practitioner, especially if the labour is premature. In such cases the probability is, that the undeveloped condition of the cervix is as much to blame as the rigidity of the os.

Smellie gives three cases where he found it impossible to dilate the os uteri without extensive tearing, and adds, " by this it appears how difficult it is to dilate this part in women going of a first child, especially when they are pretty old; indeed it is sometimes impossible to be done before they come to their full time, and even then not till the parts are soft and thin."

Plugging the os or vagina, or both, in order to arrest the flooding, is, to say the least, a very doubtful practice. Merriman thinks it inapplicable after the first five months of pregnancy; and for this reason, that the parietes of the uterus, especially after the sixth or seventh month, are so easy of distention as to yield readily to the accumulation within, and thus the blood may collect and increase in the cavity of the uterus till a fatal termination takes place; for as justly remarked by Ramsbotham—(I cannot help taking this opportunity of saying how much the profession is indebted to Dr Simpson for the introduction of sponge-tents for the os uteri, so useful and important in many cases, not only of diseases of the uterus, but also of abortions attended with much loss of blood),—"This reasoning is undoubtedly true to its fullest extent in accidental hæmorrhage before delivery under retention of the placenta, and in flooding after its expulsion; for, as the blood concretes within that organ, the viscus are more and more distended, its vessels become more dilated, their orifices gape wider and wider, and consequently rendered capable of pouring out a larger quantity of blood in a given time. These remarks," he adds, "are not intended to apply to unavoidable hæmorrhage; other objections apply to the use of the plug, especially when near the full period of pregnancy."

*Rupture of the Membranes.*—Denman, Rigby, Merriman, Blundel, Davis, Conquest, Ingleby, all advocate this practice. Among others, one good result may be, that in consequence of the cavity of the uterus being lessened, the uterine vessels are less likely to continue bleeding, so violently at least. The probability of a farther separation of the placenta to any great extent is lessened, and the partial contraction of the muscular coat of the uterus is likely to diminish the size of the open mouths of the bleeding vessels; besides all this, it is calculated to promote regular labour pains.

*Dr Alexander R. Simpson* said, that he had shown the placenta in this case at a previous meeting of the Society. The members would recollect that there were several points where extravasation had taken place at different times. There was one of a pale colour, another more recent, of a darker hue, and a large one from which the fatal hæmorrhage had taken place. The incision of the os, as described above, was the only chance of remedy. There was no unusual discharge from the incision.

*Dr Moir* remarked that in cases of accidental hæmorrhage he never recommended the rupturing of the membranes. The best treatment he considered was to turn the child, and deliver as soon as possible. Many years ago he had a similar case to the present one. There was profuse hæmorrhage, and the woman was rapidly sinking. The os was firm, and admitted only one finger, but by tilting up the head and external manipulation he succeeded in laying hold of a foot. The os dilated to the body of the child, and he thus succeeded in saving the patient. Rupture of the membranes he considered useful only where the back of the child was towards the placenta, when it might then act mechanically by pressure on that organ. In other cases it does not stop the hæmorrhage, and greatly increases the difficulty of turning the child.

*Dr Burn* remarked that he had had a number of cases of accidental hæmorrhage; he invariably ruptured the membranes, and he did not recollect a single case where it did not restrain the hæmorrhage.

*Professor Simpson* stated that in his lectures he recommended rupture of the membranes for accidental hæmorrhage. The question was, where does the blood come from? He believed it escaped from the line of junction between the attached and the detached portions of the placenta. By rupturing the membranes, and thus lessening the contents of the uterus, you put this line off the stretch, and thus restrain the bleeding. This mode of procedure often succeeded. Rupture of the membranes, however, no doubt increased the difficulty of turning. He had performed incision of the os two or three times, one case of which he had published, where, by means of it, he had saved both mother and child. Laceration of the os was known frequently to take place during labour, and especially with turning before the os was fully dilated.

He had heard of a case lately where tetanus had followed, but this was extremely rare. In cases of unavoidable hæmorrhage, he had lately had a new question suggested to him. He had been called to see a lady who had had severe and frequent attacks of hæmorrhage during pregnancy, due to placenta prævia. She was now much exhausted from loss of blood; and the question was, whether premature labour should not be induced? There was small probability of saving the child; and should the risk of the patient's life be run by the repetition of these hæmorrhages? He had lately found that a practice had got abroad of turning in cases of unavoidable hæmorrhage, immediately after the placenta had been removed in the manner recommended by him. This he considered bad practice. It was submitting the patient, exhausted by loss of blood, to the risks of a double operation, and the probability of thus saving the child was very small, while the danger to the mother was great. In some cases, where the presentation was abnormal, it could not be helped; but where the presentation did not necessitate it, he considered it bad practice to interfere further with labour.

*Dr Moir* remarked that there was a point in reference to post-partum hæmorrhage which he had not seen referred to in books. Dr Hamilton used to state, that when a patient complained after delivery of a constant severe pain in the back, he always found that internal hæmorrhage had taken place. In practice, he (Dr M.) had also always found it the case.

*Dr Bryce* stated that he had had several patients who flooded in this manner after each delivery, and they always complained of this constant pain, but in his cases it was situated in front. He found it was never safe to leave these patients until the normal recurrent after-pains had come on.

*Dr Pattison* and *Dr Ziegler* both related cases where internal hæmorrhage had taken place after more than an hour after delivery; both patients complained of intense pain in the back. It was from this symptom that they had been again summoned to the patients.

### II. CÆSAREAN SECTION IN CASE OF RUPTURE OF THE UTERUS.

*Dr Moir's* account of the case will be found at p. 313 of the Journal.

*Professor Simpson* remarked, that in this case the child could not be removed by the natural passages, and gastrotomy was the only means of delivery. Dr Trask, an American author, had lately shown that statistics were in favour of this mode in all cases of rupture with escape of the child into the cavity of the peritoneum. It must necessarily tear the uterus much more to bring the child again through the lacerated walls of the uterus.

### III. CASE OF IMPERFORATE ANUS.

*Dr Bryce* of Dalkeith showed the preparation, and read the following notes :—

In the forenoon of the 17th of March last, I was asked to see a little boy who had been born at two o'clock the same morning with imperforate anus. On examination, the usual site of the anus could only be recognised by a small spot where the skin was slightly thinner and somewhat lighter in colour than the surrounding integument. There existed a partial frenum between the scrotum and penis. The prepuce was firmly adherent to the glans, which was about the size of a small pea, and of a dark-purple colour. As there was no distention of the bowels or bladder, and no bulging at the anal site, I thought it better to wait for a short time before having recourse to any operative interference. I saw the child again late in the evening, but there was still no protrusion of the intestines, and pressure with the finger revealed nothing but a hollow space where the sphincter ought to have been felt. As he had not yet voided urine, I attempted to explore the urethra, and with difficulty got a No 2. bougie passed down the canal as far as the neck of the bladder, but beyond that I could not pass it without using undue force. As the child was not suffering in the least, as the bladder and abdomen were not at all distended, and as the parents were unwilling that any operation should be performed unless there was some prospect of success, of which I told

them I had none, I decided on trying if a little more time would give any better prospect for operative procedure. When I saw the case on the following day, I had the assistance of Dr Alexander Simpson, who thought we ought to give the child the chance of an exploratory incision. This he did; but we were unable to find the bowel, after cutting as deeply as we thought safe. From that till the 22d the child occasionally passed small quantities of urine, and from the wound there came, on several occasions, very small quantities of what looked like mixed meconium and blood, particularly at those times when I introduced a finger to search for the bowel.

As the bladder and bowels had become greatly distended, I took with me, on the morning of the 22d, a curved trochar, to pierce what we thought, after making the incision, was probably the bladder, as it was felt behind the symphisis, occupying the roof of the space into which the incision had passed, but found that the case had died at two in the morning, having lived five days. Twelve hours after death, my friend Dr Macdonald made a post-mortem examination, with the following result :—Depth of wound, $1\frac{1}{2}$ inch. On opening the peritoneal cavity there was a considerable escape of flatus, and meconium was found extravasated among the intestines. The bladder and rectum very much distended. The latter, filled with meconium, had a perforation in the anterior wall, $1\frac{1}{4}$ inch from the upper end, the size of the perforation being about $1\frac{1}{2}$ line. On removing the symphisis pubis and perinæum, and dissecting close to the coccyx and sacrum, the rectum was found to end in a cul-de-sac behind the symphisis, $1\frac{1}{4}$ inch from the external surface, the exploratory incision having gone close to but behind the gut,—the knife, in fact, was in the right and the bowel in the wrong place. The large intestines were filled with meconium, but natural; the small intestines empty, and to all appearance normal.

On examining the preparation which I now show the Society, it will be seen that the point of the knife has made a very small puncture at the extremity of the gut where it is bound down behind the symphisis; the operation having thus been almost a successful one as far as it was itself concerned, but as to the ultimate results to the child probably questionable. This small puncture explains the statement of an attendant, that on one or two occasions flatus had passed with a shrill whistling sound.

I have given the minute details of this case, trusting that the case—its failure, and yet the well-nigh success of the operation—may be of some use to others if they should meet with this rare congenital malformation. It will be seen from the preparation that had I used the trochar during life it would have pierced what turns out to have been really the bowel, but what we conjectured to be the bladder; and if another similar case were occurring to me, I would not hesitate to use it or a knife at once, even if it were only to give temporary relief, which is probably all that can be done in such a case. It will also be seen from the preparation that the canal of the urethra is pervious throughout, and that the inability to empty the bladder must have been caused by the loaded and distended rectum.

---

SESSION XXIV.—MEETING XI.

10th May 1865.—Dr Pattison, Vice-President, in the Chair.

### I. CASE OF RECURRENT ABORTION.

Dr Ziegler gave the following history :—

Mrs L., æt. 36, has been married twelve years, and has now three living children of the respective ages of 11, 8, and 5 years, all born at the full term of gestation. She has had five miscarriages; the first occurring in the eleventh week of gestation, between the birth of her first and second child. The second was in the sixth month of gestation, and occurred between the birth of her second and third child, and on this occasion the placenta was so adherent that it could be removed only in small portions.

The other three miscarriages followed each other in succession, and at the following dates:—*October* 1861, at third month of gestation: the ovum not being seen, but probably escaping with the discharges. *September* 1862, at fourth month of gestation: the fœtus was, on this occasion, expelled without the membranes or placenta, which were never seen. Her last miscarriage occurred on the 23d *April* 1865; and with the following history:—

She (Mrs L.) last menstruated on the 3d October 1864, and continued to enjoy her usual health until the end of three and a-half months, when she was seized with severe cutting pain over the pubis, followed by rigors, with sleeplessness at night and great nervous depression. Eight days afterwards, discharge of a brownish fluid commenced, and this continued in greater or less quantity till the 22d of April 1865, when violent flooding set in. Uterine action began during that night, and, after several doses of ergot, the entire ovum was expelled.

The ovum which, in this case, had been retained in utero for several weeks after its death, showed marked signs of inflammatory action. The placenta was much thickened and indurated, while its uterine surface was covered with coagulable lymph of a yellowish colour, and numerous small rounded and partially-decolorized clots were embedded in its texture. Between the chorion and amnion there was a slight effusion of bloody serum. On opening the membranes they were found to contain a considerable quantity of turbid bloody fluid, with floculi of lymph floating in it. The umbilical cord was coiled many times round the body and extremities of the fœtus, and presented a well-marked instance of that excessive twisting on itself, to which, at a former meeting of the Society, Dr Alexander Simpson called the attention of the members.

(Is it not probable that in many of these cases where death of the fœtus has appeared to result from excessive twisting of the umbilical cord, the fatal termination may rather have been dependent on some pathological state of the placenta, such as has been already described, and that the twisting of the cord may be a secondary occurrence, caused by the struggles of the fœtus while suffering from imperfect oxygenation of its blood?)

On this, as on former occasions, this patient was treated with large doses of chlorate of potash, but without any good effect.

*Dr Pattison* had had cases of repeated abortion in one year,—one case where the patient aborted regularly about every six months. He had tried separation from the husband for a time, but without effect.

*Dr Keiller* had tried the chlorate of potass in these cases,—in some with good effect; but it was difficult to attribute the entire credit to the remedy, for it was generally found that there was an improvement in the general health at the same time.

## II. CHINESE CUSTOMS DURING MENSTRUATION.

*Dr Keiller* stated that a naval officer, now stationed in China, had directed his (Dr K.'s) attention to the peculiar manner in which the Chinese women were accustomed to treat themselves during menstruation. The gentleman referred to had, in his communication, enclosed a folded sheet of soft and apparently very bibulous paper, which he stated was similar to that generally used in China instead of the ordinary cloth or napkin used by females in this country. Dr K. read from the letter he had received the following observations regarding the matter; jocularly remarking, that he had no intention of soliciting any order, nor would he promise to commission his excellent and most observing absent friend to "make arrangements" for providing us with "a regular supply."

"You will be much surprised and puzzled when you receive this letter and its enclosure; so I will at once clear it up. I was talking to Dr Medows of Ningpo about the personal cleanliness of the Chinese being so bad, when he told me it was not so bad as I supposed, and mentioned that in one particular they were more careful than ourselves,—that is, with women during their menstrual period, instead of using a cloth as European women do, they use

the paper enclosed, which is folded exactly as it is in the envelope, except that I have turned down one end to make it fit the envelope. He tells me a belt is used, and a cloth to go over the paper and keep it in place. The Chinese amas (servants) to European ladies refuse to wash the cloths which they use, and ladies therefore use the paper, and Dr Medows tells me, prefer it. The Chinese always burn it. I do not know if such paper could be procured at home; but should you think it worth a trial in Scotland, I will be most happy to send you some, and make arrangements for your receiving a regular supply, should it be adopted to any extent, and no substitute found at home. I have troubled you with this because I believe small things sometimes contribute much to the comfort of all, and are frequently overlooked because it is no one's business. I have so little to do of my own at present that I have to look out for some."

Dr Keiller made some observations on the customs of other countries in regard to the menstrual period, and especially referred to the different views entertained respecting the precautionary measures required and adopted. He had frequently conversed on the subject with professional friends who had practised in various parts of the world, and within the last fortnight had, during the medical examination at St Andrews, enjoyed a favourable opportunity of ascertaining some curious facts regarding this matter.

Among the candidates for the degree were several gentlemen who had long resided abroad, either as army or civil medical officers; and from what they stated, it would appear that while in some if not in most parts of India the menstruating female secludes herself during her "uncleanness," in other parts of the East there is no such attempt at seclusion, and not even at concealment, for the fact of their condition is in some countries openly announced by the wearing of a coloured handkerchief or other well-known badge, which is understood to indicate the being " unclean." The use of paper " guards" instead of cloth ones in China, and the burning of the one as well as the refusal to wash the other after being soiled, are readily enough understood when the peculiar ideas regarding handling or making use of anything which has been rendered " unclean" are considered.

### III. CASE OF ABORTION.

*Dr Bryce* showed the preparation, and gave the following history :—

Mrs D. states that she has always been perfectly regular in her family habits: that she is the mother of seven children, all born at the full period of utero-gestation, and all still alive; that she never aborted, and that she had always had several catamenial periods after weaning one child before a subsequent conception. Early in every pregnancy the veins of the lower extremities, from the groins to the feet, become very much enlarged. On this occasion she menstruated last on the 4th of January, and at the end of a month from that date considered herself pregnant, as the usual symptoms were as marked as on former occasions, and soon thereafter the veins began to enlarge. General œdema gradually come on, and by the middle of May (or about the fourth month) the whole body was very much swollen, so much so that the face was quite altered by it. In the end of May, or towards the end of the fourth month, a dark discharge from the vagina set in, and continued, with some short intermissions, up to the 8th of August, or the seventh month, when pains came on, and continued till the 12th, when they ceased, and she felt as if there was something filling the vagina. On the morning of the 14th, while at stool, a fœtus was expelled, and I was requested then, for the first time, to see her, when I learned the above history, and also that the swelling of the body began to subside after the appearance of the dark discharge, and was entirely gone when I first saw her on the 14th August. I found on the bed the apparently four months' fœtus which I now show the Society, and the os so contracted that I could not dilate it with the finger to search for the placenta, which I concluded, from her own statements and her description of a mass that had also been discharged, but which had been put away before my arrival, that it also

had been born at the same time. On the 19th, however, seven days after the fœtus had been expelled from the uterus into the vagina, a very fetid discharge came on, and with it portions of the putrid placenta. She started for London on the 25th, the discharge by that time having entirely ceased. She has since given birth to a healthy child at the full time.

*Dr Pattison* referred to a similar case, an account of which appeared in a former minute of the Society.

### IV. CASE OF PREGNANCY COMPLICATED BY OVARIAN DROPSY.

*Dr Keiller* remarked that the Society would remember an account of this case which he had lately given at a former meeting. The patient had since been safely delivered of a fine male child at the full period. The abdominal muscles were prevented acting in the second stage, and recourse to the forceps was found necessary.

---

## ARMY MEDICAL SCHOOL.

THE following questions were submitted at the examination at the close of the tenth session of the Army Medical School, Royal Victoria Hospital, Netley, between 31st July and 5th August 1865:—

### A.—WRITTEN QUESTIONS.

#### I. *Military Hygiene.*—(Professor E. A. PARKES, M.D., F.R.S.)

1. What amount of sickness does a regiment on home service usually furnish? Supposing this sickness to be in excess, and to be in the form principally of dyspeptic and diarrhœal complaints, with occasional cases of typhoid fever, and that you attribute this to something wrong in the barracks, what points would you particularly investigate, and how would you investigate them?

2. What has been the medical history of the white troops serving in Jamaica? Supposing the diseases which formerly caused mortality to return, what preventive measures would you adopt?

3. What are the weights carried by the British infantry soldier? How may these affect his health? What amount of daily exercise ought a healthy adult of the soldier's age to do, and what is the extreme amount of work which he may be called upon to do? What amount and kind of food would you give, (*a*), under ordinary circumstances, (*b*), when men are undergoing excessive fatigue, as in war?

#### II. *Pathology.*—(Professor W. AITKEN, M.D.)

1. Give an account of the appearances seen on examination of the body after death in cases of *acute* dysentery, and describe how the characters and extent of the lesions may be modified by the influences of certain dietetic causes of ill-health (cachexia). State the place which dysentery holds in the classification of diseases you are directed to follow in the Army Medical Regulations.

2. Describe the anatomical characters of amyloid degeneration, as you have seen this degeneration in the kidney, the liver, and the intestines. Give the composition of the test solution used to determine the presence of the degeneration.

3. Describe the prominent lesions seen at the post-mortem examination of ———, who died 12th July 1865, and whose body was examined on the following day. He had completed ten years of service, and had served in Malta one year, in Bengal three years, and the remaining period in England. He had suffered from ophthalmia at Malta, from erysipelas and remittent fever in Bengal, followed by ulcers on the legs, for which he was invalided, after suffering from them for five years. These ulcers occasionaly healed up. He was admitted into the hospital on 27th June 1865, with open ulcers on the

lower extremities, and on the 2d July (the fifth day after admission) he was seized with shivering. Two days after this shivering he complained of pain on micturition, and great pains referred to the perinenm. He passed his urine with difficulty. There was no history of stricture. The urine passed was turbid, and contained both pus and albumen. The symptoms during the termination of the case indicated septicæmia, with evidence of an abscess in the region of the neck of the bladder. The case terminated fatally on the 12th July, ten days after the shivering.

The points requiring your attention are as follows:—What were the nature and condition of the ulcers? What the condition of the femoral veins? Of each kidney, of the bladder and surounding parts, and of the lungs and pulmonary vessels?

And, lastly, give an account of the pathology of the case, the probable immediate cause of death, and connect the lesions seen in post-mortem examination with the indications of disease during life.

### III. *Military Surgery.*—(Professor T. LONGMORE.)

1. You are required to record in your case-book a full report of the case of an invalided soldier who has recovered from a gunshot wound of the chest. To what circumstances in the history of the case, and to what physical observations of the patient before you, would you chiefly direct your inquiries with a view to determine what the true nature of the injury has been—whether a non-penetrating or penetrating wound of the cavity, and if the latter, whether accompanied or unaccompanied with a wound of the lung?

2. Describe and explain the different conditions of vision which follow the use of atropine when employed so as to produce paralysis of the accommodatory function, according as the eyes thus acted upon are emmetropic, myopic, or hypermetropic.

3. To what causes may pyæmia be usually traced when it occurs in field hospitals, and what precautions should be taken to prevent its occurrence?

### IV. *Military Medicine.*—(Professor W. C. MACLEAN, M.D.)

1. Give the best sketch you can of the influence of military life on the health of the British soldier in India, drawing your illustrations from what you have observed in the Royal Victoria Hospital, and gathered from your studies here.

2. Describe the different conditions under which insolation has been observed in India, and the causes which, in addition to heat, seem to produce it. Give, (*a*), the best preventive, and (*b*), the best curative treatment.

### B.—PRACTICAL EXAMINATION.

#### I. and II. *Military Surgery and Military Medicine.*

Make an examination of the case of ———. You are required to write concisely a history of the case, your diagnosis, prognosis, the probable effects of treatment, and the influence of the disease (or injury) on the man's fitness for service as a soldier.

(Twenty minutes allowed for the examination; half an hour for the description. Written notes may be taken.)

#### III. *Hygiene.*

Chemical and microscopic examination of water. Chemical examination of adulterated beer. Microscopic examination of adulterated milk.

#### IV. *Pathology.*

1. Describe the morbid specimens numbered respectively I., II., III., IV., and V., and in your description embrace the following points:—(*a*.) Name the part shown; (*b*.) Describe exactly the lesions which it exhibits; (*c*.) Describe how the lesions have originated, and name the diseases of which they are

significant, and the period or stage of the disease which had been arrived at; and (*d.*) Connect your account of the condition of the parts with the probable phenomena during life.

2. Examine microscopically the portion of organ given you, name the organ of which it is a part, and give an account of its morbid condition.

3. Demonstrate the urinary tubules with their contained functional cells, and leave the preparation properly displayed under the microscope.

---

# REPORT OF THE MORBID APPEARANCES FOUND IN COWS AFFECTED WITH RINDERPEST AND OTHER FORMS OF EPIZOOTIC DISEASE.

## By Andrew Smart, M.D., Edinburgh.

This *interim* report is restricted to a description of the pathological condition of the animals inspected. The dissections were made, with the assistance of Professor Strangeways, at the Edinburgh Sanatorium and at Tynecastle, and the appearances noted were seen by many persons who were present on these occasions. The parts described and illustrated by drawings and preparations were selected only from examples of the pure and uncomplicated form of the disease.

### DESCRIPTION OF PARTS.

I. *Windpipe and Lungs.*—The entire mucous membrane lining the respiratory passages is reddened and highly vascular, presenting the appearance seen in the early stage of acute bronchial catarrh. It is sometimes nearly dry, but more frequently, especially in the smaller tubes, there is an abundance of frothy mucus, often of a slightly red or sanguineous tinge. The membrane is entirely free of the aphthous eruption which appears in the mouth; and very rarely are there any indications of an effusive or depositive inflammatory condition. The air-cells of the lung, in uncomplicated cases, are healthy; and when an emphysematous condition of the organ exists it is evidently chronic, and not, as represented, one of the morbid states superinduced by the disease.

II. *The Mouth, Pharynx, and Gullet.*—The appearance presented by the mouth is characteristic. The gums, lips, hard and soft palates, under surface and root of upper surface of the tongue, the superior surface of the epiglottis and epiglottic folds of membrane, and the pharynx, are marked to a greater or less extent by an aphthous eruption. This condition has been termed "ulcerous," but we have repeatedly shown that the subjacent membrane is entire. The roughened and granular aspect presented to the eye readily scrapes off, and consists of accumulated epithelium. It collects on the surface of the membrane around the orifices of the follicles. This gives it a punctated or honeycomb appearance, resembling minute ulcers. It enters the pharynx, but is not at all found on the gullet or air-passages. It occurs in only one other situation,—namely, on the vulva, at the junction of the mucous membrane with the integument. The gullet itself exhibits no trace of disease.

III. *The Stomachs.*—The first and second stomachs are generally loaded and distended with undigested food, which indicates their suspended function. No observable change of structure is apparent in either organ, and their lining membranes, as in other portions of the alimentary tract, are not reddened and congested. It is in the third stomach or omasom that the first marked changes of structure occur. These consist of irregular circular patches, varying from the size of a pin-head to a crown-piece. They are characterized by bright red or scarlet margins, which in the larger patches enclose a central portion of the dirty yellow and somewhat gangrenous colour. These very remarkable appearances are not invariably present, and have been met with in only one half of the animals dissected. They are found on the gastric folds or manyplies,

and occur at varying intervals. The central portion of the patch is slightly depressed, friable, quite bloodless, and the papillæ on its surface shrunken, especially towards the middle; but there is not any breach of substance. The spots are found in every stage of advancement, and pass through the following changes:—A single papilla is first attacked, and its vessels become extremely congested. The congestion quickly extends to the neighbouring papillæ, and as the circle widens those first affected entirely lose their vascularity; hence, their vitality is destroyed, and death proceeds from the centre towards the circumference. The bright colour of the outer ring, as determined by the microscope, is due, not to ecchymosis, but to the confluence of the congested papillary vessels. The knowledge of this fact explains at once the sharply defined marginate character of the patches, and their mode of extension.

I would here beg leave to point out a double error into which many observers have fallen. The superficial membrane of this stomach which so readily peels off in sheets, and is found adhering to the plastic surface of the food with which the stomach is usually found distended, is not, as often represented, a diagnostic mark of the disease. Neither is it the mucous membrane, as supposed, that shows so much facility of being removed. It is the epithelial layer which is cast, and the subjacent mucous membrane is left perfectly intact. This change is constantly going on in health, and the membrane can be removed (as I have often done) with like facility from the folds of the third stomach of a freshly slaughtered perfectly healthy animal.

It is in a very special manner the mucous lining of the fourth stomach, or abomasom, that suffers from the inflammatory charge, if such a term can be applied where there are no inflammatory products. The morbid condition appears earlier in some portions of the membrane, but eventually every part is involved in the destructive process. In the earlier stage of the disease the membrane is reddened only a little deeper than in health, but deepens as it advances, and towards the termination is dusky red with interspersed claret-coloured patches. The latter condition indicates a more, perhaps the most, advanced stage of morbid degeneration of mucous tissue.

The membrane, on more careful inspection, presents the following deviations from health. *Firstly*, Its vital attachment to the muscular coat is generally loosened, and at many parts destroyed. *Secondly*, It is soft and friable, easily breaks down under any pressure, and, where the change is furthest advanced, peels off as if cohering mechanically to its sub-mucous connexions. Cracks and abrasions are thus readily formed, which have been mistaken for ulcers. *Thirdly*, The epithelium of the entire membrane is deficient and imperfect, and at many parts quite absent. *Fourthly*, The high colour of the tissue, as microscopically determined, is due, not, as has been stated, to sub-mucous or intra-mucous extravasation, but to vascular congestion in its most extreme form. The vessels being distended to their limits are greatly enlarged, but without rupture or dispersion of their contents, unless artificially produced. *Fifthly*, In some instances, generally in stomachs of animals examined a few hours after death, some small ulcer-like depressed abrasions have been found. These are not true ulcers, and do not penetrate beyond the epithelium. In other instances, black spots, without breach of surface, and evidently due to pigmentation, were met with.

IV. *The Intestines.*—Passing from the abomasom to the lower bowel, the latter is seen to participate in the changes already described, although not to the same extent. The lining membrane of the whole of the intestine is in a state of nearly uniform congestive vascularity, resembling the condition existing in the muco-enteritis of cattle. It is the minuter vessels in the smaller intestine that are mostly injected. These are well seen by the naked eye in the various aborescent forms of their numerous and intricate reticulations. In the large intestine, on the contrary, it is the considerable vessels that are mainly and in a higher degree affected. This imparts to the gut a peculiarly striped aspect. This vascular engorgement increases towards the terminal portion of the canal, and the mucous folds of the rectum exhibit the tumid and

deeply purple appearance of internal hæmorrhoids. The whole mucous lining of the bowels is unduly soft, and its epithelium imperfect. There are no true ulcerations, and in this respect its condition differs broadly from the ulcerative typhoid of man. Not unfrequently a viscid fetid mucous covers the membranous surface. The bowel is usually empty, or its contents are fluid and slimy, but not sanguineous; sometimes there is (as first pointed out by Mr Scott) a discharge resembling the "rice-water" stools of cholera. The ileo-cæcal valve is, as regards function, healthy; but its lining membrane, as also that of the cæcal appendage, is involved in the general hyper-vascularity. There is no sloughing or invagination of the bowel, nor any desquamation of its mucous surface in the form of casts.

V. *Glands.*—There has been much discussion as to the condition of the intestinal glands. I have repeatedly had occasion during the dissections to show that they did not share to any marked extent in the altered condition of the membrane with which they are so intimately connected. They are less prominent, and their outline is obscured by the discoloration of the superjacent membrane. They are never ulcerated, but a chronic tuberculous condition of the solitary glands is of frequent occurrence. This is commonly met with in healthy animals, and is not significant of any particular form of disease. The mesenteric glands show no lesion of structure. They are bloodless and shrunken, and their lacteal vessels are generally empty.

VI. *Kidneys, Bladder, Uterus, etc.*—The pyramids of the kidneys are usually the congested portions, while the cortex is pale, but the structure is entire.

The condition of the lining membrane of the bladder and urethra are variable, but in no instance seriously involved. The uterus exhibits no peculiar feature, but the state of the vagina is characteristic. The vulva is swollen, its membrane tense, and has a very red and irritable aspect. An aphthous eruption appears where the mucous surface joins the integument. A glairy ropy mucus flows from the orifice, and hangs in strings from the vulva.

VIII.—*Heart, Liver, Spleen, Blood, etc.*—The muscular substance of the heart, like the muscular system generally, is flabby and pale. Its condition is not peculiar, but such as is ordinarily induced by many exhausting diseases. There is no valvular lesion or structural change. The large vessels and their lining membranes are healthy.

The liver is of natural size, pale in colour, but sound in structure. The gall-bladder is usually filled with bile, which is thin and of a light green colour.

The spleen is too pulpy, and breaks down under slight pressure. The pulp is composed of broken-down tissue and blood cells of very dark colour. It is the splenic condition of exhausting fever.

The blood when retained in the vessels of a dead animal remains fluid for a considerable period after death (Professor Lister). Forgetful of this fact, it has been assumed that the blood in this disease is "watery and deficient in fibrine." In the single instance in which I have been able to examine the blood, excess of the fibrinous element was found. Should our further researches confirm this observation, additional light will thus be thrown on the pathology of the disease.

The blood is unusually dark in colour, and coagulates quickly and firmly out of the body.

The serous membranes, when the disease is uncomplicated, are healthy, and without effusion into their sacs.

The cellular connective tissue of the loins in some animals is in a perfectly emphysematous condition. It is quite blown up and distended with air, and the appearance presented is unusual and remarkable, but not singular.

The parts not yet examined are the brain, spinal cord, and udder.

### SUMMARY.

The number of cases examined and reported upon are insufficient as a basis of general inference, and the following conclusions are not intended to go beyond our present information :—

1. It is the mucous membranes that manifest the diseased condition principally.

2. They do not all exhibit precisely similar morbid states, nor suffer to the same extent.

3. In some of the membranes the pathological condition is constant and characteristic; in others it is variable.

4. Many of the pathological appearances present in the diseased organs are not peculiar to this malady, and are not distinctive. Thus, (e.g.) the state of the bowel in the muco-enteritis of cattle closely resembles that presented in this disease.

5. The condition of the bladder and uterus is such as occurs in all congested states of these organs.

6. The heart, liver, kidneys, and spleen may be regarded as functionally healthy. They are in the condition which results from exhausting disease of any kind; while the lining membrane of the air-passages exhibits the morbid change which occurs in acute bronchial catarrh.

7. The remarkable rings or patches found on the folds of the third stomach were found present in only a proportion of all the cases examined, and are not consequently distinctive.

8. The condition of the membrane of the fourth stomach is invariable. It likewise manifests the morbid changes in their most advanced and destructive form. It is therefore the most characteristic pathological lesion. The swollen, congested, and aphthous vulva, and aphthous mouth, have also been found invariably present. When these morbid conditions concur with that of the bowel in the same animal, the group is complete and decisive.

9. As regards negative conditions, there is no ulceration, and very rarely any trace of inflammatory products.

10. The reddened colour of the membranes is due to congestion in its extreme form, and not to ecchymosis or extravasation.

11. Emphysema of the lung is not, as has been stated, a concomitant of the disease.

12. Present information would appear to indicate that the blood, instead of being "watery and deficient," is in an opposite condition—viz., that the water is deficient, and the fibrine increased.

13. We must not omit to mention an invariable and characteristic feature of the disease,—namely, the smell of the diseased parts, and especially of the abdominal viscera. The odour once experienced can never afterwards be mistaken. It is peculiar and distinctive.

14. As to complication, a proportion of two-thirds of all the animals examined were affected with pleuro-pneumonia.

---

## THE CHOLERA IN FRANCE.

The appearance of cholera on several points of the shores of the Mediterranean has naturally produced alarm regarding the probable spread of the disease in adjoining countries. There are, however, several circumstances calculated to dissipate these fears. In the first place, there is a diminution in its intensity both in Egypt, where it first appeared, and in Ancona, where it occurred most recently. A second reassuring circumstance is, that the progress of this disease does not take place in the direction of longitude, but in that of latitude, and by zones in some degree climatic. The coast of the Mediterranean presents geological, meteorological, and thermometric analogies which explain the diffusion of an epidemic to several points at once or within a very brief interval. But between the climate of the South of Europe and that of Paris there is almost an abyss. Thus, we can understand how cholera has prevailed at Paris and at Marseilles at very distant intervals.

But another very encouraging circumstance is, that cholera, which has never altogether ceased to show itself in Paris in the sporadic form, seems this

year to have undertaken the task of reassuring us regarding itself by showing itself less frequently and less malignantly than in former years. According to a report of M. Gallard on the prevailing diseases, only seven cases of cholera have been seen in the hospitals of Paris, of which only one was fatal; while, last year, from eighteen to twenty-one occurred every month, with a mortality of two or three in the same space of time.—*Révue de Thérapeutique Médico-Chirurgicale*, for September.

## EGYPTIAN THEORY AS TO THE ORIGIN OF CHOLERA.

A LETTER from Alexandria, dated 23d August, says :—" At a recent meeting of consular delegates, the President of the General Sanitary Department for Egypt, Colucci Bey, communicated the draft of a report submitted by him to the Egyptian Minister for Foreign Affairs, which he recommended to the special attention of the meeting, expressing at the same time the wish that its contents might be brought to the knowledge of the respective European Governments. In this memoir Colucci Bey stated the opinion already shared by all physicians and enlightened men in Egypt, that the cholera, which first made its appearance at the commencement of this century, and has since several times completed the circuit of the world, sacrificing many million victims in thickly-peopled Europe, had its rise in Hedschaz, the Holy Land of Islamism, and notably in the cities of Mecca and Medina, and upon Mount Ararat. The Kurban Bairam, or Feast of Sacrifice, which falls in the first half of the month Zil-hegge, and forms the object of pilgrimage, annually assembles in the Holy City 700,000 to 800,000 pilgrims, who congregate from all points of the Islamitic compass, in order then to return home with the title of Hadji. The unreasonable manner of life,—the nameless filth in which these pilgrims exist during the whole period of pilgrimage,—is sufficient, combined with the murderous character of the climate, to kill a large number. The dead are not regularly interred in the hurry of this wandering life, but hastily shuffled under the desert sand, subject to be uncovered by moderate wind, so that they after a short period infect the air. Added to these miasma come the exhalations from the garbage of perhaps 2,000,000 sheep, offered as a sacrifice to the deity,—for even the poorest pilgrim must offer at least one. The flesh is consumed by the devotees: but the offal, blood, bones, entrails, and even the skin, soon decompose in that glowing atmosphere, so that ultimately a deadly epidemic could not fail to issue from that abundance of stench. This was also the case in the present year, when the festival of the Kurban Bairam fell in the first week of May. It was impossible but that cholera should proceed from such a centre of decomposing animal matter, and it broke out with such violence that in the space of a fortnight 100,000 pilgrims died. The scanty reports that have been this year received from these regions are positively appalling, and an agent of the Egyptian Government writes from Mecca itself that the corpses of the dead waiting for burial were piled up in all the mosques of the town."

## PUBLICATIONS RECEIVED.

Arnott,—Elements of Physics. By Neil Arnott, M.D., F.R.S., etc. London, 1865.

Bigg,—Orthopraxy: A Manual. By Henry Heather Bigg. London, 1865.

Copland,—Dictionary of Practical Medicine. By James Copland, M.D., etc. London, 1865.

Daubeny,—Climate of San Remo. By Henry Daubeny, M.D. London, 1865.

Doherty,—Philosophy of Religion. By Hugh Doherty, M.D., etc. London, 1865.

Hammond.—Lectures on Venereal Diseases. By W. A. Hammond. Philadelphia, 1865.

Rendle.—London Vestries and their Sanitary Work. By W. Rendle. London, 1865.

Salt,—Rupture; its Causes, Management, and Cure. By T. P. Salt. London, 1865.

Smith.—The Treatment of Enlarged Tonsils. By W. J. Smith, M.B. Lond. London, 1865.

Story. — Cholera. By William Story, L.K.Q.C.P.I. London, 1865.

# Part First.

---

## ORIGINAL COMMUNICATIONS.

ARTICLE I.—*Case of Successful Amputation at the Hip-Joint, on account of Malignant Tumour of the Femur.* By JAMES SPENCE, F.R.C.S.E., Surgeon in Ordinary to the Queen in Scotland, Professor of Surgery in the University of Edinburgh.

WHILST it is generally admitted that the true principle in regard to amputation for malignant tumours of bone is to amputate beyond the bone affected, yet in the case of the femur, the dread of the immediate risks of amputation at the hip-joint has led practically to the abandonment of the principle, and amputation through, or near the trochanters, in cases of malignant tumour, is often advised, and resorted to, as being safer than disarticulation, unless the growth is so high up as nearly to involve the trochanter. I think this a very serious error, as, I believe, in such tumours, the disease permeates the whole of the medullary and cancellated texture of the femur, and that there is no security for the removal of the disease except in removal of the entire bone, by disarticulation; and from what I have seen of high amputations in the thigh, for malignant tumours, I believe the risks of amputation through or near the trochanters to be as great, if not greater, than those of amputation at the joint. In the latter, the rapidity of performance is greater, the loss of blood less; whilst the risks of pyæmia are certainly less in cases of disarticulation than in amputation through the continuity of a bone, where we have the chances of myelitis, inflammation of the veins of the medullary Haversian canals, and acute necrosis, which are especially liable to occur in the state of the system accompanying malignant disease. In my own practice, out of above one hundred cases of amputation of the thigh, there have been five for malignant tumours of the femur, and of these, four have proved fatal; whilst the successful case was one of amputation at the middle of the femur, for malignant disease of the condyles.

The following seems to me a case in point, as being the second in which I have amputated at the hip-joint, for malignant growth of the femur; and in both with success:—

CASE.—M. W., aged 5 years, was admitted on May 29, 1865, into the Royal Infirmary, suffering from a tumour of the right thigh.

*History.*—Patient's mother states, that about six months previous to admission the child complained occasionally of wandering pains

in the right thigh, for which fomentations were applied.  About three months previous to admission, however, the mother observed the patient walking as if her right knee was stiff; she had a considerable amount of pain in the limb; was sometimes fretful and low-spirited, but her general health was good.  On being asked where she had pain, the patient referred it to various parts of the thigh; and on examining locally, the mother observed an oval lump on the outside of the lower part of the thigh, about the size of a "blackbird's egg," and said to be deeply seated.  No increase of growth was observed, however, till about ten weeks before admission, when the child is said to have received a blow on the lower part of the thigh (with a chair), and since that time the tumour has been increasing rapidly.  Since then, the patient has also complained greatly of pains in the limb, on account of which the part has been fomented, poulticed, and leeched, without any relief.  The parents then consulted Dr Thomson of Yetholm, who, recognising the nature of the case, prevailed upon them to send her to the Infirmary. During the last few days, the patient has been losing her appetite, is low-spirited, and complains more of pains.

*On admission into hospital.*—Patient appears to be healthy, but is unusually quiet and reserved.  Tongue slightly furred; lungs, heart, and other organs, normal.  A tumour, of oval form, $5\frac{1}{2}$ inches in length, and about the same in breadth, was found occupying the anterior, outer, and posterior surfaces of the middle and lower part of the femur, just above the condyles.  It is of firm consistence throughout, and movable along with the femur.  The superficial veins are enlarged, but the skin is not adherent.  Patient does not complain of pain on pressing the tumour.  The inguinal glands on both sides, as also the right cervical glands, are hard, and somewhat enlarged.

*1st June.*—P. 86.  Sleeps well, and is much better in health. Ordered alterative medicine, followed by tincture of the muriate of iron and cod-liver oil, twice daily.

*6th.*—P. 96.  Has had more darting pains in the limb.  General health continues good.  Tumour not perceptibly larger since admission of patient into hospital.

*7th.*—P. 80.  Slept well.  Chloroform having been administered, Professor Spence made an exploratory incision into the swelling, and discovered it to be a tumour, as diagnosed.  The patient's pelvis was then brought well over the edge of the table, and held firmly by an assistant.  Professor Spence next, grasping the soft parts with his left hand, entered the point of the knife at the front of the ischial tuberosity, and passing it obliquely upwards in front of the articulation, brought it out almost midway between the anterior superior iliac spine and the trochanter major, so as to form a large anterior flap.  Having opened the joint by this first incision, while Dr Gillespie abducted the limb, the disarticulation was effected, and a posterior flap nearly equal in size to the anterior one was cut from within outwards.  Immediately thereafter, a sponge

was applied over the posterior flap; Dr Watson compressed the vessels over the brim of the pelvis. The vessels, including the femoral vein, were then ligatured, the flaps stitched together, pads placed over the anterior and posterior surfaces, and bandaged. When the effects of the chloroform had passed off, an opiate was given, but the patient continued very restless, requiring to be held down for some time. No reactionary hæmorrhage took place.

On examining the limb, the tumour was found to be of a greyish colour, of firm consistence, and of a medullary character. Microscopic examination of the adductor muscles of the thigh showed the nuclei of the muscular fibres to be greatly increased in number, and seemingly about to undergo cancerous degeneration. The fibres of the gluteus muscle showed fatty degeneration, but no proliferous cells.

6.30 P.M.—P. 80. Patient has been sleeping continuously for some hours, is very thirsty and restless at intervals, and vomits on trying to take food.

9 P.M.—P. 100. Urine drawn off with a catheter. Opiate given as vomiting continued.

8th.—P. 100. Spent a very good night. Urine drawn off with a catheter.

4.30 P.M.—Patient has been sinking gradually, and is now unconscious. Pulse, not perceptible; respiration, rapid and very weak; has slight bronchitis; eyelids partially closed, and eyes oscillating. Ordered Spt. ammon. arom. in five-drop doses, every five minutes; also an enema of beef-tea and wine, and a mustard-poultice to the chest.

6.30 P.M.—Still continues in a very weak state. P. 156, and very weak; respirations, 60 per minute; has had her bowels opened freely. Beef-tea injections given every hour, and sal-volatile at intervals.

11 P.M.—Under this treatment patient rallied gradually; but as the nervous symptoms continued, fifteen drops of sol. mur. morph. were added to the beef-tea injections. P. 140; respirations, 40 per minute and regular; has grinding of the teeth, twitchings of the face, with knitting of the brows, and gives occasional starting and delirious exclamations.

1.30 P.M.—P. 150, weak. Cerebral symptoms and general weakness somewhat increased.

9th, 10 A.M.—Has slept well during the night, and cerebral symptoms are absent. P. 140, weak; takes no food, and is supported by enemata; respiration regular and not laboured.

4 P.M.—Continues in much the same condition; has occasional grinding of the teeth and startings of the limbs. P. 112. Supported by enemata.

10 P.M.—P. 130, and weak. Same treatment continued.

12.30.—P. 140, and weak. Consciousness returned and patient is inclined to sleep; cough somewhat increased, and pain in

the abdomen complained of. Ordered a large poultice over the chest and abdomen; takes some food.

10th, 4 A.M.—P. 120. Has been sleeping calmly, but is beginning to start somewhat, and grinds her teeth slightly.

8 A.M.—P. 104. Very weak; is unconscious; grinds her teeth; lies with her eyes wide open, and complains of great tenderness in abdomen. Enemata continued, and poultices on abdomen.

10 A.M.—P. 116. Drinks milk heartily; stump dressed and looking healthy.

12 M.—P. 120. Weak; some of the old symptoms returned. Enema given. From this time forward, patient gradually recovered. None of the cerebral symptoms returned, and appetite gradually improved.

11th.—P. 100. Is in good spirits; slight erythema along the edge of the wound. Some stitches removed. Discharge somewhat increased.

12th.—P. 108.—Patient continues to do well. This morning she took a hearty breakfast of porridge and milk, in addition to tea, toast, and eggs. The stump looks very well; there is not much discharge, and it is of a healthy character. As the cough is troublesome she is ordered to-day the following mixture:—R Ammon. sesquicarb. grs. xxiv.; tinc. scillae, ʒii.; decocti Senegae, ad ʒvi.; sig. a teaspoonful every three hours.

13th.—P. 104, of good strength. During the night she slept well, but was observed to start occasionally. Plasters applied to stump, and stitches removed. Dressed with soda lotion.

15th.—P. 90; bowels regular; appetite good.

17th.—All the ligatures except one have separated. General health continues to improve. At the inner angle of the stump is a hard, red, and painful swelling, evidently an abscess commencing to form; for this fomentations were applied. P. 110.

21st.—P. varied from 104 to 120 during the night. Stump looking very well. Sleeps soundly. Appetite good.

23d.—P. 104. Yesterday, passed four, and to-day eighteen ascarides lumbricoides, of which some were very large. Ordered a purgative, followed by R Santonin, gr. viii.; sacchar, ℈i.; M., et divide in pulv. iv.; Sig. one to be taken night and morning.

25th.—Passed two more worms of the same kind, but smaller. Appetite has fallen off somewhat. The abscess in the groin burst to-day at the outer angle of the stump.

July 3d.—Since last report convalescence has been uninterrupted. To-day passed another small worm, which was the last.

From this date onward the patient improved daily. Appetite returned by degrees. The femoral ligature was withdrawn on July 6th, and the incision healed up without the slightest bad sign, and remains quite sound at the present date.

*Remarks.*—Besides the interest which attaches to a successful

case of amputation at the hip-joint, that just recorded possesses interest in regard to the diagnosis of the disease, and the condition and treatment of the patient after the operation.

The general history of the disease from its commencement; the obscure wandering pains referred to the thigh long before any alteration in the limb was detected, and then the appearance of a small but distinct lateral swelling as described by her mother, together with the subsequent rapid growth and increased pain, were all very characteristic of malignant disease of the femur. On the other hand, however, there was not the slightest appearance of the peculiar cachectic state which so generally accompanies malignant growths; the child was of ruddy complexion and plump, her appetite good, and all the functions natural, though she was restless and somewhat irritable at night. The mother, however, had with her another younger child, evidently affected with strumous swelling of the periosteum, bones, and glands. At the time of the patient's admission into hospital there was no lateral projecting swelling, simply an elongated ovoid swelling, or enlargement of the femur, from the condyles to near the trochanters. Under these circumstances the question arose,—Might not these symptoms be caused by periostitis and subacute ostitis, and perhaps incipient necrosis? To my own mind, the absence of rigors or febrile symptoms at any time during the progress of the disease, the distinct lateral swelling noticed at first, and the peculiar ovoid form of the enlarged femur, seemed pretty conclusive as to its being a malignant tumor of the bone; but still, under the circumstances, before proceeding to such an extreme measure as amputation at the hip-joint, I considered it right to resort to an exploratory incision as recorded in the report. In many instances a free exploratory incision will at once satisfy the surgeon as to the true state of matters; but here it had a tendency to mislead, for owing to the cancerous deposit being situated partly within the medullary canal of the femur, and partly between the periosteum and the shaft of the bone, and easily separable, it resulted that when I introduced my finger into the incision, I felt the bone bare and loosely connected, with swollen periosteum, just as in a case of necrosis, so that I had to enlarge the incision considerably to judge of the true state of the swelling. I draw attention to this, because unless I had been pretty well decided before as to the nature of the disease, the condition presented in this case, so different from the irregular softened mass mixed with osseous spiculæ, generally met with in malignant osseous tumours, might have led to a wrong diagnosis and most disastrous results.

The method of operating in this case was the same as that I adopted in the case of L. S., published in this Journal, vol. viii. page 585, and I think it the best in such cases where we have the power of choosing our procedure. Amputation at the hip can be readily performed by cutting one very large anterior flap, and disarticulating

and dividing the posterior parts almost directly backwards; but as examination shows that even at an early period the muscles near the diseased bone are liable to be affected, it is of vital importance to plan our operations to avoid proximity to the tumour, so as to diminish the risk of retaining any morbid tissue in the flaps. Hence I prefer two shorter flaps to one very long one, as it must encroach more on the altered parts by its greater length. Rapidity of execution in this operation is of great importance, as diminishing the risk from loss of blood; and in cases of tumours where we have the leverage of the whole limb, the disarticulation may be accomplished in from ten to twenty seconds. The chief things to be attended to for its rapid performance are,—attention to the position of the patient; that the hip projects well over the table, whilst the pelvis is kept firmly secured, so as to prevent the body. receding: this allows the limb to be fully depressed after the anterior flap is formed, and also facilitates the other movements necessary for enabling the knife to be passed readily beyond the trochanter major, so as to cut the posterior flap. The direction given to the knife in passing it across the front of the limb to form the anterior flap is all-important as to the ease with which the subsequent steps will be accomplished. In operating on the right thigh, the surgeon, standing on the inside of the limb, which must be abducted, and slightly flexed on the pelvis, should enter the knife immediately in front of the tuber ischii, and carry it steadily in an oblique direction across the front of the joint to a point nearly midway between the great trochanter and crest of the ilium. In doing this great care must be taken to make the knife pass close in front of the head of the femur, so that when the flap is formed and raised, the capsule will be found to be opened, and when the limb is forcibly depressed the head of the femur either at once starts out, or a single cut upon it divides the remaining portion of the capsular and round ligaments, and nothing remains to be done but to clear the trochanter major, and form the posterior flap. In this case the common femoral artery was commanded by manual pressure, and the vessels on the posterior flap by a large sponge firmly applied the instant the limb was severed. The abdominal aorta could have been readily compressed if required; but, as I have found in other cases, the means above mentioned were quite sufficient, and less blood was lost than in an ordinary amputation of the thigh. In this as in other successful cases I have recorded, the femoral vein was tied to arrest bleeding, and I believe, as I have stated elsewhere, that instead of being hurtful it seems rather to do good, by preventing pus or unhealthy discharge entering the venous circulation by the large open mouth of the vein kept patent as it is by its fascial connexions.

The progress of the case as given in the report shows how much success in such operations depends on careful after-treatment, watchfulness of symptoms, and persistence in appropriate remedial measures, even when the condition of the patient seems very hopeless.

It will be noticed that the urgent unfavourable symptoms in this case did not supervene till about twenty-four hours after the operation, when the risk from primary shock and reactionary hæmorrhage had passed. The morning report on the 8th June was, " Pulse 100 ; spent a very good night," and at my visit at noon the child presented no unfavourable symptom ; but shortly after 3 P.M. symptoms of restlessness, nausea, and a state approaching to collapse, as detailed in the report, set in suddenly, and at 4 P.M. she seemed to be rapidly sinking. I believe that, but for her being very carefully watched, and the prompt and continued use of external and internal stimuli, the little patient must have soon died.

The character of the symptoms was peculiar : there had not been the slightest amount of reactionary oozing from the stump, and the child had slept well after the operation. Yet the state must have been the effect of the operation on the nervous system ; although it is just possible that the tenderness of the abdomen, the grinding of the teeth, and convulsive startings, may have been due to the presence of so many large intestinal worms ; for though the more severe symptoms passed off, and she gradually began to amend, yet her rapid and thorough convalescence dated from the time the worms disappeared under the use of the santonine.

ARTICLE II.—*On Uterine Hæmatocele.* By J. MATTHEWS DUNCAN, M.D., F.R.S.E., Lecturer on Midwifery, etc., etc.

I PUBLISH the following cases partly on account of their own intrinsic interest, chiefly with a view to confirm the opinion that intra-peritoneal hæmatocele may be produced by uterine hæmorrhage flowing through a Fallopian tube, even while the natural passages for its discharge are not found to be obstructed ; also to show the possible frequency of this occurrence, and to suggest the mechanism of its production.

*Retro-Uterine Hæmatocele with patent Fallopian Tube.*

In the following case this interesting pathological condition was observed.[1]

A. A. was delivered of twins in the Royal Infirmary on 12th April 1865, and made a good recovery. Both children died within a few days. She left the Infirmary on 15th May in good health, and was readmitted on 27th May. She had felt so well as to resume her usual avocation. Three days before readmission she was suddenly taken with severe pains, which she describes as resembling those of labour, and which continued till some days after admission, when they gradually disappeared. On the day after the supervention of the pains, bloody vaginal discharge com-

[1] Extracted from the Reports of Mr John Wilson Moir.

menced; it was copious, and lasted till admission into the Infirmary, when it gradually ceased.

On admission, examination per vaginam discovered behind the neck of the womb an extremely tender hard mass, which increased in dimensions for several days (up till 31st May), and displaced the uterus more and more forwards and upwards. On 31st May, the vaginal discharge having ceased, a probe was introduced into the uterus; it advanced easily till it touched the fundus at a distance of two and a-half inches from the os; then, its point being moved a little to the right side, the probe advanced, under the least possible force, through the right Fallopian tube. It was pushed a distance of six inches from the os uteri, and its further propulsion was desisted from, not on account of any resistance, but because the patient complained of pain. This use of the probe was a few times repeated by myself, and by Drs J. Carmichael and Banks.

On admission, this patient had much pain and tenderness in the lower part of the abdomen, especially on the right side, where there was great tension and fulness, but where no distinct hardness could be felt.

From the first day of June, all the symptoms began to moderate and slowly disappear. At the same time the tender retro-uterine mass began to lose its sensitiveness and bulk. On the tenth day of July the symptoms were entirely gone. The probe could not be passed farther than two and a-half inches through the os uteri, and the retro-uterine mass was reduced to a mere tightness and hardness, the uterus being now drawn backwards from its natural situation in the pelvis.

In the beginning of September this patient again menstruated, and without any unusual symptoms.

### Recurrent Hæmatocele confusible with Dysmenorrhœa.

Several cases have occurred to me in which an hæmatocele, with all the usual symptoms and physical characters, has been reproduced or aggravated and increased in bulk by renewed hæmorrhage at successive monthly periods. This occurrence has been noted in cases with great effusion of blood; but I have now in view minor cases, which, without care, and especially without physical examination, might easily be mistaken for cases of dysmenorrhœa. This recrudescence of an hæmatocele has been well described by Nonat.[1]

I may here mention a case of severe long-continued dysmenorrhœa in which I was recently consulted, and in which I found a congenital extreme smallness of the external os uteri. The history of this case presented two attacks of what were called "inflammation" in the hypogastrium coming on suddenly during menstruation, and presenting general features more like those of hæmatocele than of any other affection.

The cases to which I more particularly refer now had attacks

[1] Maladies de l'Uterus, p. 343.   See, also, West, Diseases of Women, p. 450.

of hæmatocele at repeated monthly periods. I do not relate them at length. It is sufficient to say that in one patient the disease has occurred twice with an interval of many months; and that in the second attack there was every evidence available under the circumstances of intra-peritoneal hæmorrhage taking place into Douglas's space, in three successive menstrual periods. At no time was the retro-uterine tumour large enough to be felt by external hypogastric examination. This case was under daily observation in the Royal Infirmary.

### Retro-Uterine Hæmatocele after Abortion.

Mrs J. S., æt. 25, was admitted into the Royal Infirmary, under the care of Dr Sanders, on 14th August 1865. She complained of a swelling in the left iliac region, and of recurrent attacks of pain in the same situation.[1]

She states that nearly eight weeks ago she had a miscarriage, at about the end of the second month of pregnancy. The usual discharge continued for two days after the abortion had been removed. At this time it was suddenly arrested, and her medical attendant gave her something to bring it back, but without effect. About a week after the abortion, she suddenly in the morning felt violent pain in the left iliac region, which lasted for about three hours. In the evening, placing her hand over the seat of pain, she discovered a swelling. She has had recurrent attacks of pain, lasting for about three hours once a-week, and generally on the same day of the week. A week ago she menstruated, the period lasting for five days.

She had had a miscarriage five months before that now spoken of. Her menstruation has always been regular.

Her only suffering is from the recurrent attacks of pain already mentioned, and from difficulty in defecation. She has no disturbance of the micturition.

*Physical Examination.*—A tumour is felt in the hypogastric and left iliac regions. Per vaginam the pelvic cavity is found to contain a rounded fluctuating tumour, which pushes forward the uterus against the symphisis pubis.

*21st August.*—Menstruation, which had again come on, has now ceased. Dr Sanders punctured the tumour behind the cervix uteri with an exploring needle, and drew off about an ounce of bloody fluid, which, on microscopic examination, was found to contain no pus globules.

*23d.*—*Examination by Dr Matthews Duncan.*—Hardness with little tenderness can be felt extending from the anterior superior spinous process of ilium of left side to the middle of Poupart's ligament of the right side. Percussion gives absolute dulness for fully two inches perpendicularly above a line from the anterior superior spinous process of left ilium, along Poupart's ligament of left side,

---

[1] From Notes of Dr J. Carmichael.

and across the mesial line to the middle of Poupart's ligament of the right side. Nothing rounded or tumour-like can be felt, except above the horizontal ramus of left pubic bone, which is overhung by the projecting body of the uterus, as is verified by internal examination. The uterus is, by vaginal examination, found to be pressed upwards and forwards, its cervix being behind the left horizontal ramus of pubes. Behind the cervix uteri, and pushing down as far as the middle of the pelvic cavity, is a rounded tumour filling up the left more than the right half of the pelvis. It is soft, not tender, and evidently contains fluid. The uterus measures three and a-half inches in length. The vaginal discharge is tinged with blood.

26th.—Dr Matthews Duncan, with a guarded bistoury, made an opening in the pelvic tumour behind the cervix, large enough to admit the finger. Several ounces of bloody fluid were discharged, and, on microscopic examination, found to contain no pus. The finger introduced into the tumour easily felt the contained clot.

29th.—Discharge becomes fetid. Hypogastric hardness has disappeared, except over left horizontal ramus of pubes. Some pain complained of in pelvis. No feverishness.

1st September.—Yesterday discharge very copious, in clotted masses. Some tenderness of hypogastrium. No feverishness.

5th.—No hardness to be felt in hypogastrium. Uterus in its natural site, fixed, its cavity measuring a little above three inches. Fetid grumous discharge, containing a few pus globules, is now inconsiderable.

18th.—Is now quite well. After three days of greenish discharge in moderate quantity, there is now almost nothing coming away. Uterus slightly mobile; still hypertrophied.

In a former paper on uterine hæmatocele,[1] I laid too much stress on the existence of fluid in the lower part of the sac of an hæmatocele, as a sign of inflammation of the sac, and the admixture of pus with the dissolved blood. The statement then given was founded on the observations I had made. The case first recorded is an example of fluidity of part of the contents without admixture of pus, and the clinical history of the case, the absence of feverish excitement, and the slightness of the local tenderness, are notably in accordance with the absence of pus or pus-secreting inflammation of the sac. This fluidity of bloody contents without purulent admixture is not at all an extraordinary circumstance, being often well illustrated in the history of cephalhæmatoma.

The treatment adopted in this case I do not wish at present to discuss, nor to be held as a precedent. It was perfectly successful, in common with every other operation which I have done in hæmatoceles. The early opening no doubt accelerated the recovery of the patient. The free opening by knife gave easy vent to bulky

[1] Edinburgh Medical Journal, November 1862.

viscid masses, which would only have comparatively slowly dis-
tilled through the hole made by puncturing with a Pouteau's
trocar.  The successful issue does not support the rash assertion of
Nonat, that, if an hæmatocele be intra-peritoneal, the operative
opening of the tumour will be always productive of death.[1]

As I have already said, one of my objects in publishing these
cases is to confirm the opinion that blood or fluid uterine contents
may flow into the peritoneal cavity through the Fallopian tubes,
and this even when there is no obstruction to their escape in the
natural direction through the cervix uteri.

So far as I am aware, this opinion has not been fully stated by
any author; for it appears to me that the accident may happen
without bodily obstruction of any kind to natural progress, even
such as is produced by uterine flexions or by spasm of the excretory
canal.  The fact of reflux from the uterus into the peritoneum has
been ably stated, illustrated, and defended by M. Bernutz, but not
as I propose now to demonstrate it.

To explain its occurrence there appears to me to be no unusual
physical condition required, except a free passage through the
uterine mouth of a Fallopian tube.  This being granted, the
discharge of fluid from the uterus, through the tube into the
peritoneum, or its progress in the natural direction through the
cervix, will be determined simply by the amount of resistance
or the amount of attraction in either direction; the flow will take
place by one route or by the other, or by both, according to ordinary
physical laws.

In a paper which appeared in the number of this Journal for
June 1856, I pointed out that the uterine ends of the Fallopian
tubes must be periodically dilated to give passage to ova, and to
discharges from the tubes; and I adduced cases to prove that
continued dilation of the uterine extremity of a Fallopian tube
in the unimpregnated state occurred as a pathological condition.
Further experience has shown me, that this morbid condition
of continued dilatation is not very rare; for in each of the last
four years I have had more than one case of the kind to demonstrate
to my clinical class.  In most of them the lesion was not discernibly
associated with any special symptom or disturbance.  In all of
these cases the patency was accidentally discovered; and, in esti-
mating its frequency, not only must this be held in mind, but also
that it may not be demonstrable or discoverable even when it is
really present.  That the patency was dangerous it was easy to
fancy; and the occurrence of the first case recorded in this paper
justified the fear that evil might arise from the open tube trans-
mitting into the peritoneal cavity fluids that ought to pursue
another direction.

The openness of the route into the peritoneal cavity being
established, it appears to me easy to explain why this route is

[1] Maladies de l'Uterus, p. 369.

sometimes taken by fluids passing from the uterus in preference to the natural route. This leads into the wide topic of the retentive power of the abdominal cavity,[1] and I wish at present to avoid entering into it, satisfying myself with giving a few illustrations, chiefly from the progress of fluids into the body in unnatural directions, phenomena strictly and closely analogous to that whose explanation is desired.

Every one is aware of the mechanism of the accidental passage of air during operations along large venous trunks to the right side of the heart. In a similar way, after successful straining efforts in bearing a child, the relaxation of the abdominal wall, if the woman be lying on her side, leads sometimes to passage of air into the bladder and into the vagina and uterus. In Marion Sims' position for the operation for vesico-vaginal fistula, air enters and distends the vagina, and, were the passage open, would no doubt permeate the uterus and Fallopian tubes. In persons having the abdominal walls relaxed, and resting on the knees and elbows, air generally passes through the anus into the rectum. The tendency to displacement of the pelvic organs downwards producing sometimes hæmorrhoidal and uterine congestions and their consequences, also descents and procidentia, attracts, on account of the pain and discomforts produced, more attention than the displacement upwards from the reverse mechanical cause. Displacement upwards, or strong attraction in that direction, is often seen overcoming gravitation of abdominal organs. The uterus, even when enlarged by a heavy tumour, sometimes is drawn up into the middle of the belly, instead of falling downwards into or upon the pelvis. The same is true occasionally of the womb in early pregnancy, and also of ovarian tumours of moderate dimensions.

If the student reflects on these phenomena, produced by varying degrees of retentive power, the result of aerostatic pressure in the mechanical arrangements of the abdominal cavity, he will have no difficulty in recognising the importance of continued patency of a Fallopian tube, should there be in the uterus any fluid liable to pass through it. In the first case recorded in this paper there was continued patency, and there was blood in the uterus. I believe it flowed through the tube into the peritoneum, and that the recent twin pregnancy of the woman, and the mechanical condition of the abdomen, with its relaxed anterior wall, were not without important bearings on the production of the hæmatocele. In this case, the aerostatic mechanism just illustrated easily and fully explains the production of the hæmatocele.

It is to be remarked that this mechanical explanation of the passage of fluids through a tube into the peritoneal cavity may also be called in, under some conditions, to account for the retrograde

---

[1] This power is an important, perhaps the most important, element in the maintenance of the abdominal viscera in their natural situations, and has close relations with the amount of flatulence in the abdominal cavity.

course of tubal fluids, or even the abnormal course of natural and unnatural discharges from an ovary into a tube.[1]

Further, the same mechanical circumstances may, in part at least, account for the varying position, as to descent into the pelvis, of the blood effused in intra-peritoneal hæmatoceles, from whatever source.

Lastly, it is necessary to guard the reader from supposing that, with a patent Fallopian tube error of the course of fluids will necessarily or even probably happen. It is only in peculiar mechanical conditions that the error will appear. I have already said that, in the great majority of cases of continued patency that I have seen, no apparent evil has resulted.

This explanation of some uterine hæmatoceles has the great philosophical advantage of finding the origin of the effused blood in the body of the uterus itself, in what is an ordinary and natural source of profuse and of moderate hæmorrhages. And the same philosophical advantage will be, to some extent, available in the explanation of cases of pelvic peritonitis and abscess having the same mechanical history: indeed, also in the explanation of some cases [Haller, Ruysch] of peritonitis post partum, which, if fatal, are very likely to be included under the designation of puerperal fever.

The importance of this advantage is rendered apparent by contrasting the certain frequency of hæmatocele and the believed rarity of many of the assigned sources of the blood in such cases. Experience is constantly giving increased scope to the hæmorrhagic pathology of pelvic tumours, and many of the cases are diagnosed beyond room for doubt. But we have no corresponding demonstrated or probable frequency of large bleeding from a recently burst Graafian vesicle, from a hæmorrhagic bursting of the ovary, from a Fallopian tube, from a ruptured sac of an extra-uterine pregnancy, from a ruptured vein in a broad ligament, etc.

It is apparent that all these sources of hæmorrhage apply only to the explanation of intra-peritoneal hæmatocele,—the site of

[1] In the text I have only mentioned occurrences in close relation with hæmatocele that may admit of the mechanical explanation given. I may here add that sterility and fertility may be partly under dominion of like mechanical conditions, the fertilizing fluid advancing upwards with difficulty, or easily, in different conditions of the abdomen, whether of a permanent or temporary kind. In a farther part of this paper I refer to other occurrences, which may be also explained: as the retrograde movement of lochial fluid, etc. The entrance of lochial fluids and of air into the uterine sinuses has more mechanical difficulties to encounter and overcome than the various transmissions through a tube; but to them also the mechanical explanation here given may come to be applied. Legallois, who cites his father's observations, and those of Chomel, appears to believe in the occurrence; see Journal Hebd. de Med., tome iii. p. 183: 1829. John Reid and Olivier express no opinion as to whether air may enter uterine sinuses or not, but the former quotes an observation by Simpson, to which a different explanation is attached;—see Reid's "Physiological, Pathological, and Anatomical Researches," p. 578. Further, the same mechanical operations may have to do with the causation of extra-uterine pregnancy, and of the wandering of the ovum in women.

the bloody effusion in at least a great majority of the grave cases of this disease.[1] The small bleedings that may be well allowed to be liable to proceed from such sources as a ruptured Graafian vesicle or a tube, may be admitted as the explanation of minor cases, such as some of those already mentioned as confusible with dysmenorrhœa.

In defending the ovarian origin [Nelaton, Laugier, Voisin, Rouget] of the bleeding, MM. Nelaton and Laugier describe the disease as an accident of menstruation. But both seem to feel the insufficiency of this, the blood in menstruation or menorrhagia not owning this source; and, therefore, they call in the aid of an imaginary hæmorrhagic flux, which has its effect on the small vessels ruptured in the small Graafian vesicle.

The tubal source of the blood [Trousseau, Fenerly, Puech, Scanzoni] demands a little more consideration, that its unnaturalness may be shown. Several authors have stated their belief that the mucous membrane of the tubes yields a contribution to the bloody menstrual flow. I have seen the mucus of the tubes tinged with blood in the autopsy of a woman dying during menstruation, and in some other cases. Cases of occlusion at the uterine extremities of the tubes, in which they have been found distended with bloody fluid [See Bernutz et Goupil, Maladies des Femmes, vol. i.] are upon record. Numerous other instances of repletion of the tubes with blood or bloody fluid are to be found, but in these last there is generally no good evidence that the blood was derived from the tubes themselves. It is, however, sufficiently well demonstrated that in some cases blood is excreted from the mucous membrane of the tubes, in small quantity. It may be regarded, I think, as nearly certain that, in natural menstruation and in menorrhagia, blood is not excreted from the tubes in such quantity as to produce a considerable hæmatocele.[2] I have already said that I have seen the mucus of the tubes tinged with blood: I have also seen the mucous membrane of the uterine extremities of the tubes detached, —hornlike,—with the proper decidua uterina in abortion; the hornlike tubal projections from the uterine decidua measuring three lines in length. This detachment from the tubes appears to be analogous to that occasionally observed from the cervix uteri: in both situations some little blood may, no doubt, be excreted in

[1] My increasing experience in this disease and in pelvic abscess, and the (too few) autopsies which I witness, force upon me the conviction expressed in the text, as to the frequency of the intra-peritoneal site of both these kinds of effusion.

[2] The statement by Tuckwell [see his excellent thesis, " On Effusions of Blood in the neighbourhood of the Uterus," p. 7] of his opinion to an opposite effect, is unsupported by any adduced evidence. Were the uterine ends of the tubes as they are generally described, and did the mucous membrane of the tubes supply, as Dr Tuckwell believes, no inconsiderable part of the blood that escapes in what is called an attack of menorrhagia, then great hæmatoceles would be frequent to a very much greater degree than any one at present supposes them to be. Further, the anatomical demonstrations of Rouget are hostile to the tubal source of menstrual blood.

natural menstruation, but neither does afford the hæmorrhage in menstruation or menorrhagia. In order, therefore, to derive uterine hæmatoceles from tubal hæmorrhage, it is again necessary to call in the aid of an imaginary and marvellous local hæmorrhagic flux of the tubes.

The explanation suggested by Virchow, in the first volume of his new work "Die Krankhaften Geschwülste," is perhaps the most unnatural of all, and is, so far as I know, unsupported by the clinical history of cases. He believes the source of the blood to be almost, if not altogether, in the newly produced vessels of false membrane, the result of partial peritonitis in the pelvic excavations. In perusing Virchow's remarks on this subject, one cannot fail to perceive that he is impelled to seek a new theory of the bleeding, on account of the justly considered, far-drawn, and unnatural character of the theories then before him, and this circumstance renders the more remarkable his own ingenuity in proposing a theory of the source of the blood as far-fetched as any of those of his predecessors, which he wishes to supplant.

The uterine origin [Ruysch, Hélie, etc., see Bernutz et Goupil, Maladies des Femmes, tom. i. p. 432] has been the subject of the most lively attack and defence on the other side of the Channel. And, without the mechanical explanation founded on the condition of the uterine mouth of a tube, I could not foresee the conclusion of the conflict; for it appears to me that suddenly occurring hæmatoceles in otherwise healthy women receive no sort of explanation from a study of the numerous cases of prolonged retention of menses, in which the gradually accumulating fluid had forced a passage into the peritoneum. Such cases have no analogy, or only the most remote, with those under consideration. Without demonstration of patency of a tube I can see no satisfactory explanation of hæmatoceles by reflux from the uterus.[1]

In defending the uterine origin of the blood in hæmatocele, I must not be understood as denying that there may be other sources as well; far less would I be taken as throwing any doubt on the numerous narratives of cases in which other sources have been demonstrated by post-mortem examinations.

---

[1] The view ordinarily taken as to the inconceivability of reflux from the uterus is expressed as follows, by Richet (Anatomie Med. Chir., 2d edit., p. 801),— "De même on a peine à comprendre comment ces énormes amas de sang qui constituent l'hématocèle péri-utérine pourraient avoir, pour origine, comme le pense M. Bernutz, le reflux du sang menstruel dans la cavité péritonéale par le canal des trompes." On the same subject we quote from M. Nonat (Maladies de l'Uterus, p. 356),—" La doctrine de M. Bernutz ne peut soutenir un examen serieux ; elle est trop en contradiction avec tous les enseignements de l'anatomie, de la physiologie, et de la saine observation. Ceux qui l'ont combattue ont fait remarquer avec raison qu'un liquide éminemment coagulable, comme le sang, ne pouvait point refluer, au moins en assez grande quantité pour former une hémorrhagie, à travers un conduit étroit, capillaire et tout hérissé d'épithélium vibratile, comme l'est celui des trompes. D'ailleurs n'imputons pas trop à M. Bernutz le tort de cette théorie; elle ne lui appartient pas : ce n'est qu'une seconde edition de la doctrine de Chaussier et de J. Frank."

ARTICLE III.—*On Pepsine in Diseases of Children.* By WILLIAM STEPHENSON, M.D., F.R.C.S.E., Extra Physician to the Royal Hospital for Sick Children, Edinburgh.

*(Read before the Medico-Chirurgical Society, July 5, 1865.)*

THE object of this paper is to illustrate by a few cases the effect of pepsine in some disorders of the stomach and bowels in children. My experience of the remedy now extends over a period of upwards of three years, and the extensive field for practice afforded by the Royal Hospital for Sick Children has enabled me to test its effects in a very large number of instances. I was first led to try it in a case of obstinate constipation, by the recommendation of Dr Tanner in his book on Diseases of Children; and subsequent experience has proved that it is of marked service in various other derangements. Knowing that the action of pepsine can only be upon the food taken, the cases may all be classed under the head, dyspepsia. If so, we must at once see how various are the symptoms in children due to this condition. In one set we have the easily recognised indication of the vomiting of the food in an undigested state; but in others this sign is entirely absent, and although the food may be light and nourishing and the appetite good, the bowels are loose and the child is becoming more and more emaciated; while yet, in a third class, the only symptom is an obstinate constipation of the bowels. The general character, however, of these affections will be better illustrated by a few selected cases.

CASE I.—Joshua S., a healthy-looking boy, of a year old, was greatly troubled with constipation, causing temporary languor and uneasiness. His bowels were scarcely ever moved without some aperient. The usual list of alteratives, tonics, and purgatives, together with changes in diet, were tried with but temporary relief. The food, with the exception of the more indigestible kinds, was never observed in the motions, which were generally darker and much more solid than natural. Three grains of Boudault's powder were given three times a-day with meals. While he was taking it the bowels were moved regularly without medicine. After ten days' trial the medicine was stopped, when he again gradually returned to his costive habit. A drachm of the pepsine was again ordered, divided into three-grain powders, to be given as before for a time, and then gradually stopped. Since then he has continued in perfect health.

CASE II.—James A., 17 months old, suffering from well marked rickets. Has been taking cod-liver oil and tartrate of iron, from which he has derived much benefit. At first his bowels were loose, with a strong fetid odour. This was succeeded by obstinate constipation. Pepsine was prescribed as in the former case. The bowels at once became regular, and in a short time remained so without the use of the remedy.

CASE III.—Mary J. The mother of this child had three still-born children. During the fourth pregnancy she took chlorate of potash, and was delivered at the full period of a small and delicate living child. The skin of the infant at birth seemed dry and wanting in its natural elasticity, and desquamated slightly over the abdomen. A number of small elevated red patches appeared in a few days after, first on the feet and legs, and finally over the whole body. They came and went, and some formed small vesicles. Her bowels were never in a satisfactory state, often confined for a couple of days, requiring the frequent use of castor-oil, magnesia, and grey powder. The cloths were never of a natural colour, generally black and scanty. She cried much, and was very restless. The milk often passed through the bowels in curded lumps. During this time she was receiving nothing but the breast.

8th Oct.—I now determined to try the pepsine, and ordered five drops to be given with each drink. No aperient medicine to be given for two days.

15th.—Three days elapsed without passage, so a dose of castor oil was administered, which resulted in a very copious discharge of healthy-looking faeces ; the first natural cloth, the mother remarked, which the child had had. After this the bowels remained for some time, under the use of the remedy, regular and of good colour, though rather stiff.

This child, now nine months old, has been reared with much difficulty. The stomach and bowels have been frequently deranged ; but the mother soon observed the value of the pepsine, and has frequently employed it without its being prescribed. She states that it frequently has checked diarrhœa.

These cases sufficiently indicate the first class where pepsine is of benefit. Its action also throws light on their pathological nature ; pointing to an imperfect action of the stomach as the true source of the functional lesion. It explains, moreover, the ineffectual if not injurious action of purgatives in such cases. The use of the remedy, I may add, is the only means as yet by which the nature of the cases can be made out. The food is not always passed in an undigested state by the bowel—except occasionally when it is of an indigestible character,—so that the absence of this symptom is not to be depended upon as necessarily indicating a perfect performance of digestion. Several cases which follow will further illustrate the effect of proper digestion of the food in the stomach upon the secretory functions of the intestines, showing how much their actions depend upon the vigorous and perfect performance of the gastric function.

More readily distinguished is the second class, where we have vomiting as the marked symptom. Not the constant nausea and retching of some disorders ; but where, from some morbid condition of the stomach, the food is rejected undigested, either immediately or at a greater or less interval. This condition is often the sequela

of an acute disorder of the stomach or general system. It is then often of a very persistent character; continues long to retard convalescence; yields but slowly to a course of tonic treatment; but in many cases is at once relieved by the use of pepsine.

CASE IV.—M. K., a girl four years of age, was brought from the country to the Children's Hospital for advice. Her mother states that she has never been a very strong child. When three years old she had measles and hooping-cough. A month ago she had an attack of sickness, vomiting, and purging, which lasted a week. Since then she has vomited everything she has taken. The vomiting generally occurs four or five hours after meals, the food presenting little indication of digestion. Milk always returns in a large solid curd. The bowels are never moved except by a large dose of medicine,—magnesia, rhubarb, and senna have no effect: it takes a full ounce of castor-oil to operate. The patient complains of no special pain, but since the vomiting began she has been gradually losing flesh. Sherry wine and lemonade were prescribed by the medical attendant, but with no effect. Ordered a drachm of pepsine wine with each meal.

The mother returned in a week to express her thanks for the advice given, and informed us that the child had never vomited since we saw her. On getting the pepsine she bought her daughter a bun, and gave her a sip of the wine on the way; the bun was retained on the stomach, and since then she has taken the ordinary diet of the family without any inconvenience. The bowels have been perfectly regular without medicine, and she is rapidly gaining flesh. Recommended to continue the remedy for a short time longer.

CASE V.—James T., a year old, of rachitic constitution. A fortnight ago he had an attack of catarrh and diarrhœa. The acute symptoms and cough have passed off; but the diarrhœa continues, with constant vomiting after he has taken the breast or any food. Ordered (March 27) five drops of pepsine wine after each time he takes the breast, and ten after any food.

31st.—The vomiting stopped the day after the last visit, and has not returned. The bowels are still relaxed, but much improved.

CASE VI.—John L., eighteen months old. Had diarrhœa for four weeks, but it has been stopped for about seven days. For a time before the improvement in the bowels the stomach became irritable, and he still continues to vomit all his food. It is generally rejected about an hour after each meal, but sometimes not till the evening, when it has been noticed that food taken at dinner was then vomited. His chief diet is bread and milk and rice-soup, with occasionally porridge. Ordered (5th April) ten drops of pepsine wine with food. Diet not to be changed in any way.

7th.—Has vomited occasionally immediately after food, before the drops could be administered, but never after they were taken. The drops to be given before meals.

*12th.*—Has vomited only once since last date. He is still weakly, and hangs his head much. Continue drops and give cod-liver oil.

We need not wait, however, for the acute stage to have passed off, as in these cases, before administering the remedy. One of the most valuable uses of pepsine will be found in enabling us to ward off the serious and often fatal effects of inanition in children suffering from an acute disease. The stomach then, in sympathy with the rest of the body, is often unable to digest the food presented to it, or from its irritable condition rejects it before sufficient time has elapsed for its perfect digestion. In aiding the enfeebled stomach in the one case, or rapidly hastening the process in the other, pepsine is of service. In the following case, from the very delicate condition of the child, and the well-known rapid effects of exhaustion in infants, I cannot but ascribe the successful issue to the beneficial action of pepsine in allaying the vomiting.

Case VII.—J. W., a very small, delicate seven months' child. For the first three weeks he was fed on wine whey, afterwards on cow's milk slightly diluted, and Liebig's food; although small and delicate he was thriving nicely, and was gaining in size. When about three months old his mother observed him becoming languid, taking less food, and his bowels greatly confined. Vomiting now set in, with sudden prostration of the whole vital powers. He lay restless and uttering a constant whining cry. No other symptom could be detected to explain the attack; there was no spasm, no sign of any mischief in the head, nor was his general appearance that of one labouring under disease of the encephalon. A dose of castor-oil had been administered, and the bowels relieved. To allay the vomiting, and in the hope that it would also manifest its effects on the bowels, pepsine was given, together with wine-whey. The vomiting was at once relieved; the general uneasiness disappeared, and he was able to obtain some sleep. It could now be determined that there was no tenderness of the abdomen, so that all fears of an inflammatory nature of the disorder were removed. For some days he continued in this condition; the vital powers low and flickering, but evidently sustained by the small amount of nourishment he was able to take, and retained to him by the pepsine; for once the remedy was stopped for a day, the vomiting returned, again to be relieved by the continuance of the medicine. But here the action on the bowels was not observed; they continued obstinate, necessitating the occasional use of small doses of grey powder and rhubarb, from which he seemed to derive much benefit. One day, observing the breathing greatly depressed, and a slight cough, a careful examination of the chest was made, fearing collapse of the lung had ensued. A faint respiratory sound was heard over the whole of the chest, but at the base of the right lung there was harshness of breathing, with fine moist râles. Brandy was now given in addition to the wine-whey, and three to four drops of

ipecacuan wine three times a-day. Notwithstanding this complication the infant made a good though tedious recovery.

CASE VIII.—Mary P., fourteen months old, was attacked with measles of a low type, complicated with congestion of the lungs. A due amount of nourishment was here of vital consequence, but the stomach rejected all taken; the milk returning in large curds; some pieces of which were also seen in the stools. Pepsine was given with the milk; it remained on the stomach, and the curded appearance soon ceased by the bowels. The child made a good recovery.

Though pepsine is thus evidently of service in some acute diseases, it must not be expected to manifest its good effects in all cases.

The next case is of value in showing the effect of pepsine in a different condition of the stomach, and although incomplete in itself the effect of the remedy is clearly marked.

CASE IX.—Agnes M'E., six years of age. Since an attack of bronchitis when a year old she has never been very strong, and has ever since suffered from her stomach. For a fortnight at a time she will vomit frequently during the day. In the morning she brings up about a teacupful of yellowish fluid. At each meal she will rise four or five times to eject the food she has taken, together with a large quantity of frothy mucus. She has also frequently been sent home from school for vomiting. These attacks come on at intervals of about a month, and generally last a fortnight, always leaving gradually. The bowels are regular and moved twice a-day. She has occasionally complained of pains in her arms and legs, head and breast, but never of any after food. She is subject to a short cough, and is pale and anæmic. Her hair also is falling out. The present attack has lasted a fortnight. She took the pepsine wine and the vomiting at once ceased. To decide, however, that this improvement was not a natural one but due to the pepsine— although the attack had never before stopped so suddenly—the medicine was suspended for a day. The vomiting at once returned as bad as before. Again it was given, and again she was able to retain her food. Here, however, this interesting experiment was cut short by the child taking measles. The case, however, is under notice.

At the Children's Hospital we frequently meet with cases, the appearance of which has been graphically described by the pen of Dr John Brown. " They are poor little wretches who have faces like old puggies, and are all belly and no legs, and are screaming all day and all night too." The term marasmus or tabes will generally comprehend them. It is not my object here to enter into the consideration of the diathetic conditions which are undoubtedly at the foundation of this state of affairs; but in almost all they are to be found aggravated by errors in diet and other antihygienic

influences. The correction of these errors in diet will go but a short way where disease has obtained so firm a hold on the constitution. The vital powers are often at so low an ebb as to seem incapable of rallying. The condition of the stomach, participating in the general debility, and long sorely taxed by the administration of improper food, is often too weak to perform its function. Here pepsine, I am convinced, will prove of great service.

The following cases, although in some the use of the remedy was combined with other agents, when viewed together with its action in others, will clearly bear out this opinion.

CASE X.—John S., two months old, was brought from the country to the Sick Children's Hospital for advice. He is the first child; parents apparently healthy. The mother states that he was a plump and apparently healthy child when born. When a fortnight old, however, he began to fall off in flesh. He had hitherto only received the breast-milk, but the doctor now ordered corn-flour in addition. He was given a small teaspoonful at a time, prepared by boiling water being poured upon it and sweetened with sugar. This food he had three times a-day, with the breast two or three times, and twice during the night. He has gradually become more and more emaciated, till he is now scarcely like a human child, —the abdomen large, and the skin hanging in wrinkles round his legs. He has had a rash on the buttocks, but this has now almost disappeared; so that it is difficult to say what its exact nature has been. The skin is dry and scaly round the mouth, but there has been no snuffles. He seldom vomits; bowels constipated, scarcely ever moved without medicine; the motions are more solid than they ought to be for so young a child, of a dark generally mixed colour, but not much green.

Ordered five drops of pepsine wine with each meal, and cod-liver oil twice a-day on a full stomach; to begin with five drops, and gradually to increase the dose to thirty; no other medicine to be given, and to have milk and cream only for food.

The mother returned in a fortnight to show us her child much improved. The bowels moved the third day after her visit, " the first natural-looking motion," she said, " almost since birth." They have since continued regular without medicine. He is perceptibly gaining flesh.

The mother again returned two months afterwards, with a fine plump, lively, and in every way healthy-looking child, which she assured us was the same as the withered-looking creature she had brought to us on her first visit. He is still taking only the breast and cow's milk. Three times bread food was tried, but he seemed uneasy after it, and it was discontinued. The pepsine is used occasionally, and he still takes the cod-liver oil twice a-day.

CASE XI.—Margaret N., nine months and a fortnight old, was weaned at four months on account of the mother's health. She has never been a strong child, but was firm in the flesh and thriving till

seven months old, when she cut her two under incisors. She was fed on the bottle till she was nine months, and for the last fortnight has had rusks, corn-flour, and egg-custard. She has been gradually wasting away, and is now (21st December) very emaciated. She is dull, restless, and almost constantly crying. She does not vomit much, but her bowels are very loose, green, and curdled looking. The two upper incisors have been newly cut. Ordered five drops pepsine wine each time she takes food, and cod-liver oil, a few drops to begin with night and morning; the dose to be gradually increased. Diet not to be changed.

28th December.—Bowels are now moved but once a-day, and are quite natural in appearance. She now lies quite contented; does not cry, and is much more lively. A neighbour's remark was, "She is not like the same child." Continue as before.

13th January.—Bowels have continued very regular till a few days ago, when they again became loose and lumpy, but are again better. The child continues to be very contented and is filling up in flesh.

This case introduces us to the last disorder to which I shall allude—namely, diarrhœa. That this symptom is often produced by the passage of undigested food from the stomach is well known. To treat it by the restriction of diet is often most irksome to the child, whose appetite is often voracious, and the directions are rarely carried out. By the use of pepsine, however, this restraint is removed; and unless some glaring error is committed in the diet, I have seldom made any change, being anxious to put the remedy fairly to the test. At the same time a certain restriction I believe in these cases will be found of benefit.

The temptation to multiply the record of cases is great, but I shall limit myself to one more bearing on the point now under notice.

CASE XII.—William F., six months old, a pale anæmic child, has been brought up on the breast, but since about a week old has regularly taken rusk and bread food. For the last six weeks he has been getting soft in the flesh and "failing every day." His bowels are generally moved five or six times in the day, very watery, with greenish lumps and slimy matter. He seems pained each time the bowels are moved, and cries very much. He will scarcely take any food, and what little he takes, together with the breast-milk, is regularly vomited. He was at first ordered lime-water with sweet milk, and no solid food; but from this he received no benefit. The vomiting and diarrhœa continued the same.

17th April.—Ordered five drops of pepsine wine with each drink.

21st.—Has not vomited since the drops were administered. The bowels are greatly improved, moved but twice or three times a-day; no lumps or greenish matter, and are of a good natural colour, though still rather fluid. He is now much more lively and contented.

We have now obtained a view of the different classes of disorders in which experience has shown pepsine to be of benefit, and they are certainly sufficient to warrant us in regarding that remedy as one of great value in the treatment of infantile diseases. There remains one other point to which I would allude. Seeing its beneficial effects in disease, may we not look to it as of value in preventing many of those disorders which we have found it alleviate so readily. Direct proof is here more difficult to give; but wherever the child is delicate of constitution, wherever there is the slightest tendency to derangement, or frequent recurrence of attacks of vomiting, I now recommend the regular use of pepsine. There are strong *prima facie* grounds for thus giving it in all cases where the child is being brought up artificially; and by its use we may yet get over many of the difficulties which attend the nurture of a child deprived of its natural food. Cow's milk is perhaps the only material which as a substitute comes nearest the natural aliment, and which can be readily obtained and prepared by the general populace. To it alone, then, our remarks will at present be directed.

The fact that the stomach of calves is so liberally endowed with pepsine of itself shows the important part which that substance plays in the digestion of cow's milk. That fluid we also know contains a much larger proportion of caseine than human milk. This at once necessitates a considerable amount of pepsine for its digestion. The caseine from the milk of different animals is found to have different bearings with acids and pepsine. The differences between that of human and cow's milk is such as to render it highly probable, that not only do they differ in relative proportion, but also that the caseine of the cow's is more difficult of digestion in the stomach of a child than that of human milk. The gastric juice of man is moreover found to be comparatively feeble as compared with that of other animals. The name of Liebig has lately become associated with a new food for infants, in which the natural process which takes place in the stomach has been partially performed, and thus the process of digestion is lightened. In the same way, seeing that cow's milk is the most easily attainable and generally adopted substitute, yet not perfectly satisfactory, may we not take a hint, and as we borrow from the cow for our supplies, also borrow from the stomach of the calf that substance which nature has so liberally supplied it with to digest that fluid.

Thus far clinical study has shown us the use of pepsine. Recent physiological experiments, however, have thrown light on the subject; and a review of the results thus obtained will enable us to come to a better understanding of its action. Albumen is not wholly digested in the stomach, but is changed by the gastric juice into a soluble form—peptone, and three insoluble forms—parapeptone, metapeptone, and dyspeptone. These latter are passed from the stomach into the bowel, there to be acted upon by a secretion from the pancreas. When the pancreas cannot act, or is not charged

with its peculiar ferment, these unabsorbable forms of albumen are unused. Now "the quantity and activity of the secretion of the pancreatic ferment depend, up to a certain point, on the quantity of absorbed peptones. At least, there seems to be a constant relation between the rate of its formation and that of the rate of the formation and absorption of the peptones by the stomach itself." In removal, injury, or disease of the pancreas, the stomach takes on a compensatory action. But when the function of the stomach is impaired, we may conclude that of the pancreas is also; hence the necessity of a due amount of the peptone-formers. The spleen, however, has also an influence in the charging of the pancreas. When that organ is extirpated, or its vessels tied, an infusion of pancreas was found to possess no power of digesting albumen. The stomach, however, takes on an increase of action.[1] From this we may expect pepsine to be of value in many cases where we have disease of the spleen. The voracious appetite present in many children who are nevertheless becoming more and more emaciated, is explained by this view of digestion.

Some surprise may be expressed at the small quantity of pepsine which has been used in these cases. The best answer, however, is, that the quantity in all the cases proved sufficient; and the tender ages of the patients excludes the possibility of fancy playing any part in the cure. I am aware that, in the minds of many medical men, pepsine is viewed with distrust. Some admit its use, but consider the cases where it will be of benefit as only occasional; others deny any virtue to it at all; while one writer declares that " any advantages derived from its use have been through the medium of the mind."[2] I rather suspect that much of the disfavour which it has acquired must have arisen from either too much having been expected from it, or a fault in the preparation used. Dr Pavy found that only one specimen of pepsine, among many procured by him from London druggists, possessed any perceptible digestive property. Now, however, this source of fallacy is in a great measure removed, and the maker's name is sufficient guarantee of the purity and strength of the article. That which I use in private practice is the wine prepared by Mr M'Kenzie, of George IV. Bridge, who devotes great personal attention to its manufacture, and prepares it direct from the fresh stomach of the calf. I have been led to prefer his wine by finding it always of good strength, undergoing no change by keeping, and devoid of the disagreeable putrefactive smell and taste possessed by all other preparations of the remedy I have seen, and which is sometimes complained of by patients.

While preparing this paper, the views I have adopted have been confirmed, by finding that Corvisart, Debout, Barthez, and Joulin have recorded their successful use of pepsine in many of the infan-

[1] New Sydenham Society's Year-Books, 1861-2-3.
[2] Dr Leared, Medical Times and Gazette, 1859.

tile disorders I have mentioned.  Dr Ballard of London has also testified to its use in similar cases.  But as the writings of these gentlemen are not accessible to many of the profession, this record of personal experience may be of some small value.  I now give it as a second contribution to the clinical study of diseases of children from the Royal Hospital for Sick Children, hoping at the same time that it will tend to the removal of that distrust which still remains regarding what must be ranked as one of the most useful aids in the treatment of disease.

ARTICLE IV.—*Organic Spirality—The Umbilical Cord.*  By JOHN RHIWEN ROBERTS, M.D., Castell, Bangor.

Two theories have been promulgated with a view of explaining the spirality of the umbilical cord, both of which are inefficient for the purpose, and both of which can be easily demonstrated to be erroneous.  The first and oldest hypothesis supposes that the cord is twisted into the spiral form by the rotation which the fœtus is assumed to practise for some occult ends, and that the thing is effected somewhat in the same manner that a ropemaker twines hemp into a cord.  Until lately, this served as a satisfactory reason why the cord was spiral ; when an attempt was made to substitute for it another less fanciful but quite as incorrect a theory, viz., that the phenomenon is due to the unequal size of the two arteries, through which it is supposed that the larger current must overcome the smaller one, so as to cause the fœtus to turn round, and thus produce the spirality.  The one theory asserts that the fœtus *actively* performs the part of a ropemaker, while the other assumes that he rotates *passively*, as the natural consequence of the unequal volume of the currents in the two arteries.  These two hypotheses have evidently been framed to account for the isolated spirality under consideration, and are not applicable in the case of spiral gland ducts, or any of the other numerous instances of spiral arrangements met with in organic nature, while there can be no doubt that spirality is in all cases due to the same cause.  But neither of these theories accords with all the facts of the case of the umbilical cord ; they do not tell us why the turns of the cord are sometimes, although rarely, met with from right to left instead of from left to right, as is ordinarily the state of matters ; they are equally incapable of explaining why the cord sometimes has the turns in opposite directions at each end down to the middle or thereabouts, instead of being spiral uniformly in the same direction throughout its whole length, which is the case in the vast majority of instances.  This fact alone is fatal—inevitably so—to both hypotheses.  It is utterly impossible that the large current can pre-

vail in one half of the cord, and the small one in the other. Yet
this is the absurdity the theory of the "sizes" entails. Equally
impossible it is for the currents to vary as they travel along, and
even if they could thus vary in the two halves of the cord, in
volume or strength, the one would balance the other, and the fœtus
would necessarily remain at rest instead of rotating (as supposed) so
as to occasion the spirality. The advocates of the old theory might
certainly get over the immediate difficulty by assuring us that the
young gentleman who has been amusing himself by gyrating in one
direction, suddenly changes his mind and finishes his rope by
twisting it in the opposite direction for the remaining half; but in
thus avoiding one difficulty they would fall into another quite as
bad, and it would still remain for them to explain why turning in
the opposite direction does not undo the completed half of the rope
before the rest begins to have contrary twists communicated to it.
The fact is, neither of these theories accords with all that is known
regarding the spirality of the umbilical cord. And I shall afterwards
prove that rotation of the fœtus, whether active or passive, would
produce results quite different from what the cord presents; as an
example of which I may here point to the fact, that such rotation
would, of necessity, twist the vein as well as the arteries, while the
latter only are spiral; and gland-ducts, etc., supply us with as per-
fect samples of spirality as the cord, and where it can be demon-
strated that they assumed such form without the aid of any rotating
body attached to one of their ends.

I apprehend, therefore, that the spirality of the cord is but an
example of the extensive class of spiral arrangements to be found
in such plenty in organic as well as inorganic nature, all of which
are due to the same cause; all of which are to be explained in the
same manner; and all of which are the result of the operation of
some general law. Of such arrangements, we meet with instances
innumerable in the province of the physicist. Whenever two
currents of a fluid or liquid meet at an angle, or such a stream
flows into a quantity of such fluid or liquid at rest, we observe
eddies that exhibit spirality in perfection; it is the same when a
fluid flows from all points towards a centre, and hence removing
the plug from a hole in the bottom of a tub full of water, shows us
the liquid escaping in a spiral manner. It is the same with regard
to the atmosphere; storms and whirlwinds are spiral as well as
water-spouts. A stone dropped into water does not go perpendicu-
larly to the bottom, but travels by a spiral course in preference;
and the same phenomenon is observed whenever a body sinks
through a fluid, provided the difference between their specific
gravities is not too great, or, in other words, provided the resistance
the fluid offers to the passage of such bodies is neither too great nor
too small. For a stone falls perpendicularly through air, while
through water it courses spirally; but if the velocity is augmented
so as to increase the resistance, even lead travels spirally through

air, as when shot out of smooth-bore rifles.   Those who believe in
what is termed the nebular hypothesis, think that the cyclical move-
ments, the revolutions and rotations, of the heavenly bodies are
derived from the original spiral motion of the matter that went to
form them, which motion they believe resulted from such matter
collecting towards a centre as in the case of the tub experiment
above alluded to.   It is needless to multiply examples of this kind,
for they are innumerable.   But it is clear that all these spiral
motions must be due to some general law operating throughout the
physical world, and this law I propose to express thus :— *When the
motion of any body through any medium is sufficiently resisted equally
on all sides, it becomes spiral in character.*   It seems to me that this
is a legitimate deduction from the facts presented to us.   I hold it
to be proved that the above conditions are sufficient to produce
spirality.   I have adduced instances enough where such is the fact ;
and in all cases where motion is spiral, it is evident that the condi-
tions under which it occurs offer resistance to it which is equal on
all sides.   I shall, therefore, now leave the province of the physicist
and go on to demonstrate that spirality is due, in organic nature, to
the operation of the same law still ; in other words, that organic
spiral forms are the result of motion being sufficiently resisted
equally on all sides.   I do not mean to deny that spirality cannot
be produced in any other way ; torsion does so in the manufacture
of ropes.   All I deny is, that torsion is the cause of it in the case of
the umbilical cord, gland-ducts, etc. ; and I maintain that torsion
is the exceptional and, indeed, only artificial method of producing
it.   Produced in the manner here pointed out, independently of
torsion that is, spirality occurs so commonly in the animal and
vegetable kingdoms, that I have heard a distinguished physiologist
accusing Martin Barry of saying, that " almost everything was
spiral ! "   I need not, therefore, do more than enumerate a few of
the best known instances of it.   What botanists call rotation, or
the motion of fluids and their suspended molecules and granules
within vegetable cells, is invariably spiral.   Molecular movements,
seen whenever molecules float in a fluid, are, for the most part,
spiral ; being often, however, circular, vibratile, serpentine, or ir-
regular in character, from causes which interfere with the operation
of the law, such as attractions, repulsions, and the like.   I must not
here omit to mention the umbilical arteries in the cord, as it is the
best-marked example of spirality occurring in the human body, and
the only one that has excited speculation with regard to its produc-
ing cause so far as I am aware ; and the ducts of the sudoriparous
glands present us with beautiful illustrations of the same conforma-
tion occurring in our own selves.   In the vegetable kingdom we
observe that the stems of the class of twining plants are spiral as
well as the tendrils and other parts,—of the group of climbers more
especially,—of different vegetables ; while spiral vessels take their
name from the most prominent character which they exhibit, and

the specific name of *Vallisneria spiralis* has a similar origin.    I
will not enumerate more.   These examples show us how universal
the law is in its operation; how it comprehends the most minute
and embraces the most gigantic in all the kingdoms of nature.
We strain our eyes to see the microscopic volutions of a molecule;
we are struck with awe at the magnitude of the cyclical sweeps of
the stars; we observe the tiny eddy, and dread the awful water-
spout; we are puzzled with whirlwinds, and do not understand all
about storms; yet the spirality of all these is clearly due to the
operation of the law given above.   When we turn to organic
nature, we meet with gigantic twiners and microscopic vessels that
exhibit the spiral form, while gland-ducts and umbilical arteries
illustrate the same thing in the human body.   Here, again, it will
be shown that the result is due to the operation of the same law.
But as giving instances, however numerous, of spirality occurring
in the different kingdoms of nature, as the inevitable result of
given conditions, may not suffice to convince some that organic
spirality acknowledges the same cause as the rest, I must now give
the reasons why I have adopted the views I here advocate, and go
on to argue the question out.

In the first place, then, it is well known that in the immense
majority of cases of spiral arrangements, the turns of the spiral have
their direction from left to right, there being, however, a few excep-
tions to the rule.   For example, twining plants wind around their
support in that direction with great uniformity.   The same thing
is still better seen in the univalve shells of the class mollusca, a
very few species only having the left side the largest, so as to
interfere with the ordinary state of matters, and determine the
direction of the turns of the spire from right to left.   The hairs of
animals, the heart in its development from a straight tube, and
other examples of organic spiral forms, afford additional instances
of this.   The duct of the sweat-gland figured in Quain's Anatomy,
is indistinctly spiral as it traverses the corium, where the turns of
the spire are from right to left; but in passing through the epider-
mis the spiral form becomes perfect, and turns in the usual direc-
tion of left to right; nothing, however, is said in the text regarding
the direction normal to the ducts.   Now, the umbilical arteries
follow the usual rule by twining around the vein in a direction,
like twining plants, from left to right; while exceptional cases have
been met with where they assumed the opposite direction,—the
exceptional cases being very rare, as in the instances enumerated
above of exceptions met with elsewhere.   Why the spirals assume
this direction so uniformly I am at a loss to know.   But this very
uniformity leads me to suspect that it is a corollary to the law, the
results of which we are now discussing.   In the case of univalve
molluscous shells, it is true, this seems to be determined by the
right side of the animal preponderating, when compared with the
left as regards size.   Still, I have no doubt that the umbilical

arteries twine around the vein from left to right for the same reason that twining plants do likewise around the stems of the trees supporting them ; for in these cases there is an entire absence of any apparent determining cause of the direction assumed.  Is it a corollary of the law under consideration, or a mere imitation of its operation in the actions of man, that all cork-screws and screws are made with the turns of the spiral in the same direction still—from left to right?

Again, why are the umbilical arteries spiral while the vein is not?  Not only are the existing theories incapable of accounting for this, but the fact that such is the case is fatal to them both. The old theory assumed that the fœtus deliberately revolved of its own accord, and in that way occasioned the phenomenon,—overlooking the fact that the direction of the turns indicated such unanimity in the habits and usages of intra-uterine existence, which the practices of after-life by no means warrant us to have much faith in!  The other theory supposes that the stronger current of the larger artery causes the fœtus to rotate passively, and so avoids some of the difficulties and objections that fall so plentifully to the lot of the former, with which, however, it agrees, in so far as it attributes the spirality of the cord to torsion applied to its free end; in other words, these two theories would have us believe that the fœtus acts either deliberately or passively as a ropemaker. But here the serious difficulty arises (and to me it appears very strange that this difficulty did not occur to the authors and advocates of these theories) as to how it happens that, while torsion would thus necessarily be applied in an equal degree to all the structures entering into the formation of the cord, only the arteries are spiral ; just as if a ropemaker took two cords of hemp and another of flax to twist into a rope, and found that the hempen ones only became spiral, while the flaxen cord remained straight and unaffected in the middle!  It appears to me that these hypotheses had their origin in the erroneous belief that torsion only can produce spirality ; while the fact is, that such is never the case in nature, and torsion is only an artificial source of it.  A twining plant assumes the spiral form independently of it, and what is there to twist the duct of a sweat-gland?  Bubbles rise to the surface of water, and the stem of *Vallisneria spiralis* follows their example in a perfectly spiral manner, when it is impossible that any agency can exist that applies torsion.  Yet who will doubt that in all these cases spirality results from the same cause?  Since it is evident that a gland-duct becomes spiral without being twisted, why may not the umbilical arteries as well?  But I venture further to assert that if torsion were applied to the cord, the result would be very different from what actually exists ; the greater strength, thickness, and rigidity of the arteries would lead to their twisting so closely as to press the sides of the vein together; the latter would be twisted equally with the arteries, and this again

would help to obliterate its canal from its flaccidity; and it would
be so involved and compressed in the resulting rope, that not a
drop of blood could pass along it. It is evident, therefore, that
spirality is uniformly produced in nature quite independent of
torsion, in whatever way applied; and equally clear it is that, in
the case of the umbilical cord, torsion *cannot* have been the cause
of it. It is utterly impossible for the fœtus to rotate without
twisting the vein. Still the vein is free from twists, while the
arteries are spiral. But this demonstration that it is not torsion
(which we have seen is only an artificial source of spirality) that
causes the spirality of the umbilical arteries must not by any
means be regarded as a misfortune. This would be awkward, if we
had no other and better cause to suggest; but this demonstration
merely throws the case of the cord into the extensive group of
spiral arrangements observed to occur in organic nature inde-
pendently of torsion, in place of allowing it to remain propped up
in an isolated and anomalous position by the erroneous theories
that first placed it there; in other words, the umbilical arteries are
spiral for the same reason that gland-ducts are of the same form,
and for the same reason that a pebble courses to the bottom of
water and a bubble rises to the surface of it in a spiral manner.
It thus appears that the existing theories are incapable of being
accommodated to the fact that the vein is not spiral like the arteries,
and it remains for me to show that this is due to the operation of
the law under discussion. The conditions mentioned in it exist in
all the cases of spirality, whether organic or otherwise, and the
cord is not an exception. The umbilical arteries become spiral
because the motion of the blood within them is sufficiently resisted
equally on all sides. In the case of the vein, the resistance is
insufficient, which results from the continuous current within it
being much slower than the interrupted ones in the arteries. For,
as we saw before, a leaden ball travels straight through air and
water when simply dropped from the hand. By increasing the
velocity we may, however, augment the resistance; hence the same
ball that travels perpendicularly through water, will, when shot out
of a smooth-bore rifle, course spirally through air. Nothing can
be more conclusive than this, that its spiral course in the latter case
was due to the increased resistance established by augmentation
in speed. It is analogous in the case of the cord; the sluggish
current of the vein is not spiral, because the resistance is insufficient.
Increase the velocity of the blood or the leaden ball, and their
course becomes spiral. But that this is a fact may be proved by
a better illustration. A pliable tube, along which a stream of
water under low pressure is passing, will be observed to remain
quietly at rest; if, however, the pressure is increased, the velocity
will be also increased, and consequently the resistance, and the
tube will bend in an arched manner at various points; in other
words, the stream and its containing tube will assume evident

traces of spirality. Water poured from a height into a tall glass
vessel nearly full of the same liquid, may be observed to penetrate
into the latter spirally, and the bubbles of air it takes down with
it rise again to the surface in the same manner. The only difference
in the instance of the umbilical cord is, that the stream is confined
in a tube to which it communicates its form. The facts presented
to us forbid any other conclusion.

I need not further refer to the incompatibility of the existing
theories with the facts that the cord is sometimes spiral from right
to left, and at other times has the turns of the spiral in opposite
directions in its two halves; for I have already exposed the
absurdities inseparable from the entertainment of such views.
But these facts are in perfect accordance with the usual operation
of the law here discussed. We have seen exceptions in the
direction of the turns in twining plants, in the shells of univalve
mollusca, in the sinking of pebbles and rising of bubbles through
water, and many more. If, as I maintain it to be the case, in all
the instances of spirality it is the same cause that is in operation,
it would be strange indeed if the umbilical cord did not present us
with exceptions in common with the rest; hence such exceptions
occur. As to the spirals being in opposite directions in the two
halves of the cord, it is merely an instance of the ordinary and
exceptional operation of the law in two portions of the same
current. The transition curve is by no means abrupt or improbable.
Besides, accidental circumstances (pressure or fixation otherwise
occasioned) might interfere with the current at one point of the
cord, and thus determine a change in the turns, in accordance with
the customary operation of the law wherever it produces spirality;
and, as we saw before, the duct of the sweat-gland figured in
" Quain's Anatomy" is spiral in opposite directions at its two parts,
and I have seen the same thing more than once exhibited by the
long hair of a whisker. All these considerations point to the
certainty that we would have all the arteries in the body spiral
if they only came within the operation of the law, like the umbilical,
by swimming in a liquid.

It has lately been attempted[1] to explain all cases of organic
spirality through the operation of " growth under limit;" it being
maintained that a structure growing in length, while its two ends
are permanently fixed in proximity, necessarily assumes the spiral
form, in the same manner that a plumule becomes bent and arched
under similar circumstances, or that nucleated cells growing within
an unyielding capsule are rendered polygonal by mutual pressure.
That growth under limit produces certain forms in organized
structure is a fact, and the cases of bent plumules and polygonal
cells are fair examples of it. But it is easy to prove that growth
under limit cannot possibly cause regular spirality, and the theory
that it does so is opposed by many evident facts and convincing

[1] Cornhill Magazine, vol. ii. p. 313,—" On Living Forms."

arguments based thereon.  For example, the tendency of all flexible
tubes and other structures growing in length while their ends are
fixed, is to arch, coil, and bend irregularly, so as to assume the
appearance of a bunch of earthworms.  Of this we have instances
in plenty in the way the convolutions of the brain are formed, in
the intestines, in the spermatic artery of the ram and bull, and in
the irregular and tortuous coilings of the tubules of the kidney,
testicle, and sweat-glands.  All these present us with the conditions
given in the theory of growth under limit.  The structures elongate
while their ends are fixed, and they ought, therefore, to become
spiral; but such is not the fact.  The umbilical vein is, as regards
"limitation," under identically the same circumstances as the
arteries; how does it happen, then, that the arteries only are spiral?
If it really happens that "limitation" at the two ends of structures
growing in length causes them to become spiral, how are we to
reconcile this with the fact that those structures which are least,
or not at all limited in this way, are most noted for the possession of
the character of spirality?  The umbilical cord is fixed only at
one end, the other floating about in a liquid during most of the
term of intra-uterine existence, yet its arteries are spiral.  Is there
any "limitation" of the growing points of twining plants?  *Vallis-
neria spiralis*, the horns and hair of animals, spiral vessels, the
rootlets of plants, and many more supply us with instances of the
most perfect spirality that is certainly not produced by fixation of
the two ends during growth.  Independently of the proofs I
have adduced, that organic spirality has a different origin, these
facts forbid the conclusion that it is due to growth under limit.

The only way in which growth under limit might by any
possibility result in the production of a regular spiral, is in a
manner that is unmentioned by the author of the theory himself.
The latter endeavours to prove that growth under limit causes the
shells of some gasteropods to become discoid, and the frond of
a fern to have circinate vernation.  If the two ends of a structure
thus rolled up be drawn asunder, it will be changed from the
circinate to the spiral form, as shown in the figures here introduced.
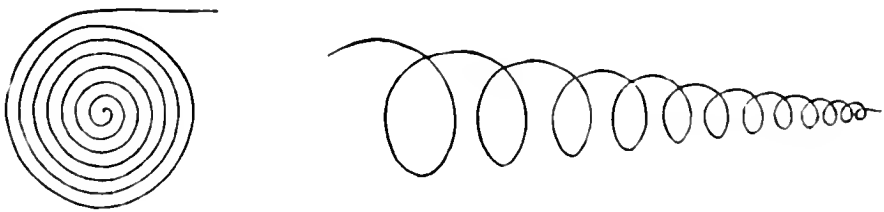


But is there evidence to prove that gland-ducts, the umbilical cord, etc.,
have ever been in this circinate form?  The facts of the case of the
cord prove the contrary; for if it had, the vein must have, of

necessity, been twisted equally with the arteries. Besides, we see that the stems of twiners and of *Vallisneria spiralis*, the horns of animals, etc., assume the same form in a different manner. I therefore conclude that growth is connected with organic spirality only in so far as it produces motion, and that it is in those cases only where motion is resisted equally on all sides that a spiral form results in organized bodies in obedience to a physical law. It matters not whether such motion is caused by the growth of structures (hair) or by the action of the heart on the blood (umbilical arteries); in either case, if resisted equally on all sides to a sufficient degree, it becomes spiral in form with as great a certainty as a bullet courses spirally through the air, or a mountain-rill shows the same character when it empties itself into a lake.

ARTICLE V.—*Chlorate of Potass in the Treatment of Ovarian Disease.* By W. CRAIG, L.F.P.S.G., Ayr.

DISEASE and enlargement of the ovarium have long resisted all kinds of treatment, and many medicines have been used in attempts to absorb the tumour, but hitherto unavailingly. Extirpation for some time past has been the favourite method of treatment, but it is attended with a large expenditure of life,—at an average, nearly every second case. It is very desirable, then, that a mode of treatment could be discovered which would prevent such an expenditure of life, and this usually when the patient is in the most hopeful and interesting period of her existence.

In the following cases the cure has been effected by the administration of chlorate of potass. I could form no opinion regarding the nature of the tumours, other than that they were ovarian.

In his work on the Science and Art of Surgery, Mr Erichsen states, that " medical means exercise no influence in curing, and but little if any in retarding the progress of ovarian tumour."

Dr Charles Clay, of St Mary's Hospital, Manchester, makes the following statement in the London Medical Review :—" I conscientiously believe that neither medical treatment, external or internal applications, pressure, nor galvanism, are of the slightest benefit. They neither cure nor palliate the disease. All such attempts, then, are fallacious, and only throw obstacles in the way of any benefit that extirpation of the tumour offers; increasing the difficulties of that operation, if not defeating it altogether."

The treatment in the following cases is an exception to the above statement, as they were treated and cured by means of a very simple medicine, viz. :—Chlorate of potass. It may be that one only of the species into which this disease is divided may be of a nature to be removed by this medicine, and, consequently, the others may

remain uninfluenced by it; but, during the life of the patient unless after the operation, it is not always possible to learn the exact nature of the tumour; but whatever be its character, it can do no harm to the patient to allow her to have the benefit of a trial of this medicine, as it has a favourable action on the functions of the body, irrespective of the action on the tumour.

Chlorate of potass, as is well known, is a medicine that can be used with the utmost freedom. I do not pretend to offer an opinion as to its *modus operandi* in this disease. The circumstance of this salt having in combination a large quantity of oxygen, which is held feebly by the potass, and is let free in the system, may cause it to operate beneficially on the constitution, invigorating and improving the animal functions so as to enable them to throw off these morbid growths.

CASE I.—Miss S., of Ayr, is of middle height, sallow complexion, and apparently of sound constitution. The tumour is on the left side, rising out of the iliac region. It is about the size of a child's head of a month old. The patient states that the tumour is sore when pressed, also during defecation and micturition. She menstruates regularly. It is free in its attachments, and rolls from side to side as the patient turns in bed. It is about five years since she first observed the enlargement, and it was about a year after this when she first applied for medical advice. She had the counsel of many medical men, and took many medicines, but received benefit from none of them. She was under the treatment of one medical man during the twelve months immediately before coming to me. This gentleman used many medicines and numerous external applications, all without any beneficial effect. He then, with the consent of other medical men, resolved to perform the operation, but he died when preparing for it.

When she came to me I immediately put her upon a saturated solution of chlorate of potass,—a dessert-spoonful thrice daily. She stated that she had only taken the medicine two or three weeks when she felt a gradual improvement in her general health. The tumour gradually diminished in bulk till, at the end of ten or twelve months, it disappeared. After the tumour had been so far reduced as not to be felt through the parietes of the abdomen, it could be felt in its greatly reduced size lying close to the uterus. About this time it was about one and a-half inch in diameter. Subsequently, the tumour has disappeared completely, with the uneasiness and symptoms depending on its bulk, and she has since continued in her usual health.

CASE II.—Miss C., from London. The attention of this lady was first called to her complaint when taking a bath in June 1861. At this time she felt a swelling rising from the right side of the pelvis. It was then about the size of an egg, and moved from the side to the middle of the abdomen. There was no pain in the tumour when the body was at rest, but in quick walking and some

other forms of bodily exertion, it seemed as if bound by a light net-work all over the lower part of the right side.   There was frequently a dull pain in the iliac region, and more rarely a sharp, stinging pain ; but when at rest, or in ordinary walking, there was no pain. The patient states that her health was very sensibly affected by it. " I lost strength and tone, and became listless."   The tumour grew rapidly from June to September, but after this the enlargement was slow in its progress.   The tumour till now was always movable, but subsequently it became more fixed.

She came expressly to Scotland to consult an eminent practitioner, and was under his treatment two months of the summer of 1862, and nearly as long in the same season of 1863.

It was in the autumn of 1863 that she consulted me.   On examining the tumour I noted no particular induration of its texture, but its size was about that of a large fist.   Her former adviser had used many medicines and appliances with but little effect.   The patient was twice cupped and leeched over the tumour, and the skin was twice painted over with a preparation which acted like a blister, and was also painted many times with combinations of iodine.   She had tonics from the beginning, and daily a solution of bromide of potass.   She also passed an electric current through the tumour for half-an-hour daily, and this was done during two years.   The only effect of the treatment hitherto applied was to make the tumour " more compressed or harder."   Immediately on her application to me I commenced the administration of the saturated solution of chlorate of potass in dessert-spoonfuls thrice daily, and with what effect I shall allow the patient to tell. " I will add, that about three or four months after I had seen you, and taken the medicine you prescribed, the swelling disap-peared as it came, silently and suddenly.   I continued your medicine, and the occasional use of the electric battery until a few months ago.   I use neither now ; and as I said before, I have no swelling ; none whatever ; none."

I saw this lady in London a short time ago, and I could not dis-cover a vestige of the tumour.

CASE III.—Miss H., from Glasgow, has had for a considerable time a tumour of the left side in the left iliac region.   It was about the size of a large fist closed.   She had used the chlorate of potass for two or three months before observing much diminution of the tumour, and at this time left off the use of the medicine.   She was induced, however, to commence the use of the medicine again, and she states, that during the last few weeks, whilst using a renewed supply, the tumour has become much smaller, and gives less uneasiness.

A fourth case, in Ayr, presented herself with a small tumour in the left iliac region.   The tumour had been observed for many months.   It was painful, more especially when it was pressed. The size could not be well estimated, as the abdominal parietes

were thick. I commenced with the chlorate of potass, but the patient soon became impatient and would not persevere, and has failed to continue the medicine.

So few cases go but a small way to establish the efficacy of this medicine in the cure of ovarian tumours; but, in view of the declaration of experienced and practical men who have seen much of this disease, and who maintain that medical means exercise no influence in curing, and but little in retarding the progress of ovarian tumours, it is right to produce facts, in however small numbers, when they show that medicine is not so inefficacious as represented by some writers on this subject.

It may be admitted, that before publishing my experience of the efficacy of chlorate of potass in the treatment of ovarian disease, I ought to have been able to present a larger number of successful cases; but in a small town, amongst a rural population, there is less chance of meeting a satisfactory number of cases than in the crowds of large cities.

Though the number of examples be small, the success attending the treatment ought to encourage a trial of a medicine that can be used with the greatest freedom, and could always be tried—provided there is no contra-indication—before having recourse to such a formidable operation as that of ovariotomy.

ARTICLE VI.—*Elephantiasis of the Leg, treated by Ligature of the Femoral Artery ; Death from Pyæmia.* By J. FAYRER, M.D., F.R.C.S.E., Surgeon Medical College Hospital, Calcutta.

A BENGALLI, named Haradun Koomar, aged 30, was admitted into my wards on the 22d February 1865, suffering from elephantiasis of the right leg, of seven years' duration. The leg, below the knee only, is involved in the disease, and the greatest circumference of the swelling, nineteen inches, is just above the ankle. The foot is also implicated, and the mischief is rapidly increasing, with the usual periodic attacks of fever, twice, or even more frequently, in the month. The swelling presents a tumour-like appearance, being limited to the lower part of the leg, and when he has been for some time in the recumbent posture, it becomes flaccid. In other respects he is a healthy-looking man, and has no symptoms of elephantiasis in any other part of the body. He had fever on the 23d and 24th February, but the leg was only slightly affected by it.

*25th February.*—I determined to ligature the femoral artery, as recommended by Professor Carnochan, of New York, and Mr Butcher, of Dublin. The case seemed, in all respects, a favourable one for testing this mode of treatment. Accordingly, I performed the operation at 9 A.M. of the 25th February, assisted by my colleagues in the hospital.

The thigh being in a normal state, there was no difficulty in passing a ligature round the femoral artery, near the apex of Scarpa's triangle. There was slight venous oozing as the aneurism-needle passed between the artery and vein; but it stopped immediately, and did not recur. The ligature being secured, the wound was brought together with four silver-wire sutures; a compress and bandage were applied, and the limb was wrapped in cotton and flannel.

9 P.M.—He is doing very well; no bleeding; the lint over the wound is barely stained. Temperature in axilla, 100°; left leg, 98°; right leg, 94°. No pain; no numbness.

26th.—Had a good night; no fever; pulse, 60. Temperature in axilla, 98° to 100°; in left leg, 95°; in right leg, 96°. Foot and leg feel warm; free from pain or numbness. No hæmorrhage from the wound. No inconvenience, in fact, of any kind. The tumour is more flaccid, and the integuments of the foot more wrinkled than they were before the operation. The tumour has assumed somewhat the aspect of a fold of loose and thickened skin. The limb is kept enveloped in cotton and flannel; and his diet is light and unstimulating.

27th.—He is doing very well Leg warm,—a degree or two warmer than the other. The tumour quite flaccid, and hanging like a loose fold of integument. The foot also is wrinkled. Suppuration commencing around the ligature. No fever; pulse, 60 to 64. The circumference of the tumour has diminished by four inches.

28th.—Pulse, 64; no pain. Limb slightly warmer than the other. Foot slightly œdematous. Tumour much as it was yesterday. There is slight purulent discharge from about the ligature. He is in all respects doing well. The limb is kept enveloped in a flannel bandage. His diet is the ordinary simple food of the natives.

5th March.—He had slight fever yesterday; hitherto he had been doing well. The sutures have been removed, but the ligature still adheres though the wound has cicatrized round it. The tumour is still shrinking, and so is the thickened integument of the foot. The temperature of the limbs is now equal.

6th.—No return of fever. The discharge from about the ligature continues; it is healthy.

9th.—He has had fever and diarrhœa since yesterday; but the leg is not affected. The tumour is smaller; and the skin is also shrinking. The foot is slightly œdematous. The tongue is moist; but the pulse is 120 this morning. He says the fever is similar to the periodic attacks from which he formerly suffered, but in this instance the tumour and foot are not affected. The temperature of the limb is good. The house-surgeon gave wine, an astringent to correct the diarrhœa, and quinine for the fever. His diet consists of beef-tea, milk, and a little port-wine. The discharge from the ligature is healthy.

10th. — He is much better to-day. Pulse, 100; no fever.

Diarrhœa better.  Continue the quinine, and, with it, ferri. sulph. gr.i., ter die.

*11th.*—He has had fever again during the night, but is free from it this morning.  Quinine has been given in ten-grain doses.  The leg is doing well; it is the same temperature as the other; and the tumour is not only smaller, but the skin over it has shrunk, and the foot is less œdematous.  The ligature is still firm.

*12th.*—Has had fever again this morning, and is bad in consequence ; but the leg looks well.  The tumour is now fifteen inches ; it was nineteen inches in circumference when he was admitted.  It appears that the accessions of fever take place twice a-day.  He has also had rigors.  He has ten-grain doses of quinine every four hours.

*13th.*—Fever again, and great depression ; ligature still adheres. Pulse is more rapid ; breathing, hurried and harsh.

*14th.*—He is worse this morning ; breathing hurried, countenance anxious ; pulse very rapid and feeble: I fear he is sinking.  Still the limb looks well in all respects.  The wound is healed, except just round the ligature, which came away this morning.  The temperature of the limbs is equal, and the tumour has diminished nearly five inches in circumference.

*15th.*—He gradually sank, and expired at 2 A.M.

*Post-mortem* at 7 A.M. of the 15th March : *Abdomen.*—Liver and spleen healthy.  Kidneys looked healthy externally, but, on section, a quantity of pus oozed out of the structure of the organs, and the pelvis contained purulent matter.  Iliac veins on both sides were healthy.  The right vein contained clots.  No suppuration within the pelvis.

*Thorax.*—Lungs apparently healthy in anterior surface; but, posteriorly, they contained several patches, some of them large, of dead lung-tissue,—one nearly as large as an orange, not containing pus, but dead lung-tissue and sanious matter.  I examined the structure carefully under the microscope, but found only lung-tissue, granular matter, and some shrunken white corpuscles.  I have elsewhere pointed out the frequency of this result of pyæmia, or capillary embolism in the lungs, and expressed my opinion as to the nature of the formation of many of *the so-called pyæmic abscesses in the lungs*,—that in those cases where pus is found in or around these patches of dead lung-tissue, it has occurred as a *result* of the local deaths, and not *as a precursor of it :* the suppuration being the result of the natural effort to throw off the dead portion of lung-tissue, which has been *killed* by capillary embolism, just as the " core " of an ordinary boil is killed, where a portion of areolar tissue perishes under the skin, and an abscess (the boil) forms around it in the effort to get rid of the defunct portion of tissue.

There was some aplastic exudation on the pleuræ, and especially on the pulmonary pleura, at the base of the right lung, where a friction sound was heard the day before death.  There was also some sero-purulent fluid, with flakes of lymph, in the pleural cavities.

The pericardium contained more than the natural quantity of fluid ; it was of a yellowish turbid appearance.

The right ventricle of the heart was completely filled with a firm yellow fibrinous clot, which had moulded itself in the cavity, and extended far into the pulmonary arteries.   The left ventricle contained a smaller though similar clot.

The femoral artery was injected, and the anastomotic circulation traced.   In the main trunk it ran to where a small branch was given off, not very far above the ligature ; below this the vessel was contracted where the ligature came away.   The vein was discoloured, and the parts softened and broken down, but on slitting it up, there was no sign of disease.   The lining membrane was smooth and clean.   The vein was laid open up to the common iliac ; but, beyond the clots already mentioned, it presented no sign of disease.

*Remarks.*—Notwithstanding the fatal termination of this case from pyæmia, the result, so far as the decrease in size of the elephantoid tumour is concerned, is satisfactory, and tends to confirm the views of Professors Carnochan and Butcher.

The ligature of the main artery was attended with no evil consequences locally.   The circulation in the limb was rapidly re-established, and the tumour did not either ulcerate or slough.   The diminution in its size was steadily progressive, and not only did it become more flaccid, but the integument gradually was shrinking in proportion.

The case was a very favourable one for testing the value of this mode of treatment, and had not the evil influences of the place, in the form of pyæmia, intervened, it appears probable that complete success would have been attained.

ARTICLE VII.—*Case of Femoral Aneurism successfully treated by Digital Compression in the Groin.*   By Surgeon-Major J. KENNEDY, Trichinopoly.

MAY 9, 1864.—Samuel Belfield, sergeant-major, Trichinopoly garrison, æt. 36, an Englishman, resident in India six years,—temperament lymphatic, habits temperate, an anæmic and delicate-looking man,—was reported sick in quarters on 7th of this month, and removed to hospital this day.   When first visited, two days since, he stated that, about a week before, a small fluctuating and slightly-painful swelling appeared at the inner side of the lower part of the left thigh, which gradually increased ; and thinking that it was of a phlegmonous nature, he painted it with tincture of iodine, which has reddened and slightly abraded the integument.   The tumour is distinctly an aneurism of the femoral artery, being of the dimension of a large-sized billiard-ball, and situated at the upper

part of the inferior third of the thigh, where the artery is about to turn to the posterior aspect of the thigh. There is strong pulsation in the tumour synchronous with the arterial pulsation, and by compressing the artery in the groin the tumour becomes flattened and ceases to pulsate, and on removing the pressure the swelling and pulsation at once return. Treatment by compression having been previously decided on, it was tried whilst he was in his own quarters, but not effectively, from want of proper apparatus and assistance; he is accordingly removed to hospital; and as there is no special apparatus for compressing the artery procurable, six European orderlies have been detailed for that purpose, and have been instructed to effect, by means of the thumb, constant compression of the artery in the groin, relieving each other in turn.

10*th.*—Found that compression of the artery, from some unexplained cause, had not been continuously kept up during the night: the aneurismal tumour is consequently much in the same state as noted yesterday. Strict orders issued to see that the orderlies (who are to be relieved at regular intervals) compress the artery as previously directed. Chicken-diet.

*Vespere.*—Pressure kept up steadily since morning; aneurismal swelling less prominent; pulsation slightly decreased; some uneasiness from the pressure in the groin. Compression to be continued. ℞. Tinct. hyoscyami, ♏40; spirit ether. nitrici, ʒss.; mixt. camphor, ℥i.; M. ft. haust.; to be taken at 8 P.M. and at midnight.

11*th.*—Compression has been constantly applied. Tumour decidedly smaller, being considerably flattened and circumscribed; force of pulsation diminished; slept at intervals, and does not complain much of the pressure. Compression continued.

*Vespere.* — No material change. Continue the compression. Repeat anodyne draught at 8 P.M. and midnight.

12*th.*—Slight diminution of tumour; pulsation of same force as yesterday. Continue the treatment.

13*th.*—The tumour has materially decreased; it is harder, more circumscribed; and when the artery is left uncompressed, the pulsation is observed to be lessened in force. Compression continued. Anodyne draughts to be repeated as before. One pint beer.

14*th.*—Tumour smaller and harder; less pulsation; complaining of the pressure. Treatment continued.

15*th.*—Slept pretty well; tumour continues to lessen; solidification slowly taking place; pulsation much decreased. Treatment continued.

16*th.*—Complaining much of the pressure in the groin, which has been in consequence shifted to a spot about an inch lower. Tumour of same size but more solid; pulsation is now very feeble. Continue the treatment.

17*th.*—No pulsation whatever in the tumour, which is somewhat larger, entirely solidified, and hard to the touch. It is stated that

the pulsation ceased about midnight; has complained much of the pressure in the lower part of the groin, and he is irritable and restless; compression to be changed to site first selected, higher in the groin, and to be continued in a slighter degree forty hours longer. Repeat anodyne draughts.

18*th.*—No pulsation in tumour, which has become somewhat smaller and softer. Continue compression, and repeat the anodyne draughts.

19*th.*—Tumour softer; no pulsation; compression discontinued at midnight, and has not been resumed. Patient directed to remain quiet in bed; no compression. Full diet.

20*th.*—Tumour continues to lessen; no pulsation.

21*st.*—Tumour less prominent.

22*d.*—No material change.

24*th.*—Tumour slightly decreased; no pulsation; looks pale and weak, and he is evidently suffering from the intense heat of the weather. ℞. Ferri et quinæ citrat., grs. iij.; tinct. zingiber, ʒss.; aqua cinnamomi, ℥i.; M. ft. haust. bis die sumend.

28*th.*—General health has improved within the last four days. The tumour is less prominent; but, on a minute examination, whilst the muscles are relaxed, it is found to be of nearly the size noted on his admission into hospital. No pulsation detected; and there has been since the 17th instant distinct occlusion of the artery. No pain or uneasiness experienced; has been permitted to sit in a chair with the limb elevated and supported. Continue mixture.

1*st June.*—Tumour very slowly decreasing; no uneasiness; general health materially improved. Continue medicine.

5*th.*—Tumour decreasing; it is firm and entirely free from pain, even on pressure; allowed to walk about the ward. Continue medicine.

12*th.*—The tumour has materially diminished; no uneasiness. Continue medicine.

14*th.*—Tumour now about half its original size. Patient moves about without any uneasiness; discharged from hospital, and permitted to reside in quarters during convalescence. Omit medicine.

The foregoing case of femoral aneurism of a large size, treated by digital compression, speaks for itself. Want of proper apparatus (the common screw-tourniquet having been tried and found un-suited) necessitated the employment of the mode of compression above noted, which, though neither perfectly constant nor very accu-rately regulated, had the desired effect, viz., the gradual deposition of fibrine in the aneurismal sac, and complete occlusion of the artery on the seventh day.

31*st December.*—I have repeatedly seen and examined Sergeant-major Bellfield since June; and on the last occasion (about a month ago), there was not a vestige of the aneurismal swelling perceptible, and its site was entirely free from pulsation.

ARTICLE VIII.—*On the Prevention of Smallpox.* By THOMAS
WARDEN, M.D., R.N.

To this disease all must allow that the trite proverb, "Prevention
is better than cure," is more peculiarly applicable than in the case
of most other diseases.  The following remarks are intended prin-
cipally for the use and direction of the medical officers of the navy
and army; but in present times and circumstances the preventive
measures I propose are difficult and often impossible to be had recourse
to.  The line of proceeding I suggest is, that on the appearance of
variola, a system of revaccination, together with other minor but
important details to be mentioned later, should be begun at once.

The circumstances which have led me to the conclusion that
this is a safe and beneficial plan to pursue, are derived from my
own personal experience of the benefits accruing from it on more
than one occasion.

I wish to lay before those in authority the great difficulty
experienced in procuring lymph, for it is to the practitioners on
shore that the medical officers of the navy up to the present time
have been indebted for their supply of vaccine.

About a year ago, a circular relative to vaccination was issued
from the Admiralty, stating, "that every man or boy volunteering
for the service, who has not had smallpox, nor been vaccinated, or
who presents a doubtful cicatrix, shall be sent to hospital to have the
operation performed, and should there be no naval hospital within
reach, he shall be vaccinated on board the ship at the earliest
opportunity, and as soon as lymph can be procured."

As matters are at present arranged, I do not see how the spirit
of the circular is to be carried out.  Means ought to be adopted
through which it would be in the power of medical men to
vaccinate wherever they considered the operation advisable.  Surely
the lying-in hospitals of large towns, together with the out-and-
in-door practice of dispensaries, would afford sufficient lymph for
the supply of both services, if properly collected in hermetically
sealed tubes, in which the vaccine will retain its properties for any
length of time, provided proper care be taken to exhaust the air
before the tube is sealed.

I beg now to relate the circumstances upon which I base the
claims of these preventive means to a place among prophylactics.

On being appointed resident-physician to the smallpox and fever
wards of the Edinburgh Infirmary, my first proceeding was to
vaccinate myself.  Small pustules appeared in due time, running a
short and mild course with little fever.  While in attendance on
these wards, I was necessarily a good deal in contact with the
disease in all its stages and forms without being at any time affected.
I would recommend this revaccination to all students attending the
wards of smallpox patients.  Again, in the year 1862, smallpox

broke out in the ship in which I was assistant-surgeon.   We were stationed at Naples, and the origin of the outbreak was easily traced to an Italian frigate moored at a short distance from us.   This vessel had upwards of forty cases of variola on board.   The first case which occurred with us was slight, the others more severe.   Two blacks then became affected—one was a genuine African, the other a native of Madagascar.   The former had hardly a trace of a cicatrix, its position on the left arm being the only evidence which would lead to the supposition that it was the result of vaccination. This, as might be expected, was the worst case we had, the man being ill only eight days; a severe attack of hæmatemesis terminated the scene.   The chaplain afterwards became affected, and as he was at the time convalescent from an attack of remittent fever, he also sank after an illness of fourteen days.

All these cases were provided with quarters on shore, and were under my care for nearly a month.   They were visited twice daily —the worst cases as often as three times.

Through the kindness of Dr Sim of Naples, I got two vaccine points, with which I again vaccinated myself, allowing an interval to elapse between the operations.   The first presented but a very small pustule, and less inflammation than was present after the vaccination four years previously.   The second point produced no pustule at all, and only a slight inflammatory blush consequent on the mechanical injury of the puncture.   This I looked upon as a sign that the system had been thoroughly tainted, so to speak, by the three operations, and prevented me from being obnoxious to the infection.

I consider, from my experiences on these occasions, that I am justified in bringing this method forward as a sure and important preventive measure.   Further, I am disposed to believe that if an unvaccinated person was to undergo the operation, and then be exposed to infection, he would pass through the ordeal unscathed; nature, as a rule, being averse to carry on two morbid processes in the same body simultaneously.

The recommendations which I beg to lay before the profession are as follows: and if they are strictly adhered to, I think an epidemic of smallpox would be much modified and restrained.

I take the case of a ship, but the remarks apply equally to the army; indeed, the measures would, I should think, be more easily carried out in that service than in the navy, as separation can be more readily and perfectly attained on shore than on board ship.

1st, On the appearance of the disease, separate the affected man from his comrades.   Ascertain, as far as possible, where and when it is likely that he became infected.   Learn also who were his companions on that occasion.   Place his messmates, and those who have their hammocks slung near his, under surveillance for some days.   In the case of the army, those who sleep in the same barrack-room as the affected man are to be carefully watched.

2*dly*, Vaccinate all these men. The place to perform the oper-
ation is of some importance; it ought not to be done directly above
nor directly below the old cicatrix, but upon one side or other.
Let these men then, with their immediate attendants, be kept
separate and under observation for about a week; in the course of
which probably more than the half of them may be discharged to
duty, as in some the vaccine will not take at all, and in others but
slightly.

3*dly*, Let a man who has had the disease be the only means of
communication between the separated men and the galley or
kitchen, etc.

It has not been my intention to speak dogmatically on this
subject, as I have only myself to bring forward as evidence of the
efficacy of the plan, in which I firmly believe. I would wish others
who have the opportunities and facilities for making experiments
on this subject to do so, and communicate the results arrived at to
the profession; as all must admit, that if by any means the scourge
of smallpox can be averted or disarmed of some of its more terrible
phases, surely no stone should be left unturned to arrive at so
desirable a result.

H. M. S. PSYCHE, *Malta.*

---

ARTICLE IX.—*On Temporary Insanity.* By W. LAUDER LINDSAY,
M.D., F.R.S.E., Physician to the Murray Royal Institution for
the Insane, Perth.

I. *History of a Case:*—About seven o'clock, one evening last
winter, I was hastily summoned to a neighbour's, to see one of his
servants, a cook, (of whom I may more conveniently speak here-
after as A. B.), who was said to have become suddenly and
alarmingly ill. The messenger sent explained, on cross-examina-
tion, that the patient was "out of her mind;" that something had
"gone to her brain." Losing no time in obeying the summons,[1]
I found the household, a large one, thoroughly upset; its members,
especially the domestics, consisting of three women and one man,
in a state of great alarm, their energies paralyzed.

I first saw the family, and learned from them that nothing had
been observed amiss in the patient's health, mental or physical, or
in her behaviour, till she was dressing dinner about an hour before,
when she suddenly dashed down the dishes, rushed to a window,
and drove her hand through a pane thereof, becoming greatly
excited, and weeping violently, protesting that the dinner was
wrongly prepared, and that she was looked down upon by her
fellow-servants. It appeared that she had, at some prior period,

[1] I may explain that my visit was altogether *non-official*, and, in one sense,
*non-professional;* it was simply that of a neighbour and an old acquaintance of
the family.

been a cook in the same house; that she had thereafter served in a similar capacity in a gentleman's house in the country; but at last term, only four days before, she had returned to her old master. She imagined that, in consequence of or in connexion with this return to her old master's house, she was despised by her companions. She had been sent into town in the forenoon of the same day, and this fact was connected, by the family, with the sudden outburst of mania: drink and delirium tremens being the supposed cause and effect. I had, however, a private interview with her sister, who was one of her fellow-servants. She assured me that neither drink nor disordered uterine functions had anything to do with the causation of the outbreak, which she attributed entirely to emotional excitement. All subsequent inquiry went to prove the correctness of the girl's surmise and statements. Her fellow-servants generally were ready with narratives of *prodromata*. They described the patient as having been " peculiar" all afternoon,— " fidgetty," " fast," " funny;" as naturally " touchy " or irritable; as having long complained of " pains in her head," which, however, her sister referred to *tic*.

Soothing measures were found not only useless, but the attempts made only added to the excitement. She raved and tore her hair violently, and all her actions for a time were those characteristically described as " frantic." She showed a determination to escape from the premises, which escape was—dreading some accident therefrom —very properly prevented by her master. Fortunately he—and he alone of the household—had the strength of arm, as well as firmness, friendliness, and tact necessary to her control. But this physical strength she tested to the utmost in more than one struggle with him, the results of which were fortunately for both parties, as well as for the household, in *his* favour. During her first paroxysm, he saw her alone; for all the other members of the household were too timid, too much alarmed to be capable of any effectual assistance. In a struggle which ensued, she smashed a window and endeavoured to get her head through the framework,—picking out the pieces of broken glass,—whether with a view to suicide or escape, or with no specific object, it is impossible to determine. It became necessary to shut her up by herself in a larder, where, however, she was surrounded by knives and other lethal weapons, which might have been rendered of deadly service.

Under all the circumstances of the case, *as described to me*, for I had not as yet *seen* the patient, I suggested that this excitement would probably prove a mere paroxysm, and would be transient; that it would, as yet, be not only unnecessary but injudicious to commit her to a Lunatic Asylum; that it was probably better I should not visit her, inasmuch as being known as the head of an Hospital for the insane, the effect on the patient at some subsequent period, if not at the time, might be prejudicial; that should it be necessary to have her visited by a physician, the proper person was

the ordinary family physician; and that removal from the sources
and scenes of her irritation, seclusion and quiet, with sleep, were
the measures immediately indicated as likely to be beneficial.  The
patient's sister, however, as well as the family generally, were
urgent that I should see her forthwith; the more so that she was
now heard to be kicking violently at the door of the larder in which
she was confined, and there was a general impression or dread that
some accident to life or property might occur.  The only course
open to me was to comply with this request, and I accordingly
accompanied the master of the house to the patient's temporary
place of confinement.  I found her a stout, plethoric, bloated woman,
apparently about forty—a subject for Bantingism—weeping vio-
lently, and in a state of great excitement.  The burden of her
plaint was that she wanted away—must be away—forthwith—now
—anyhow—anywhere.  Her master's house had evidently become,
for some reason not explained, unbearable for the moment, and she
was determined to leave it by any manner, promising, however, to
return next day and resume work.  When soothingly addressed by
her master, who expressed his good opinion of her services as cook,
she at once shook her head decidedly, weeping bitterly, and ex-
claiming, No, no! sobbing forth that she had been " a bad servant,"
and had " done nothing that was right."  I was desirous of having
her removed from the larder, both on account of its coldness, and of
the danger arising from the numerous weapons that might, in a fit
of frenzy, be turned to desperate account.  I therefore proposed, with
a view to prevail on her to go to bed, that she should go up stairs to
her own bedroom.  She gave her promise that she would go quietly
up stairs were the larder door unlocked.  It accordingly was so;
but the moment egress was given she fled along the passages of the
large house.  The household were neither to be heard nor seen;
apprehending violence from coming in contact with the patient, they
had given her the widest possible berth, and had secluded them-
selves for their own safety in distant parts of the premises.  No
assistance, therefore, was at hand, while the patient was furious and
ready to brave all obstacles.  She was overtaken in a passage,
and another struggle ensued with her master, whom she would
have assaulted and overcome, had not his strength of muscle been
superior to hers.  We guarded her up stairs to her room, the win-
dow of which was carefully fastened, and two of her fellow-servants
were ultimately placed beside her, on the understanding that her
master and I would be at hand, and that additional assistance would
be forthwith supplied.  Afraid to leave her alone with her custodiers
until we could be thoroughly assured of their courage and vigilance,
we merely adjourned to an adjoining room.  But scarcely had we
done so when we were startled by sudden screams and a scuffle
in the room we had just left.  On re-entering we found the window-
shutters thrown back; the window itself open nearly to the top;
the patient on the floor; one attendant exhausted by terror, and the

other by her struggles to prevent the patient jumping over the window. It appeared that the two attendants had just sat down, anticipating no immediate danger, door and window being alike secured, when the patient, who was sitting on her bed in a corner of the room, rushed in a moment to the window, threw open all its fastenings, and was half out, when her dress was seized by one of her two companions, who by main force pulled her back on the floor. It now became evident that no part of the house was sufficiently secure for her incarceration ; none of its household trustworthy as custodiers ; while the risk of accident was imminent, and the necessity of incessant and careful watching apparent. Additional temporary assistance was obtained of a similar untrustworthy kind ; a hasty consultation was held, and various plans of treatment suggested themselves. All, however, were found, on consideration, impracticable or inexpedient save one, immediate removal to the adjoining Lunatic Asylum. The following were the chief proposals hastily discussed :—

1. Removal to her mother's house in town ; but it transpired that domestic unhappiness, connected especially with her relation to a sister, who resided with or kept house for her mother, was the immediate, as well as ultimate, cause of her illness. This, then, was the *last* arrangement that could be thought of, so far as the poor woman's welfare was concerned. Fortunately for the patient, her welfare was by her master considered before the convenience of his own family. Besides, in her mother's house there would have been even greater difficulty in procuring suitable attendants.

2. Removal to an isolated lodging in town. Here, again, the difficulty as to custodiers at once presented itself. It was evident that the girl's sister was utterly unfitted, by reason of nervous prostration, induced by intense excitement and alarm, to act as nurse or attendant ; and, as will be shown in due course, the dangers of intrusting so violent a patient to untrained, stranger-attendants are of such a kind as should not be lightly encountered.

3. Removal to the police-office. But the subsequent effect on the patient's mind of having been treated as a criminal, while suffering from disease, would almost inevitably have been prejudicial, while it was altogether repugnant to the feelings of a kind, indulgent master.

4. Removal to a public Lunatic Asylum, which happens to be close at hand. This appeared to be the only alternative left ; and, on being appealed to, I found it impossible to recommend any other procedure that would be equally safe for patient and community. The master of the house dreaded suicide, or assaults of a dangerous kind ; he declined the responsibility of having her watched and treated in his own house ; and he equally desired to avoid the possible results of sending her to town as suggested under proposals Nos. 1, 2, and 3, immediately preceding. He therefore felt himself constrained to contemplate sending her to the Lunatic Asylum, and measures were forthwith taken to have this procedure adopted.

In order to the admission of a patient into an asylum, however, certain legal forms are necessary,—implying not only trouble and expense, but what is of more consequence at such a crisis, *delay.* A petition must be presented to the sheriff; two medical men must certify to the insanity; the sheriff must grant an order for admission; and somebody must come under obligation to pay the patient's board, and to conform to the regulations of the institution as to clothing, removal on recovery, and so forth. [Unless under what is termed a "*Certificate of Emergency,*" which "authorizes the detention of a patient in an Asylum for a period not exceeding three days, without any order by the Sheriff." There are, however, so many and such serious technical difficulties in such a procedure, that I have never myself taken action under this certificate.] Hence arose a fresh set of difficulties; but, during our deliberations, the night was waxing on. The family physician and another medical man were at once sent for to town; and, meanwhile, an asylum schedule was procured, which the master of the house proceeded to fill up as law requires. He was immediately staggered by two points: 1*st,* That *he* must, *in his own person,* not only take all the responsibility of sending the patient to the asylum,—contrary to the wishes of her sister, who was consulted on the subject, —and the chances of recriminations from her friends, with, perhaps, an action of damages; but also, 2*dly,* Bear all the expense of her transmission thither and detention there. For neither of these steps did he find himself prepared, and there was a consequent pause in this line of procedure.

Meanwhile, the doctors from town had arrived; and meanwhile, also, the patient, finding all her efforts at escape for the time futile, had acted on my advice, and had laid herself down on bed. Her attendants, however, male as well as female, had, probably in alarm, left her *alone,* a proceeding that *might* have led to the most disastrous consequences, for precipitation from the window would probably have proved fatal. Being quiet, she was reported as being sound asleep. I considered it advisable, however, that we should be assured of her really being in the room—in bed—and asleep, as well as take measures for more careful watching for the future. Accordingly, one of the physicians who had just arrived, entered her room, forcing the door; he found her in bed, but awake, though comparatively composed. He gradually succeeded in engaging her in conversation, and in the presence of her sister alone, in eliciting some history of her illness. There was a good deal of hysterical excitement and weeping during the narration; but the unbosoming of her grief had an immediate and most salutary effect.

It appeared from the testimony of both sisters—the one corroborating the other—that some unhappiness had long existed between the patient and the sister, who keeps house for their mother, of such a nature as to prevent the former visiting her mother. She had not been in Perth for a long period, and, consequently, had had no oppor-

tunity of seeing her mother, but the matter had preyed on her mind for months while she was at service in the country. On the forenoon of the day of her illness, she had had an opportunity of visiting her mother, having been sent to town on a message by her mistress. She felt most acutely her inability to avail herself of her opportunity, and came home in a state of great hysterical excitement. This she contrived to subdue till she was preparing dinner, when she felt it impossible further to control herself, and at once gave the rein to her pent-up emotions and passions. Now—that is, three hours after the outbreak of the paroxysm—she perfectly remembered, with shame and confusion, her behaviour; expressed her contrition, and gave every evidence of returning restoration to physical and mental composure.

This being the case, and it being now nearly 10 P. M., it was agreed to try the experiment of allowing her to remain in her own bed and bedroom all night: several attendants, male and female, arranging to sit up with her or otherwise watch her, while a large dose of " Battley " was administered. Next morning I was informed she had not slept well, but was down at work as if nothing had happened, and was to all appearance in perfect health, mental and bodily. The family physician, when he called, was recommended not to see her, and the family also had the good sense and taste not to go near her, or otherwise show that anything amiss had occurred on the previous evening. From this period she has continued quietly at work, and has been uniformly reported to me, by master and servants alike, as in all respects " quite well."

II. *Commentary.*—Short and simple though this case may appear, it offers matter for serious reflection on questions bearing on the Treatment of the Insane in Private life. Seldom, if ever, has the the Asylum Physician the opportunity of seeing insanity in its earlier stages, during its first outburst, or of a transient character. Seldom, therefore, can he put himself exactly in the position frequently occupied by his brethren in private practice. The result is, I think, *want of due understanding of, and sympathy with, the frequent difficulties of the latter.*

Such a case as that above recited, illustrates—

I. *The extremely short duration, the paroxysmal or ephemeral character, of certain forms of insanity ; their sudden incursion and equally sudden disappearance ; the occasional composure and sanity of a patient represented to have been only a short time before in a state of " furiosity."* This attack began suddenly about 6, and was over long before 9, P. M. ; it could not, therefore, have lasted more than two or three hours. The *permanency* of the recovery is another question, upon which I do not at present enter, further than to remark, that in a patient so evidently predisposed to nervous derangement, a slight exciting cause may at any time produce another paroxysm of

mania; or melancholia may be gradually, quietly, and impercep-
tibly developed; or delusions may arise and become permanent—
the case ultimately assuming the characters of monomania, or of
delusional insanity. Several months, however, have now elapsed
since her attack, without, as is reported to me, any indication of
eccentricity or peculiarity, mental or bodily.

It occasionally happens that patients sent to asylums, with the
reputation of dangerous and unmanageable furiosity, are composed
and apparently sane from the moment of admission. In such cases
the asylum physician is apt to suppose some terrible exaggeration of
the symptoms by the friends or custodiers; some maltreatment;
some improper motive in consigning the patient to the asylum;
while, on the other hand, so sudden and so complete is the change
for the better, that the patient's friends are astounded and ashamed,
feeling themselves open to the accusations or suppositions above
referred to. Such sudden changes in a patient's behaviour or
character are due either to the natural cessation of the paroxysm,
or to the removal from sources of irritation; to the effect of new
scenes and associations; to the sense of being under discipline and
control—of being mastered. Had the patient, whose history has
been above given, been transferred to an asylum when it was pro-
posed, she would either have appeared quite well next day [and
doubt would have arisen in the minds of its officials, whether
she had ever been insane,—there being left no vestige of either
violence or delusion] :—in which case removal would have proved
unnecessary. Or, the result of finding herself suddenly torn from
what was to her home, and from friends, and under confinement in
a lunatic asylum, would have produced a relapse, or induced
permanent insanity : in which case the removal would have been
most prejudicial.

Again, patients are occasionally hurriedly consigned to asylums,
who, with moderate patience, might recover rapidly at home
where there is no real necessity for such a step,—a step which
is always to be avoided when there is a reasonable chance of
speedy restoration without it. [Only the other day I was re-
quested to make arrangements for the reception of a patient
represented by his physician as affected with violent Mania.
The patient did not present himself at the expected time; and I
subsequently found that narcotism by belladonna for a single night
had brought about immediate and satisfactory composure and
recovery. Cases of a similar kind are of no infrequent occurrence.]
Under all the circumstances of the case of A. B., it is impossible,
or at least it would be unjust, to blame her master, had he, after
her attempt to jump from the window, transferred her forthwith to
an asylum. The risk equally to herself and others, and the impos-
sibility of properly managing her where she was, would fully have
justified the step. Nor could any expert have predicted, with any
degree of certainty or assurance, that a delay of a few hours or a

night, or any particular course of treatment, would have led to or ensured composure and safety. The issue of the case proved, however, that it was fortunate *delay* had been a necessity, and unsurmountable obstacles occurred to the procedure contemplated.

II. *The extreme difficulty of properly naming and classifying all the phases or forms of mental aberration, and the unsatis actory character of all modern nosologies thereof.*

I have ventured to call the case of A. B. one of "temporary insanity." Had she succeeded in her attempt to leap from the window, she would probably have been killed; in which event, had a coroner's inquest or procurator-fiscal's examination followed, the verdict would assuredly have been, that the woman had committed suicide "in a fit of *temporary insanity.*" The main objection to such a term is, that it is too vague and comprehensive, embracing many *other* forms of mental disease besides that under considera-tion. If, however, a popular designation such as this has the disadvantage of *including* more than we bargain for, the species and genera of modern nosologies have the equal disadvantage of *exclud-ing* much that enters into the composition more or less of every case of mental aberration.[1]

[1] The case of A. B. might be denominated, according as we view it, or according to the prominence given to certain of its symptoms, —one of

  *a. Hysteromania ;* for it appeared in a certain sense to be an exag-geration of *hysteria ;* a paroxysm of mania terminating a course of *hysteria ;* a transition from the one into the other. But in certain respects it was something more, and in others something less than *hysteromania ;* or either *hysteria* or *mania ;* or any combination thereof.

  *b. Delusional Insanity ;* for she exhibited two distinct sets of delusions, viz. :

    1, That she was despised by her fellow-servants; and,
    2, That she had done nothing properly since her return to her master's service; that she was eminently an "unprofitable servant;" and that in particular her culinary duties on the day of her attack had been imperfectly performed.

  *c. Monomania ;* inasmuch as the intellectual aberration was visible in one or two channels only.

  *d. Suicidal Melancholia ;* for the attempt at precipitation might have been regarded as the result of a suicidal impulse or tendency; though it was more probably a reckless determination to escape from the house, by whatever means, with no specific view to immediate or ultimate self-destruction.

[1] On the subject of the various classifications or nosologies of insanity—old and new—the reader may refer with advantage to the chapter on *Classifi-cation*, in Bucknill and Tuke's "Manual of Psychological Medicine," which has now reached a second edition.

e. *Delirium tremens ;* some of the features whereof it may be said to have presented.

This single case, then, might, by various physicians following different systems of classification, be designated perhaps in a dozen different ways,—all in one sense equally correct, all in another equally incorrect ; all probably more open to objection than that quoted as my heading.

I know of no classification so generally and so practically useful as the old one of half-a-century ago, viz., that which divides all forms of mental aberration into,—

1. *Mania ;*                          3. *Melancholia ;*
2. *Monomania ;*                      4. *Dementia ;* and,
                5. *Idiocy (or Amentia) ;*

—as indicating their main symptoms, features, or phases : violent excitement, monoideaism, depression and despondency, silliness or fatuity, and congenital negation of intellectuality. That this experience or belief is general among practitioners in insanity is proved unmistakably by the fact that, almost without exception, Asylum Reports, in all parts of the world, adopt this classification, sometimes with a slight splitting up into genera or species, which is usually no improvement. At the same time *practical usefulness* and *scientific accuracy* are very different things ; and while universal testimony points to the fact that the *older* classifications are the more useful, it equally indicates that they are far from being rigidly accurate when applied to particular cases. If, however, the old classifications are to be regarded as failures, I look upon all recent attempts at nosologies of insanity as equally so ; while the latter have the additional disadvantage of being generally *unintelligible* to the mass of the profession, characterized frequently, as they are, by a copious new coinage of Greek terms, ingeniously anglicised, manufactured to indicate what are too frequently unstable symptoms of the most protean nature,—or features that do not deserve to have attributed to them the value of specific or generic, which ought to be permanent as well as thoroughly distinctive, characters. Modern nosologies err in the direction of too great complexity or minuteness, whereas no species of mental disease can be so rigorously defined as to *include all* the phases of a particular case from the origin to the disappearance of disease,— or all its symptoms at any one phase ; while it *excludes* the phases and symptoms of all other forms of insanity.

In truth, *there can be no classification of insanity absolutely correct or scientific,* inasmuch as the phases of abnormal mentalisation are as infinite, as varied and varying, as contradictory or capricious as the phases of human nature, of normal mentalisation, of emotional exhibition, of the play or display of the passions. Neither normal nor abnormal mind has been or can be accurately defined so as to *include* all the phenomena of the one, and *exclude* all those of the

other.    I believe a scientific definition to be impossible.    The principles of nomenclature and classification as applied to such sciences as botany and zoology are inapplicable, cannot be carried out, at least into the details of species and varieties, without sacrifice of truth; and while this is so, all attempts at such classifications are simply mischievous and absurd, leaving the subject more confused than they found it, rendering the Science (?) called "Medical Psychology" a bugbear to the student, a butt for satire and abuse by the lawyer, a subject of suspicion to the public.

III. *The frequent extreme difficulty of dealing—for the best—with sudden outbursts of violent insanity in private life.*

Patients like A. B., in the lower ranks of life in this country, and in all ranks in our colonies (*e.g.*, New Zealand and Australia), are sometimes, or always, committed to gaols or police-offices; and it is customary to condemn such a practice or proceeding as cruel and unnecessary.    Are we, however, prepared to recommend, or would we ourselves submit to, the risk of suicide occurring on our premises, or of some member of our household being seriously assaulted or murdered, rather than commit a dangerous lunatic,—under certain circumstances,—to the custody of the law?    I for one would certainly neither run such a risk myself, nor would I recommend any family to do so. ` Nor did I in the case of A. B.; though *now* that the issue of the experiment of retaining her in her master's house has proved favourable or fortunate, the recommendation of removal on the score, firstly, of safety, to a large household may appear to have been uncalled for.    A. B. may, however, again at any time become insane,—dangerously so,—from some slight exciting cause, or she may be insane now,—delusional or monomaniacal for instance,—though doing her duty quietly and in an apparent state of perfect mental composure or health.    Though, therefore, the *immediate* issue of the experiment of home treatment has been fortunate, the *ultimate* result may not be equally so.

Violent lunatics in all ranks of life are frequently treated at home by powerful attendants,—male or female,—strangers generally, selected on account of their physical qualifications alone. Usually these men or women belong to the lowest ranks of society; not necessarily vicious or depraved, but uneducated, unpolished, untrained as cutodiers of the insane, unacquainted with the phenomena of insanity and its accredited treatment.    Untutored human nature, moved by no higher influence than that of temporary and small *pay*, cannot be expected, when left to itself,—in the absence of the relatives or guardians of a patient, with no witness of their conduct save the latter,—to sustain "assault and battery," taunts and impertinences, all manner of vile usage, at the hands of a lunatic, with great forbearance.    It *will* and *must* resist and repel such treatment.    Hence, my too frequent experience has been that in one class of cases,—the violent,—*bruises and injuries,*

*even to the death, are the fruits of such supervision.*[1]  In a second
class, where the patient is frequently a melancholiac with suicidal
tendency, quiet, docile, but cunning and enduring, stealthily
watching the opportunity that may be offered by any carelessness
or relaxation of vigilance on the part of the attendant, to effect his
cherished purpose, the unaccustomed fatigue of *night*-watching,
superadded probably to arduous *day*-labours cannot be resisted;
exhausted nature *will* assert her requirements; sleep *will* steel over
the senses, even of the willing and faithful nurse; and the patient
is then left to his own line of action,—whether that may be in
the direction of suicide, escape, or otherwise.  In a third class
of cases, frequently those of anæmic, enfeebled, abstinent melan-
choliacs or monomaniacs, stimulants have been provided for the
sustenance of the flickering flame of life in the patient; or the
injudicious kindness of indulgent friends has placed spirituous
liquor at the command of the attendant, to enable him to sustain
the fatigues of night-watching.  The temptations of an unusual
position are too much for the low moral tone and strong animal
appetites of the attendant; and the result is, sometimes, what I
have myself seen : that a nurse has been found " dead drunk "
by the side of a dying or dead patient, whose wine and spirits she
had appropriated to her own use and bestiality !

    In truth, skilled or suitable attendants or custodiers are not to be
obtained *out* of an asylum, and frequently scarcely *in* one ; for the
emoluments are usually so small and the work so irksome, that
only the lowest classes of our population will accept such appoint-
ments on the terms offered.  Trained attendants on the insane for
use in private life, and especially in country districts, are a deside-
ratum.  The general practitioner in a midwifery case seeks and
readily obtains the assistance of a midwife or lying-in nurse, to
whom he can delegate a portion at least of his duties, and whom
he finds an efficient and indispensable assistant ; and, in London,
large bodies of nurses are being trained, systematically, for service
in private houses, as attendants in medical and surgical diseases of
every kind.  Are *mental* maladies, may I ask, of inferior interest
or consequence?  No one who has had equal experience of the
three great classes of the ills to which our flesh is heir,—medical,
surgical, and mental,—will class the last as in any respect inferior
to the others.  On the contrary, there will probably be no difference
of opinion in regarding the latter class of cases as more imperatively
demanding proper attendance than the two former, in so far as the
interests of a far larger circle are involved : that of whole house-
holds, families, clans, or villages, instead of individuals.  I have
no wish to depreciate the claims of medical and surgical cases; I
think it of infinite importance for *them*, as well as for *mental* affec-

    [1] Illustrative cases are given in the published Annual Medical Reports of
the Murray Royal Institution for the Insane, Perth; *e.g.*, 32d (for 1859), p. 10 ;
37th (for 1864), p. 13.

tions, in private or home life, to be attended by thoroughly competent and trustworthy persons; and, until this can be done we must submit to endless accidents and injuries,—the results mainly of ignorance, and the impossibility of securing proper treatment. I have elsewhere[1] repeatedly called public attention to this subject, —*the training of attendants on the insane for service in private homes;* and I continue to hope that some philanthropist will do for the insane what Miss Nightingale and her disciples are doing for the sufferers from ordinary medical and surgical disease. We are in the habit, too much perhaps, of depreciating the customs of our continental neighbours; but I have often had cause fervently to wish we had in this country *any* corporation equivalent to the " Sœurs de Charité " of France, or the " Deaconesses " of the Rhine,—devotees to duty,—to conviction;—whose trustworthiness cannot be for a moment doubted, whose qualifications for their self-imposed duties are of the best kind, and whose labours possess the highest possible value.

Patients are occasionally presented at asylums with their arms fastened by the " camisole " (*vulgo,* " strait-waistcoat"), or hands and feet alike tied together by ropes. We, Asylum Physicians, are in the habit of cutting these bonds in our entrance-halls; and with an air of superior virtue and intelligence, rebuking the imposers of such unseemly restraint: announcing that, within the asylum walls, there is no such thing known or used as " mechanical restraint;" " moral suasion " and the " law of kindness " being the motive power of all its machinery. Now, it by no means follows that, because in an Hospital specially constructed for the insane, with its padded rooms, full staff, trained officers, and every appliance for the treatment of every symptom, mechanical restraint can be dispensed with,—it can be equally so in private life and in the absence of all these advantages. Nor do I admit that it can be judiciously or properly dispensed with, at all times, and under all circumstances, even in the best regulated asylums.

The truth is, the whole system of treatment of the insane, whether in asylums or out of them, is one essentially of *restraint:* find a more euphonious or less objectionable term if you *must* or *can;* but the thing is *restraint* and constraint. Under different circumstances this restraint may be *moral* or *physical;* and the latter, again, may be *mechanical* or *personal. Mechanical restraint* is, however, *at present* unfashionable. The public has a vague and ignorant horror at everything in the form of camisole, polka, muff, or fastening for hands or feet, of any kind. The mere name " restraint" suffices to conjure up to the fertile imagination, visions of dark, slimy, subterranean dungeons; clanking chains and fetters; straw pallets; filthy nakedness; glaring eyeballs; and the shrieks of demoniacs.

[1] Annual Medical Reports of the Murray Royal Institution; 30th (for 1857), p. 35; 36th (for 1863), p. 24. " Excelsior,"—the Literary Gazette of the said Institution: Nos. 19 and 20, 1864, p. 7.

But *physical restraint is, nevertheless, a necessity under certain circumstances ;* and hence *personal* restraint is employed ; that is, in order to prevent destruction of life or property, the patient is dogged or held by one or more physically powerful attendants.   The fruits of this practice, the present customary substitute for *mechanical* restraint, include the following :—Perpetual struggles for the mastery ; struggles which generate animosities of the most permanent and dangerous kind ; struggles, moreover, which lead directly to broken ribs, pierced lungs, ruptured viscera, and to death itself, resulting from these injuries ; besides minor mishaps, such as black eyes and bruises of every degree.   If the result of the struggle is unfavourable to the *patient,* a loud outcry is immediately raised of malpraxis or cruel usage, and all parties concerned in his treatment or custody get into bad odour from an accident, which, *under the present system of personal restraint,* it was impossible to prevent.   But if the unfortunate *attendant* is injured, if the matter is noticed at all, it is with a tone of simple and superficial regret that he should have suffered in the discharge of his duty ; generally he receives neither sympathy nor recompense.   There is also a constant recurrence of sudden and furious assaults, or homicidal attempts, which, unfortunately, are occasionally fatal.   I am assuming that the struggles, injuries, assaults, and deaths referred to, are *unavoidable under the system of personal restraint* or supervision.   That deliberate and unnecessary violence is occasionally committed on a turbulent patient by a bad attendant cannot be denied ; but such outrages are exceptional, and may well be omitted from a catalogue of the fruits of personal restraint,—restraint by *persons* instead of by *things; while insane for* sufficiently numerous and important without such an addition.

In my own practice, of eleven years' duration, among the insane, *I have never myself used mechanical restraint,* save in trivial forms, in a few surgical cases, where, I may add, even the sticklers for absolute non-restraint so-called are forced to permit its use.   I have been anxious to give the fullest trial to every possible substitute for mechanical restraint.   But looking back, I am bound to confess, that I have seen cases, which probably would have been benefited, lives that might have been saved, by its application in some form. The same belief is the result of what I have seen of the insane and their treatment in very different parts of the world, between Iceland in the northern, and New Zealand in the southern, seas, including England and Ireland.   I have, moreover, frequently discussed this subject with my brethren at the head of other asylums in this country ; and so far as I remember, their *experience has been almost uniformly in favour of some form of mechanical restraint in certain exceptional cases or conditions.*   Their *sympathies* and their *practice* may be somewhat at variance occasionally with their *experience.* There may be a strong inclination to avoid the very semblance of harshness, by the total disuse of any form of fastening of hands or feet, and there may be a difference of opinion as to the best forms

of such fastening where required; but general *experience* sets it down that in every asylum, in every community of the insane, there do and must occur *exceptional conditions, wherein some form of mechanical restraint is, for the time, not only the most, but the sole, efficient and humane mode of treatment.*

From what I have said it will appear, then, that " *absolute non-restraint*" is *absolute nonsense.* Restraint *is* occasionally necessary in the proper treatment of the insane, and the only question that remains is, what form is the most efficient in the interests of medicine and humanity, for the welfare equally of the patient and society?

In 1853, the Commissioners in Lunacy for England invited the opinion of all the Asylum practitioners of that country on this subject, —of the use or non-use of mechanical restraint. The result was that, of those who gave a decided opinion, about 70 per cent., or nearly three-fourths advocated its *qualified* use, that is, its use in certain exceptional cases or conditions; while nearly 30 per cent., or upwards of a fourth recommended its *total abolition.* The first category, that of the great majority, included the most distinguished Alienists in England, such, for instance, as Commissioners Wilkes and Bucknill; Drs Forbes Winslow; Sutherland and Monro of St Luke's; Fox of Bridlington; Newington of Ticehurst; Thurnam of Wilts; and Samuel Hill of the West Riding of Yorkshire, Asylums.[1]

Public opinion and professional practice are at present, in regard to mechanical restraint of the insane, alike in a state of reaction. Disgusted with the revelations of Old Bedlam and the Bicêtre, with the chains and fetters, which are said to have constituted the *all-restraint* of these days, we have resiled to the opposite *extreme* of *no-restraint.* Now both extremes—as extremes generally are—are mischievous, though the latter is probably not so vicious as the former. Rational practice and opinion lie *in medias res;* and the *permanent* belief, both of the public and the medical profession, I doubt not, will in course of time be, that *under certain exceptional circumstances, some form of mechanical restraint is equally necessary and humane.* It is impossible to define what these circumstances are, or foretell what they may be. Every case should be judged and treated on its own merits, and not prescribed for apart from the circumstances of its occurrence, according to the "wise saws" of text-books; which are sometimes compilations by persons, who have never themselves experienced the difficulties, which they dismiss so summarily, treat so deftly, with no consciousness of the consequences involved. The revulsions, antagonisms, and extremes of opinion and action in regard to the treatment of the insane have their parallels in the history of prison discipline during the last

---

[1] The results of the inquiry will be found given at length in the Eighth Report (for 1854) of the said Commissioners, Appendix *G:* as well as in the " Journal of Psychological Medicine," vol. vii. p. 543, 1854. In the latter the reader will find criticized the professions *versus* the practice of the Total Abolitionists.

century or half-century, or even within the last twenty years. At one time, Asylums, in common with Prisons, were the foulest blot in our civilisation. *Now*, probably too much is made of them by philanthropists and doctrinaires, who teach one-sided, and consequently unfair, views of things. A latitude and liberty are permitted, indulgences granted, which undoubtedly lead frequently to the most grievous consequences to the wellbeing of society; and this conviction, which is gradually growing in the public mind, may reach such dimensions that it may carry us back to a state of matters equally to be deplored. The tendency of our present laws and procedure in regard alike to the criminal and insane classes is to exhibit undue leniency and indulgence; to protect what is supposed to be their rights at the expense too greatly of those of the sane and well-behaved; to show *more* than justice to the one, *less* than justice to the other,—an equivocal, absurd, and most mischievous state of affairs. Laws unnecessarily minute, public opinion unnecessarily indulgent, have much to do with the present frequency of Murder and Suicide; the liberty of one class of Her Majesty's subjects may be protected too far, and that of others—the general public—too little.

III. *Resumé.*—The foregoing paper is intended to be *suggestive*, rather than descriptive. The main points mooted therein, and to which I would earnestly invite the attention of the medical profession, and through it of the general public, are these:—

1. The necessity for training a body of Attendants on the insane for service in private life.

2. The consideration of the best means of Mechanical Restr. at of the insane in cases where this is required.

3. The responsibilities, helplessness, and difficulties of masters of insane servants and guardians of insane patients under certain exceptional circumstances.

4. The responsibilities and difficulties of Physicians in the same classes of cases.

5. The medico-legal and other relations of sudden, violent, ephemeral insanity.

6. The non-necessity for, or impropriety of, sending *all* insane patients to Lunatic Asylums.

7. The distinctions that ought to be drawn between the treatment of the insane in well-appointed Hospitals and in private life.

8. The effects of a false philanthropy in multiplying and aggravating the evils resulting from insanity.

9. The dangers arising from revulsions in public opinion and practice, as illustrated by prison and asylum discipline.

10. The necessity for re-adjusting the equipoise in the relative treatment by the law and by public opinion of the insane and sane in certain circumstances.

ARTICLE X.—*Excision of One Lateral Half of the Tongue.* By GEORGE BUCHANAN, A.M., M.D., Surgeon to the Glasgow Royal Infirmary; Lecturer on Anatomy, Anderson's University.

THE bold and ingenious proposal of Mr Syme, to divide the lower-jaw at the symphysis in order to afford free access to the mouth, has provided the surgeon with the only plan by which he can freely and certainly eradicate every point of diseased tissue, in operating for removal of malignant disease of the tongue. The first three operations were fatal, but the fourth was successful, probably on account of the genio-hyoid muscles having been left entire; thus enabling the hyoid and epiglottic apparatus to retain many of its movements. I presume, the following case was successful on the same principle, and is to be reckoned another instance of recovery by this mode of operating,—for though the removal of one half is not so formidable an undertaking as that of the whole, still, when one lateral half of the tongue is invaded by epithelial cancer, the difficulty of eradicating every suspicious part is just as great as when the whole organ is involved.

Mrs M'Millan, æt. 50, was admitted into the Infirmary on the 26th May 1865. She stated, that about three months before, she had observed a small pimple on the right side of the tongue. This soon ulcerated, and the sore continued to spread, till it is now the size of a shilling. It is the seat of continual pain, which shoots to the back of her head. The ulcer is deep and excavated, with hard everted thickened edges, and a foul greyish bottom. The tissue around is hard, but the hardness does not seem to involve the floor of the mouth.

The teeth opposite the ulcer were extracted, and soothing and astringent washes were used without any effect, except that of cleaning the mouth and making the breath less offensive. As the disease was confined to one side of the tongue, and apparently circumscribed, I resolved to cut out the epithelial ulcer, and a portion of sound tissue beyond it.

On the 18th of June, having placed her under the influence of chloroform, I pulled out the tongue with a piece of strong thread passed through the tip, and cut out a semi-circular piece of the tongue, cutting wide of the hardened base. There was little bleeding, the wound rapidly healed, and she went home in a few days much relieved.

On the 10th of August, she came back with the tongue much worse than before the operation just described. The disease reappeared in the old place, soon after she went home, and spread so rapidly that it has now involved nearly the entire right side of the tongue and the sublingual gland of the same side. It seemed plain that nothing short of a very serious operation could remove the disease; but the patient was suffering so much pain and uneasi-

ness, that she begged to be relieved of it by any operation which afforded a chance of success.

On careful examination I satisfied myself that the disease was confined to one-half of the organ, and as the removal of this would leave the other side and its connexions with the hyoid bone and epiglottis entire, I resolved to extirpate this side only. In this my colleagues coincided with me.

On the 12th of August, the operation was performed in the following manner:—

The patient was put deeply under the influence of chloroform, and her tongue was secured by two cords passed one through each side of the tip. An incision was now made in the mesial line, through the lower-lip, and continued down to the hyoid bone. The jaw was sawn through at the symphysis, and powerfully separated, so as freely to expose the interior of the mouth. I now made an incision in the gum, close to the teeth, and dissected the whole of the soft-tissues from the interior of the right half of the lower-jaw. In doing so a branch of the facial artery was wounded, but did not give much trouble. The sublingual gland and mucous membrane, as well as the attachments of the tongue, were now carefully raised from the floor of the mouth, and separated from the hyoid bone. I now pulled out the tongue by the two cords, and made a vertical section of it from the tip to the epiglottis, keeping a little to the left of the middle line. A single stroke of the knife divided its attachment to the epiglottis and anterior palatine arch. The lingual artery at once appeared in the bottom of the wound, and was secured and ligatured without difficulty. No other vessel of importance required ligature, and little blood was lost. Before the incision in the tongue was made, the patient had partially got out of the influence of chloroform; but this was rather in my favour, as she could assist by swallowing or putting out the blood, as occasion required: and subsequently she told me, that though she could do so, she suffered little pain. The two cut surfaces of the jaw were united by putting a silver-wire over two teeth on opposite sides, and twisting the ends; and another was put through a hole bored on each side of the symphysis, so that all motion was prevented. The incision in the lip and chin was brought together with silver sutures, and a piece of drainage tube made to pass through the mouth and out at the lower part of the incision below the chin. This I consider a very essential precaution to carry off the pus and saliva during the granulation of the wound in the mouth.

The progress towards recovery was very rapid. The incision healed by first intention, and the stitches were removed on the seventh day. The greatest attention was paid to washing away all pus and secretion from the mouth. The drainage tube was daily renewed, and the patient was directed to wash and gargle her throat frequently with a solution of chlorate of potash, and to spit

out instead of swallowing the fluids she felt in her mouth.   I was especially careful on this point, because, I am sure, I have seen death ensue from swallowing pus, in a case of excision of part of the lower-jaw for fibrous tumour.   The patient did well for a fortnight, and the typhoid symptoms which came on could be accounted for in no other way.

In the present case the patient was very careful and followed my instructions to the letter, and there was not a bad symptom during the whole progress to cure.   Some slight swelling about the throat and angles of the jaw occurred on the third and fourth days; but it soon subsided, and gave her no more annoyance.

On the 16th of September, cicatrization was complete, and the two sides of the jaw were completely united by osseous deposit.   On slightly pulling the silver-wire which had been passed through the holes on each side of the symphysis, it came away, having ulcerated through the bone.   For some days previous to this, I had desired the patient to shake the wire so as to loosen it, and when I pulled it it came away without difficulty.   Ten days previously, being satisfied that little or no pus was formed in the mouth, I removed the drainage tube, and the hole below the chin soon closed.

On the 20th of September, before she was dismissed, I made a careful examination of the state of the parts, and requested my colleague, Mr Lister, to do so also.   The result was most satisfactory. As far as the operation was concerned it was a perfect success.   Every point of the incision was completely cicatrized.   The jaw was united. The parts in the mouth felt soft and yielding, and there seemed no trace of the disease left.   On looking into the mouth the tongue occupied the middle line, and it was only on careful inspection that one could detect which half had been removed.   She could swallow very well, and articulation, though somewhat defective, was sufficiently distinct to enable her to make herself understood; in fact, was much better than when she returned with the extension of the disease.   She was free from pain, which she had not been for months, so that she was able to sleep soundly, and this had had such an effect on her general health that she was stouter, fresher, and altogether better than for a long time previously.   The only thing that annoyed her was a copious salivation, which seemed to fill her mouth frequently with fluid, and necessitated the frequent use of a handkerchief.

I saw her at her own home, at Arran, a fortnight after she left the hospital, and I found her still more improved.   Articulation better; salivation much less; appetite good; had good rest at night, and was in every way very comfortable.

# Part Second.

## REVIEWS.

*Winter in the South of Europe: or Mentone, The Riviera, Corsica, Sicily, and Biarritz, as Winter Climates.* By J. HENRY BENNET, M.D., etc., etc. Third Edition. London: Churchills: 1865.

*San Remo as a Winter Residence.* By W. B. ASPINALL. London: Churchills: 1865.

*The Climate of San Remo, as adapted to Invalids.* By HENRY DAUBENY, M.D. London: Longmans: 1865.

*The Climate of Malaga in the Treatment of Chronic Pulmonary Disease.* By THOMAS MORE MADDEN, M.D., M.R.I.A. Dublin: Falconer: 1865.

*Nice and its Climate; with Appended Remarks on the Chief Causes of Pulmonary Consumption and on the Action of Climate in that Disease.* By EDWIN LEE, M.D. Second Edition. London: Adams: 1865.

*Health Resorts of the South of France.* By EDWIN LEE, M.D. Second Edition. London: Adams: 1865.

*The Baths of Nassau.* By EDWIN LEE, M.D. Fourth Edition. London: Churchills: 1865.

*The Principal Baths of Switzerland and Savoy, with Notices of some Swiss Mountain Resorts, and Remarks on Mountain Air.* By EWDIN LEE, M.D. London: Churchills: 1865.

*Lectures on the German Mineral Waters, and on their Rational Employment.* By SIGISMUND SUTRO, M.D.. Senior Physician to the German Hospital, etc. Second Edition. London: Longmans: 1865.

THE value of change of climate in the treatment of disease is more fully realized than at any former period, and the cases in which it is likely to prove advantageous are now much better understood. No one, in this country at least, would think of hurrying off a patient in the last stage of consumption to Madeira or to Egypt, depriving him of the comforts of home and the society of his friends, on the vague chance of amelioration: a change to do good must be made early; delayed too long, it is only likely to accelerate the fatal result. Accordingly, it is essential that every physician should have a certain knowledge of the peculiarities of the various health resorts, and the number of works on the subject, which are constantly appearing, leaves him no excuse for remaining uninformed.

The first work on our list, by Dr Henry Bennet, is entitled "Winter in the South of Europe," and has reached the third

edition. It has, however, undergone important changes since its original publication. At first, it was a mere essay on Mentone and its neighbourhood; it now contains a full account of the meteorology and winter climate of the north shores of the Mediterranean, and embraces an account of various other health resorts, such as Central Italy, Corsica, Sicily, the Italian lakes, and Biarritz, the favourite summer quarters of the Empress of the French. Mentone still remains Dr Bennet's favourite; to it the largest amount of space is devoted, and to it our remarks must principally be directed.

The mildness of the climate of the Riviera has long been proverbial. This it owes, not so much to its latitude, as to the protection afforded by the Alps and Apennines to the north-east. The Riviera is divided into two parts, the eastern and the western, the point of separation being the town of Genoa. On the eastern Riviera are Nervi, Chiavari, Sestri, Spezzia, and Massa Carrara. Of these places it may be said that, in general, they are not so sheltered as the most favoured spots in the western Riviera, and that the accommodation for invalids is inferior. Nice was, till within the last few years, almost the only part of the western Riviera which had more than a local reputation. It has now, however, several rivals; and although none, as yet, equals it in the social advantages it presents, there are more places than one which are superior to it as to climate, and are better adapted for certain classes of invalids. Mentone, in latitude 43° 45′, about twenty-three miles east of Nice, was founded as an English colony by the Rev. Mr Morgan, an English clergyman, some eight or nine years ago, whilst last winter its foreign population amounted to about 600. Its situation, even for the Riviera, is exceptionally favourable. The higher mountains, between 3000 and 4000 feet high, "receding round a beautiful bay that opens to the south-east, form a magnificent mountain amphitheatre, the centre of which is about two miles from the sea. This is the Mentone amphitheatre." The space between the sea and the mountains is occupied by a series of hills, from 500 to 1500 feet high, covered with olive-trees, and sloping gradually down to the sea. Twenty or thirty miles behind the mountains which surround the Mentone amphitheatre, rises the main chain of the Maritime Alps, which attain a height of from 5000 to 9000 feet. "Thus," says Dr Bennet, "the Mentone amphitheatre, being only open to the south, south-east, and south-west, the mistral, as a north-west wind, is not at all felt, and but slightly as a deflected south-west wind. All the northerly winds pass over the higher mountains and fall into the sea,—at some distance,—several miles from the shore. When they are high, there is a calm in the bay at Mentone, and the sea is also calm, whilst at some distance from the shore it may be crested and furious."

Dr Bennet having described the peculiarities in the situation of Mentone, goes on to consider its "Climate as shown by vegetation." From its exceedingly sheltered situation, and the consequent

mildness of its winter climate, vegetation has a more southerly
character than is to be found in any part of the northern or central
regions of Italy.    Indeed, we must go to Palermo, six degrees
farther south, before lemon-trees are to be found, as at Mentone,
growing in the open air without the shelter of walls.    The existence
of lemon and orange trees of considerable size is in itself proof of a
very mild climate.    The fruit of the orange-tree will bear 7° F. of
frost without injury ;  the trees themselves being only killed by 11°
of frost.    The lemon is more delicate : 5° injuring the fruit ;  8° or
9° killing the trees.    The existence of large groves of lemon and
orange trees is thus decisive proof of the absence of severe frost.
The presence of various other trees and plants is brought forward, by
Dr Bennet, in additional illustration of the mildness of the climate.

In the next chapter Dr Bennet treats of the geology of the neigh-
bourhood of Mentone, and in the following of its physical geography
and meteorology.    Dr Bennet having given various data with regard
to the dryness of the air, the amount of rain-fall, the number of rainy
and of sunny days, thus concludes :—

"From what precedes, it will be perceived that the characteristics of the
climate of Mentone, and of the Riviera in general, as evidenced during the six
winters I have spent there, are : the absence of frost ; the prevalence of
northerly winds ; the complete absence of fogs : the paucity of rainy days ; the
clearness of the sky ; the general heat and brilliancy of the sun : a rather cool
or chilly night temperature : and a bracing coolness of the atmosphere through-
out the winter, out of the sun's rays.    Even when the sun is obscured by clouds
and rain-falls, as the wind is then generally from the south-west or the south-
east, it is not cold at any period of the winter.    On the rare occasions, how-
ever, when it rains, with the wind from a northern quarter, there may be as
miserable and chilly a state of things as in a drizzling November day in Eng-
land.    As rain only falls on a small number of days, and then often not during
the whole day, and as the other days are uniformly bright, clear, and sunshiny,
for five days out of six, throughout the winter, exercise in the open air can
be prudently taken, from nine until three, four, or five P.M., according to
the season, with both pleasure and benefit.

"Notwithstanding the complete protection from the north, north-east, and
north-west, the wind is often rather high near the shore.    Even when in the
northern quarters, it often seems to come from the south-east or south-west,
the open region, no doubt owing to the land-locked character of the district.
However strong the northern winds may be, the mountain valleys and the
more internal hills are quite sheltered and protected.    The smaller or eastern
bay is decidedly better protected from the north winds, and is several degrees
warmer than the western, owing to a spur from the Berceau mountain rising
immediately behind the houses which line the shore.    There certainly is, how-
ever, no atmospheric stagnation at Mentone, as some writers have asserted."—
P. 80.

"A cool but sunny atmosphere, so dry that a fog is never seen at any period
of the winter, whatever the weather, either on sea or on land, must be bracing,
invigorating, stimulating.    Such, indeed, are the leading characteristics of the
climate of this region—the undercliff of western Europe.

"Behind the mountains which skirt the Riviera and the Mentonian amphi-
theatre, in midwinter, as we have seen, frost and snow extend up to the north
pole, more than a thousand miles.    On the other hand, the wind blows from
the northern quarters during the greater part of the winter season.    The air
must, therefore, be cool, and would be cold, were it not warmed by an ardent

sun, darting its rays through a cloudless sky and a dry atmosphere—were it not, also, for the summer heat stored up in the rocks and mountains, and given out by them. These causes keep Mentone free from frost when it reigns all around, but cannot make it a tropical climate."—P. 124.

The fifth chapter treats of the natural history of the Mediterranean, giving much interesting information with regard to its tides, colour, the fishes found in it, and the birds which frequent its shores. The sixth chapter, which contains Dr Bennet's observations on " the climate of Mentone considered medically," is specially interesting both to the physician and to the general reader. From what has already been stated regarding the dryness and comparative coolness of the air, and the large amount of sunshine, it will be evident that the climate of Mentone must be of an essentially bracing, and somewhat stimulating character. Rainy days being unfrequent, and the soil drying very rapidly, exercise in the open air can be enjoyed nearly every day. The functions of the skin are stimulated by the mildness of the climate and the dryness of the atmosphere; and, hence, the lungs are relieved of that extra burden which is thrown upon them in cold and damp climates. There is, consequently, far less risk of colds, bronchitis, or sore throat, than there is in our northern regions. The special disease of which Dr Bennet first speaks, in reference to the climate of Mentone, is phthisis. The following sentence expresses his general opinion on this subject :—" Phthisis is essentially a disease of debility. It principally attacks those who have received organizations deficient in vitality from their parents, or who have injured the vitality of an originally good constitution by excesses of any kind, or in whom such a constitution has been impaired by over-work, or by hardships and privations, independent of their own will. In such a disease,—one essentially of defective vitality, —a bracing, stimulating climate such as I have described, must be beneficial, and has been most decidedly so, both in my own case and in those of the many whom I have attended."

Accordingly, Dr Bennet has found phthisis at Mentone a far more tractable disease than in London or Paris. Of course, the good effects are not due to the mere increase of temperature; the dry, clear atmosphere, the absence of fogs, and the exhilarating sunshine, permit delicate patients to be in the open air for some hours daily; appetite and digestion are consequently improved; sleep is rendered sounder and more refreshing; while the beauties of the external world exercise a soothing influence upon the mind. But while patients who are in the earlier, or even in the secondary, stages of phthisis, have done well, even the climate of Mentone is rarely capable of arresting the progress of the disease in its later stages. Not being able to avail themselves of the advantages of out-door life, such patients feel changes of temperature almost as much as they would do at home, particularly as they have not the same means of guarding against them. The same remarks apply,

but with even greater force, to those who are suffering from the acute form of the disease. But even in cases the most likely to be benefited by the climate of Mentone, it must be borne in mind that mere change of climate is not enough. Judicious medical treatment is as necessary as at home, and care must be taken to avoid certain sources of danger met with in the warmer climate. In the words of Dr Bennet : " Patients left to themselves, or to rules laid down for their guidance at home, commit all kinds of errors. They constantly omit to do what they ought to do, and carried away by the example of others, or by the first dawn of improvement, do much that they ought not do do."

All this is very true, but Dr Bennet would have done well to point out that all forms of phthisis are not likely to be equally benefited by a residence at Mentone. Cases of phthisis are very different, and a climate which may be beneficial to one, may, nay, must be detrimental to others. A dry stimulating climate is likely to do good in cases where the patient is of a leucophlegmatic temperament, where he suffers from atonic. dyspepsia, and where there is copious bronchial secretion. On the contrary, it is almost certain to be injurious when the patient is of an inflammatory temperament, when there is a dry condition of the skin and bronchial mucous membrane, and where there is a tendency ,o hæmoptysis. In the latter class of cases a moister and more relaxing climate will be found beneficial. Much the same remarks apply to the effect of the climate of Mentone on bronchitis, Dr Bennet failing to distinguish the cases which are likely to be benefited by it. Of other special diseases it is scarcely necessary to speak ; we content ourselves with one quotation :—" The Riviera climate is equally propitious to those suffering from disease of the kidney, congestion, albuminuria, gravel. The dryness and mildness of the atmosphere, by promoting cutaneous transpiration, relieve the kidneys as well as the lungs,— for in our climate the kidneys have also extra work to do in winter. Moreover, the power of living in the open air, and the improvement which follows in the general health, is of as great importance as in chest affections. I have met with several remarkable cases of improvement and cure."

The next chapter, entitled " Mentone in its Social Aspect," while invaluable to those who propose to take up their residence on the spot, will be found interesting even by those who have no prospect of going from home. It describes the general tenor of life there,—the walks, drives, and donkey rides ; the commissariat and the amusements. The latter are not very numerous, for Mentone is not like Nice, a small southern capital, with its opera, theatre, and numerous balls ; it is a pure resort for the invalid, which has as yet " been chosen as a residence by none of the tribe of health loungers."

The heading of the eighth chapter is " A Tour in Search of a Better Climate." Its origin is thus described :—" Although pleased with my first winter at Mentone, I was anxious, on leaving England

the following autumn (1860), to find a still better climate, and, like
most invalids, thought I might as well see a little of the world, and
thus combine pleasure and profit." The results of this journey may
be briefly stated. Genoa is incompletely protected from the north-
east wind ; it is densely populated, badly drained, and unhygieni-
cally built. All the principal hotels are on the port, exposed to the
emanations of the drains which open into it. On the Eastern
Riviera, as already mentioned, there is no locality more desirable
than Mentone, while the accommodation is decidedly inferior. Pisa
is situated in an open plain, some miles from the mountains which
protect it ; the town is surrounded by a high wall which impedes
ventilation ; the streets are narrow, sunless, damp, and cold ; the
quarter of the invalids is a quay on the bend of the sluggish Arno,
about a mile long, on which they must march up and down ; the
surrounding country is a dull plain ; the town, with the exception
of the cathedral, the leaning tower, and the Campo Santo, presents
no objects of interest. Above all, Pisa is in itself unhealthy, the
average duration of life being only twenty-nine years, as against
forty-four in London. Florence is comparatively a mountain town,
and, as a winter residence, too cold for invalids. Rome is subject
to malaria, more or less, all the year round : when the north wind
blows it is very cold. Dirt and defective drainage still farther
detract from the amenities and the healthiness of the former mistress
of the world. Naples " exhibits the concentration of all the un-
hygienic conditions previously alluded to." The public drains
open themselves in the sands in front of the Villa Reale, the most
fashionable promenade. The town is surrounded by pestilential
marshes, and the rock on which it is built is so porous that the rain
soaks in twenty feet, and in dry weather gives out its moisture by
degrees. Dr Bennet thus describes his latest experience of Naples :—
" A few days after my arrival in November, the autumn rains com-
menced with a warm oppressive sirocco, or south-east wind. The
torrents of rain that fell in the first twelve hours washed the streets
and drains of their accumulated abominations into the sea. The
waves and the surf, on the other hand, drove them back again and
again on the shore, whilst the wind, rushing up the open drains,
escaped through the rain openings in the streets, and through the
open closets in the houses. The smell throughout the entire lower
part of the city was awful, and a considerable portion of the popula-
tion was at once affected with abdominal pains, diarrhœa, and even
dysentery. I was one of the first victims, and after nearly three
weeks' illness, I abandoned all idea of exploring Salerno and the
south of Italy. I had only one wish, that of returning as quickly
as possible to pure, healthy Mentone. I therefore embarked on a
Genoa steamer as soon as the barometer showed me that it was
prudent so to do, and reached Mentone safely in a few days. There
I remained during the rest of the winter." Though Dr Bennet's
opinions are perhaps expressed a little too strongly, there can be no

doubt that the large Italian towns are in general very deficient in hygienic arrangements, and that many of the deaths among our countrymen, which occur every year, are really due to typhus or typhoid fevers, the result of dirt and overcrowding, or defective drainage.

The next chapter describes a visit to Corsica, and in Ajaccio Dr Bennet found a winter climate equal, or nearly equal to Mentone. It is one of the most lovely spots in Europe, clean, and uncramped by walls. Its bay, though smaller than that of Naples, is as blue and beautiful. The vegetation indicates a climate at least as warm as that of Nice, or even a shade warmer. Ajaccio is also characterized by an absence of those strong winds which reign during winter in other parts of the Mediterranean. As yet, however, there is but little accommodation for visiters; were this supplied, Ajaccio might become a formidable rival to the Riviera.

In the tenth chapter there is an account of a visit to Sicily. The following is Dr Bennet's opinion of Palermo as a winter climate :— "Such a winter climate—temperate, sunny, and rather moist—may be beneficial to a certain class of patients,—to highly nervous, excitable, impressionable constitutions, that are too much braced and stimulated by the dry tonic air of the Riviera, and with whom the bracing, stimulating atmosphere of Cannes, Nice, Mentone, and St Remo does not agree. At the same time, I do not think it possibly can be as beneficial to those who require invigorating and vitalizing, to those who are suffering, like the phthisical, from defective nutrition and lowered vitality. In the earlier and curable stages of phthisis I am persuaded that the dry, invigorating climate of the Riviera is far preferable in the majority of cases." Yet Sicily is in at least one respect superior to the Riviera, or to any other place we know of. "Oranges are numerous and first-rate, sweet and juicy. I may here mention that throughout Sicily it is the custom to eat strawberries along with sugar and the juice of an orange or two. The strawberries, a small kind, come to table without their stalks, are crushed with white pounded sugar, and the juice of an orange is squeezed over them. The result is a most fragrant and agreeable compound, much superior, in my opinion, to strawberries and cream. Indeed, I think it is all but worth while to make a journey to Sicily to be initiated into this mode of eating strawberries."

The next chapter contains a short account of Biarritz. It is best known as a summer and autumn residence, but may, from the mildness of its climate, be advantageously resorted to in winter. One advantage it possesses as a winter residence is that, from having two seasons, it is much less expensive during winter than Nice or Mentone, which are completely deserted during summer. Though not so sheltered as the Riviera, and exposed occasionally to pretty smart frosts, its mild, sunshiny, and dry climate must be useful in much the same class of cases. The remainder of Dr Bennet's work is occupied by a short account of the Italian lakes,

some meteorological tables, and advice to invalids regarding the journey to and from Mentone. On these subjects, however, our limits do not permit us to enter, and we must take leave of Dr Bennet with a cordial recommendation of his work. The volume is very tastefully got up; it is illustrated by several well executed chromo-lithograph maps; while the interesting nature of its contents, the mingling of science with general information, render it equally suitable for the general reader or the physician.

A very brief notice of the works next in our list must suffice. San Remo has of late years acquired some celebrity as a health resort. It is about fifteen miles from Mentone, and is within the Italian territory. Situated in a deep bay, facing the south, and protected by the mountains behind it, the climate is very similar to that of Mentone. San Remo is, however, not quite so sheltered, and the air is not quite so dry as at Nice and Mentone. This latter circumstance Dr Daubeny accounts for by the nature of the soil, which at Nice and Mentone is a " thirsty sand and gravel," while at San Remo it is argillaceous. The accommodation for visiters is as yet not so good as at Mentone, although new hotels and villas have recently been erected. San Remo, however, appears to be a decided acquisition to our list of climatic resorts. *Mr Aspinall's* is an unpretending little volume, giving his own experience of San Remo. He had passed a winter in Madeira, which he found " depressing and debilitating," and another in Egypt, which is described as " hot and dusty"; he then proceeded to Nice, the air of which he found most clear and exhilarating, the climate, however, being treacherous, for during bright sunshine bitterly cold winds swept through the gorges from the snowy mountains. He remained in Nice till March, when, having heard the climate of San Remo strongly recommended, he proceeded thither. So pleased was Mr Aspinall with San Remo that he remained for six weeks, and has since passed two seasons there. In Mr Aspinall's opinion the climate of Mentone is enervating and relaxing as compared to San Remo. The following quotation may amuse our readers :—

" On the 24th of November 1864, I took possession of the pretty and nicely furnished new villa which had been previously taken for me, and, I must say, it presented a most attractive appearance. Being very tired with my journey, I was glad to retire to bed early, but there was little sleep for me that night, as the beds were literally swarming with bugs. In justice to Italy, I must say that this was the first time I have ever met with such an annoyance. It was subsequently explained thus : a daughter of the owner had finished her education at a convent, and her younger sister was to take her place there. It is required that each boarder entering a convent shall take her own new mattress, and in order to save money, a new mattress was taken out of the villa, and the old one put into its place. The Italians are not very sensitive to the bites of insects, but after a while it was discovered that the beds were more lively than was agreeable. An experte (as they are here called) was sent for, who took it away, and to use his own words, ' there was nearly a measure full of bugs taken out of the mattress.' Of course, living in the house was out of the question ; I therefore requested an Italian friend to make the best bargain he could.

The owner of the house was inexorable, though he was offered arbitration, and finally half the rent, he would not take anything less than the whole. The result was he brought an action against me for the full rent. An Italian trial is so different from our own, that I must endeavour to give a short account of a very long affair. In the first place, after the usual formalities are gone through, much the same as in England, the advocates on either side plead and argue; I was not present, but I hear there was much wit and joking going on. My advocate, amongst other things, declared that bugs were hardly known in England. He was answered, 'That is clearly a mistake; for I find the word bug in the English dictionary.' 'Yes; and I find crocodile in the Italian dictionary; but surely no one will tell me that crocodiles are natives of Italy.' This pleading is the preliminary step taken in order to decide whether witnesses should be called or not. Had it been decided in the negative, the plaintiff would have gained the cause. After much delay, the judge announced it was a case for witnesses. The defendant's witnesses are first examined; and a most tedious business it is, only two being examined on each succeeding Friday. These witnesses gave their evidence so clearly, that the trial was considered to be virtually over, but to our surprise the plaintiff then brought forward two servants who had been discharged from my service, one of whom he had recently taken into his employ, and the other was not in any way to be trusted. They quite contradicted all that my witnesses had sworn to. All this time there were the bugs in the house to speak for themselves, but they would not send to examine it.

"The trial was going all in my favour, but was afterwards carried to the Court at Genoa; the result was not known when this book was ready for the press."—P. 31.

Mr Aspinall is much interested in the evangelization of Liguria and Piedmont; any profits arising from the sale of his work are to be devoted to helping to defray the expenses of the new English church at San Remo.

*Dr Daubeny's* pamphlet on the " Climate of San Remo," consists of thirty-nine pages, of which only ten consist of original matter, the remainder being occupied by tables of meteorological observations. Such tables are useful enough in their way, but when they occupy a space so disproportionate to the text, they cannot fail to remind us of the relative quantity of bread and sack in Falstaff's tavern bill. Dr Daubeny has also committed a great mistake in leaving Professor Govian's Italian tables untranslated; he should have also reduced the barometrical and thermometrical observations to the same standard as that in which his own are published.

*Dr More Madden's* pamphlet (reprinted from the *Dublin Quarterly Journal of Medical Science*) on the climate of Malaga, consists of twenty-three pages. He commences with some general observations on some of the difficulties which embarrass the study of medical climatology. In these preliminary observations the following unfortunate passage occurs :—

" Fashion has a great influence on the reputation of the climates frequented by English valetudinarians; thus Cannes owes its *renommée* as a winter residence mainly to the influence of a celebrated nobleman, who, some thirty years ago, passed a winter there, and, finding the climate suited him, has since returned each year, followed by others—so that the climate that suits a very distinguished person seems to possess some special attraction for English invalids.

A medical man may, perhaps, attend in the wake, to look after the bodily infirmities of his exiled compatriots; and the doctor, having probably some time on his hands, writes a book in which he proves the superiority of the climate to his own entire satisfaction; and in some cases a very bad climate for phthisical invalids—as, for instance, Mentone—has thus been 'written up' into temporary notoriety. Unfortunately laudations of unsuitable climates are seldom contradicted. Valetudinarians, having little the matter with them, visit Mentone and similar places, and, being benefited by travelling, and change of scene as well as of air, the result is vaunted as an incontestable proof of the sanative action of the climate; but we hear nothing of the cases in which invalids suffering from organic disease are injured by an unsuitable climate, and die in these localities. Thus it is that physicians at home are misled, and their patients abroad suffer the consequence."—P. 4.

A man who writes so uncourteously of his professional brethren has no right to expect that much attention should be paid to his own opinions.

From its latitude, 36° 34′, Malaga has a much higher temperature than the Riviera, and from its situation, protected from the north and west winds, it is very sheltered. The annual rain-fall is inconsiderable, and the number of rainy days unusually small. There is no doubt that with these natural advantages Malaga might be a very valuable resort for the invalid; but, unfortunately, they are more than neutralized by the unhygienic condition of the town. On this point Dr Madden is quite explicit:—

"The hygienic condition of Malaga is as defective as it can well be. In a great many of the houses there is no provision for sewerage of any kind; and even in the more civilized part of the city, in the hotels on the Alameda, the drainage is very bad indeed. The main sewers, which run under the principal street, are choked up by the decomposing accumulation of years, and being provided with immense square openings, through which the dirt and rubbish is thrown into them, in the centre of the streets, the mephitic gases evolved below freely escape into the atmosphere of the narrow lanes of the city. The bed of the Guadalmedina is really the main sewer of Malaga; and as for nearly ten months annually it is little more than a wide dry bed of gravel, being dependant on the torrents in winter for its purification, the odour it exhales in warm weather renders a residence near it as disagreeable as it is unhealthy.

"The connexion between epidemic disease and bad sewerage is, I think, very well illustrated in Malaga, which has at all times been remarkable for the prevalence of zymotic diseases. I have collected from the older Spanish writers notices of no less than twenty-two epidemic pestilences, some of which almost depopulated the city between 1493 and 1804. The earlier of these seem to have been epidemics of genuine Oriental plague, and the latter generally assumed the form of yellow fever. Of late years, since 1834, these pestilences have not appeared, but their place has been taken by Asiatic cholera, which has several times ravaged the town."—P. 18.

*Dr Edwin Lee* is well known as a writer on climate and on mineral waters. Our list, at the head of this article, contains the titles of no fewer than four works by this gentleman, all of which, with a single exception, have passed through more than one edition. Dr Lee has travelled much; he has a good local knowledge of most of the places he describes, and has availed himself freely of the labours of others. Consequently, his works may be considered

as trustworthy guides to the localities he describes, whilst the medical portion of them is generally judicious.

"Nice and its Climate" contains a good account of this celebrated health resort. Two chapters are devoted to a description of Nice and its environs, and one to an estimate of the character of the inhabitants. The Nissards are described as amiable and inoffensive, with scarcely any sense of religion, though devoted to the celebration of their numerous fêtes. They are by no means teetotallers; though there is little intoxication, this seems to be due more to the strength of their heads than to the moderation of their potations. For M. Burnel, quoted by Dr Lee, says, "A prodigious quantity of drink is absorbed during these feasts *al fresco*, especially when the vintage of the preceding year has been good. It is seldom, however, that any great excesses are the consequence, as the Nissard can bear a good deal of drink without inconvenience."

Dr Lee's estimate of the climate of Nice is, we believe, correct; it agrees, on the whole, with Dr Bennet's opinion as to Mentone, making allowance for the somewhat less sheltered situation of the former; and Dr Lee's remarks on its suitableness in special diseases and in particular cases are judicious.

In "Health Resorts of the South of France" Dr Lee describes Hyères, Cannes, Montpellier, Pau, Biarritz, and Arcachon. Arcachon, about forty miles south of Bordeaux, in addition to a mild maritime climate, is supposed to be rendered peculiarly suitable to certain classes of invalids, from the great abundance of pine forests; among these the *Winter villas* have been built, and sufferers from phthisis, bronchitis, and rheumatism are said frequently to derive benefit from a residence there. This has been ascribed to the large amount of ozone evolved by the fir-trees.[1]

"The Baths of Nassau" has reached a fourth edition, and gives a fair account of these well-known places of resort.

In "the Baths of Switzerland and Savoy," Dr Lee describes several health resorts which are, as yet, comparatively little known in this country. Among these, we may mention St Gervais, four leagues from Chamouni, at an elevation of 2000 feet, with its sulphurous and chalybeate springs; Saxon, in the valley of the Rhone, the waters of which are rich in iodides and bromides; St Moritz and Tarasp, in the Engadine, the highest permanently inhabited country of Europe, with their saline, chalybeate, and sulphurous springs. There is also a description of some of the mountain resorts in Switzerland, and an appendix containing observations on the effects of mountain air.

*Dr Sutro's* work on the German mineral waters is of great value. The work originally consisted of eighteen lectures delivered at the Hunterian School of Medicine. These have been republished in

---

[1] See a review of the "*Almanac Général d'Arcachon*" in the August number of this Journal for 1863; and a paper by Dr William Ireland on "the Medical Topography of Kussouli," in the number for July 1862.

their original form, but an appendix has been added containing information regarding almost all the spas and climatic resorts in Europe. Dr Sutro has performed his work in a most conscientious manner. He describes the best routes for arriving at each spa, characterizes the climate, gives an analysis of the mineral water, and mentions the indications for and against its employment in particular cases. The work has additional value from having been written by a German thoroughly acquainted with the English language and with English medicine.

In general, English cookery is spoken of as very inferior to Continental; but it will be seen from the following passage that Dr Sutro considers our system of dietary very superior to what is usually met with abroad. After having described the mode of life at Carlsbad, he says,—

"And here I may state, by-the-by, that the rational manner of English living is one of the greatest safeguards against epidemic diseases.

"No wonder that life should be longer here than in many parts of the Continent, with a considerably greater freedom from avoidable diseases during the allotted period, and with comparatively greater physical resisting power, and other unmistakable signs of improved nutrition! As regards food, whilst in this island the most tender flesh from the best-fed domestic animals is simply exposed to the action of heat, just sufficient to increase its solubility in the gastric juice, in many parts of the Continent skilful cooks have to prepare savoury liquids out of the albuminous and gelatinous portions of the meat, and to season them in such various modes as to make a very agreeable impression on the palate. The warm liquid distending the stomach must momentarily diminish the power of its muscular fibres. Nevertheless, the tougher and more fibrinous portion, the parent of the juicy soup, is now introduced, and forces the intestines to unwilling action. Some other dishes make their appearance, with the mere object of recalling the vanishing appetite, and of creating an artificial desire for a greater reception of food. And even now, when the more substantial dishes come before you in their various shapes, art tries to improve on nature and make them more palatable, by sauces and numerous intricate contrivances. However satisfactory all may appear while at table, still on rising, although you may have taken an inconsiderable sum-total of really substantial nourishment, you feel overloaded, your movements are impeded, the physical oppression reacts on the mind; drowsiness, lassitude, and incapacity for exertion naturally ensue. An artificial stimulus, both for abdominal action and nervous power, is called into aid—viz., coffee.

"Now, just imagine the consequences. Whilst the one satiated the want of nature, and supplied the organic waste by the simplest substitute, which had merely to be dissolved and reconstituted into its former atoms to produce healthy chyle—the most appropriate for performing the nutrient function of the whole body—the other imposed much greater work on his teeth, on his salivary glands, and his abdominal viscera; and when all is summed up, when the whole mass is sifted for contributing its share towards nutrition, the very purpose of the whole laborious task, why, it is found that very little can be used for sanguification—at all events, less than from the former simple and short repast. Add to this increased work the proportionally advanced inability of performance, and you will not wonder at the thousands and thousands who suffer from piles, at the numerous atonic diseases of a vicious sanguification, at the frequently debilitated constitutions, and at the shortened period of existence affecting so many individuals from avoidable causes. The injurious influence of excessive smoking on the composition and power of the mechanical masticators prevents the proper admixture of saliva, and the necessary comminution of the food, and thus heightens the evil."—P. 99.

*Memoirs read before the Anthropological Society of London.* Vol. I., 1863-64.

*Lectures on Man.* By Dr CARL VOGT. Edited by JAS. HUNT, Ph. D.

*The Plurality of the Human Race.* By GEORGES POUCHET. Translated and Edited by HUGH J. C. BEAVAN.

*The Anthropological Treatises of Blumenbach and Hunter.* Translated and Edited by THOMAS BENDYSHE, M.A.

SINCE we gave, last year, a short notice of the publications of the Anthropological Society of London, the works, the titles of which we have placed above, have been issued, by the Council of the Society. The volume of memoirs consists of a selection of the more important papers, read during the session 1863-64, and contains papers on a variety of subjects connected with the distribution of man on the surface of the earth, his physical structure, social habits, and religious observances.

As several of these subjects do not come within the province of matters discussed in this Journal, we shall pass them over and content ourselves with a brief notice of those papers which treat more especially of the topics we are in the practice of occasionally giving a place to in our pages. Falling under this category is a communication by Dr Thomas Peacock, on the weight of the brain and the capacity of the cranial cavity in the negro. This observer has had larger opportunities of examining the brain of the negro than usually fall to the lot of the anatomist in this country, and at pp. 65, 520, he records the weights of five negro brains which he has examined. He compares his results with those obtained by Tiedemann, Astley Cooper, Sömmering, and John Reid, and arrives at the conclusion that there is no very marked difference between the ordinary size of the brain in the African and European; but they certainly indicate that the brain is usually smaller in the former race than in the latter. It is to be regretted that Dr Peacock confines his description to considerations of size and weight, and does not enter into the question of the mode of arrangement of the cerebral convolutions in the negro. The joint authors of the Crania Britannica contribute two papers to this volume. Dr Thurnam furnishes an elaborate memoir "On the Two Principal Forms of Ancient British and Gaulish Skulls"; and Dr Barnard Davis one on the Neanderthal Skull." The latter writer advances what he considers to be a satisfactory explanation of the mode of production of the peculiar form of the Neanderthal cranium : " it is simply an abnormal example, and owes its peculiar forms to synostosis of the cranial bones before the calvarium had attained its full development." Captain Burton and Mr Pritchard communicate papers descriptive of some of the peculiar customs, the one of the

people of Dahome, the other of the South Sea Islanders, which, however well adapted they may be for perusal by the purely professional or scientific student, are, from the grossness of their descriptive details, quite out of place in a book which, like the one under review, circulates amongst men of various pursuits, and which may even be found (in complete ignorance of the nature of some of its contents, let us hope) lying on a drawing-room table.

Dr Vogt's Lectures are partly occupied with an investigation into man's physical structure, more especially that of his skull, brain, extremities, pelvis, and skin; those parts, indeed, to which the ethnologist has particularly directed attention in his endeavours to discriminate between the different races of men. He then enters into a comparison of man's structure with that of the ape. He assumes the actual descent of the human race from the apes, and believes that the differences between the two will become greater by the further development of man, as the result of selection and intermixture. He gives an account of the discoveries recently made, which tend to show that the human race has existed much longer on the face of the earth than was formerly supposed. The Lectures are evidently the production of a man of very strong opinions; one, too, who, in his strenuous advocacy of his own peculiar views, forgets that a question has often more than one side, and that in the investigation of such highly complex problems as he has undertaken, it is above all things necessary to look at every aspect which the subject may be capable of presenting. Every chapter of the work, too, is defaced by a scoffing tone,—a sneering at those matters which men commonly regard with reverence,—a mode of treating the subject which is altogether unworthy of a true man of science, and which ought most effectually to have put a bar to the translation of the book into the English language.

M. Pouchet, in his memoir, as its title would indicate, advocates the polygenistic view of the origin of the human race. Like most of the writers of the same school, he attaches but little value to the influences of the media with which man is surrounded. By this we understand not only the influence which civilisation, clothing, climate, and diet exercise in modifying man's physical and moral nature, but the geological changes which have altered the form of the earth's surface since the time when man first appeared on it. The last-named element is one, as Mr A. R. Wallace has recently pointed out, which is undoubtedly entitled to great consideration in the discussion of this question, for now that the antiquity of man, illustrated by his co-existence with quadrupeds long since extinct, seems generally admitted, a much longer period is allowed for the action of these modifying causes to have taken place, and a powerful argument is advanced in favour of the descent of the whole human race from common ancestors.

The last volume on our list contains a translation of the contributions of the illustrious Göttingen professor, Blumenbach, to the

natural history of man, and is mainly composed of a translation of the first and third editions of his celebrated essay, originally published at the close of the last century, on the natural variety of mankind. The essay by Dr Hunter (who must not be confounded with the brothers William and John Hunter of illustrious memory) on the varieties of man, an old Edinburgh graduation thesis, is interesting, because it appeared in the same year as the first edition of Blumenbach's memoir on the same subject.

=====

*Tension of the Eyeball; Glaucoma; and some Account of the Operations practised in the Nineteenth Century for their Relief.* By JOHN VOSE SOLOMON, F.R.C.S., Surgeon to the Birmingham and Midland Eye Hospital, etc. 8vo, pp. 80. London: John Churchill and Sons: 1865.

*The Optical Defects of the Eye and their Consequences, Asthenopia and Strabismus.* By JOHN ZACHARIAH LAURENCE, F.R.C.S., M.B. Lond., etc., Surgeon to the Ophthalmic Hospital, Southwark, etc. 8vo, pp. 112. London: Robert Hardwicke: 1865.

Mr SOLOMON's little book consists of a reprint of a lecture delivered before the Midland Medical Society in 1863, a short appendix to this lecture, and a collection of cases in illustration of the views advanced. A short and very practical introduction is prefixed, in which the author defines the term " Tension of the Eyeball," and gives its most characteristic symptoms.

The account of the operations practised for the relief of tension begins by describing the puncture of the sclerotic invented by Dr Whyte in 1802, and the similar puncture of the cornea described by Mr Wardrop, in various papers, in 1807 and 1813. The subsequent researches of Mackenzie and Desmarres on the same subject are then noticed, and a very brief account of the invention and application of iridectomy, by Von Graefe and others, is given.

Under the head of Iridectomy we are introduced to the author's theory, by which he explains the relief it gives in glaucoma; it is the following:—

" That the division of the ciliary nerves, at the point where they pass from the ciliary muscle into the iris, forms an important element in the operation; that thereby a more healthy action was induced in the ciliary ganglion, which, as proved by the experiments of Dr Radclyffe Hall, presides over the organic function of the eye."

This theory is supported by ten reasons, which, numerous as they are, by no means convince us that the relief given by operative interference in glaucoma is due to nervous influence, rather than to mere mechanical relief of tension.

The next operative procedure noticed is that popularly known

as Hancock's, the so-called divison of the ciliary muscle; and the remaining twenty-five pages of the first part of the book are devoted to a comparison of the theoretical value and practical advantages of this operation, with those of another invented by the author, to which he gives the name of intra-ocular myotomy. Both aim at the ciliary muscle, but the operations vary in method of performance, in the direction in which the fibres are divided, and specially in the position and size of the external wound. In discussing these points, Mr Solomon makes out a good case for his own modification; but the tone and manner in which he enters into the question of priority, and describes from his own observation Mr Hancock's mode of operating, are hardly courteous, and certainly not so dispassionate and unpersonal as a scientific discussion ought to be.

The cases are twenty-four in number, and include examples of intra-ocular myotomy, division of the ciliary nerves, and iridectomy, so that the results of the different procedures may be compared with each other. On the whole, though he has not exhausted the subject, Mr Solomon has succeeded in arranging in a sufficiently practical manner the various operations for the relief of ocular tension. He, however, rather under-estimates the advantages of iridectomy, in certain cases, over either puncture or intra-ocular myotomy.

*Mr Laurence's* work discusses a subject of very wide extent and no ordinary difficulty. Deeply impressed with the accuracy and value of Professor Donders' researches on optical refraction and accommodation, Mr Laurence has been an apt pupil in his school, and in this work endeavours to simplify the labours of those who wish to study under the same master. Thoroughly to appreciate or even to understand Professor Donders' views, requires a very much more extensive knowledge of optics and applied mathematics than is generally possessed by members of our profession. Mr Laurence has endeavoured (and we think succeeded admirably in his attempt) to condense and simplify the chief foreign and British authorities on the subject, so as to supply to the general mass of the profession most of the information that is practically valuable, and to afford an excellent introduction to the subject for those who may wish to follow up more fully the researches of the Utrecht school.

The two first chapters, entitled " Optical Considerations" and " Physiological Optics," are a fit introduction to what is to follow. The latter especially gives, very clearly and briefly, an excellent account of " accommodation" and " binocular vision." The author strongly supports the view that the ciliary muscle is the chief, if not the only agent in accommodation.

Pathological optics divide themselves naturally into,—1. Anomalies of refraction—myopia, hypermetropia, and astigmatism; 2. Anomalies of accommodation—presbyopia, paralysis of accommodation, and asthenopia. The last chapter is devoted to the discussion of the interesting but little known connexion which exists between

convergent strabismus and hypermetropia. Mr Laurence, with justice, points out the propriety of viewing strabismus not merely as a deformity but as a cause of progressive loss of vision in the squinting eye. He does not notice, however, the very remarkable, but little understood cases, so often observed, in which the squinting eye has been unable to read even the largest type, and yet, *immediately* after the division of the muscle, before the blood has ceased to flow from the conjunctiva, has been enabled to read the very smallest.

We are to look for the explanation of these cases, not so much in any new spot of the retina brought into play, as in an instantaneous improvement of accommodation, gained by the relief of the muscular tension which had pulled on the tendinous collar of the eyeball, and perhaps thus may have paralyzed the ciliary.

We can thoroughly recommend Mr Laurence's book as a simple yet trustworthy guide to a difficult and important subject.

―――――

*The Science and Practice of Medicine.* By WILLIAM AITKEN, M.D., Professor of Pathology in the Army Medical School, etc., etc. In two volumes. Fourth Edition, revised, and portions re-written. London : Griffin and Co. : 1865.

ABOUT eighteen months ago, we brought under the notice of our readers the second edition of this work, and had the pleasure of expressing the high opinion we entertained of it, especially in regard to its pathological portion. The success which the book has met with is abundant proof that the medical profession thoroughly coincided in our favourable judgment. The second edition was exhausted within six months of its publication ; a third edition was as speedily disposed of; and the fourth edition is now lying before us. The rapidity of sale of such a large work, consisting of nearly 2000 pages, is, we believe, entirely without precedent in the annals of medical publishing. We heartily congratulate the author on his success, which has been thoroughly well deserved. Writing at one time under great difficulties and discouragements, Dr Aitken kept his object steadily before him, and " The Science and Practice of Medicine" must now be looked upon as the standard text-book in the English language.

The third and fourth editions are no mere reprint of the second. In our former notice, while praising highly the pathological portion of the work, we took occasion to point out what we considered deficiencies in the account of the symptomatology, and treatment of disease. We are happy to see that these deficiencies have been to a considerable degree supplied. We may allude, in particular, to the chapter on diseases of the lungs, in which the account of the

physical signs, formerly meagre, has been brought fully up to the existing state of medical diagnosis. The directions for the treatment of disease are now also given more fully, and are more satisfactory than formerly. Still Dr Aitken must not rest satisfied; we doubt not that each succeeding edition will be an improvement upon its predecessors.

The fourth edition, though with numerous and not unimportant alterations, is essentially the same as the third. The principal additions we notice are, an account of the mode of employing the laryngoscope, and of the information to be derived from it; a description of a larva, the exciting cause of Bulama boil; and an appendix to the chapter on parasites, "on the occurrence of 'Pentastoma constrictum' in the human body as a cause of painful disease and death."

It is needless to add that, both to the medical student and practitioner, we can unhesitatingly recommend Dr Aitken's work as containing an admirable exposition of the present condition of the Science and Practice of Medicine.

---

# Part Third.

## PERISCOPE.

### REPORT ON PHYSIOLOGY.

#### BY W. GILCHRIST, M.D., TORQUAY.

### PHYSIOLOGY OF THE BLOOD.

ON THE ORGANIC NITROGENIZED PRINCIPLES OF THE BODY, WITH A NEW METHOD FOR THEIR ESTIMATION IN THE BLOOD. BY A. FLINT.

*(American Journal of Medical Science, vol. xlvi.)*

THE author thinks that albuminous substances, when dried and deprived of their water, salts, etc., have no interest for the physiologist. The albuminous matter of the blood, milk, etc., must not be regarded as a salt dissolved in water, but (as Robin and Verdeil supposed) we must regard the albumen as a fluid in itself, and a part of the water of the collective fluid as belonging to it. This water, belonging to the fluid form of albumen, passes over by coagulation into the coagulum; and albuminous bodies must be examined, weighed, etc., in this moist condition, seeing that it is the form in which they are serviceable to the economy, and the only form in which the varieties of albumen can be distinguished from each other. Adopting these views, Flint seeks to determine the amount of fibrin and albumen in the blood. The methods of research were those of Figuier, all excepting the drying. The results were as follows. In the venous blood of two healthy men, the blood contained,—

| | |
|---|---|
| Fibrin, . . . . . | 8·82 and 7·44 |
| Albumen, . . . . | 329·82 and 277·55 |
| Blood corpuscles, . . | 495·59 and 480·44 |

In a plethoric person the proportions were,—

|  |  |
|---|---|
| Fibrin, | 16·81 |
| Albumen, | 311·47 |
| Blood corpuscles, | 484·51 |

In an anæmic individual,—

|  |  |
|---|---|
| Fibrin, | 11·34 |
| Albumen, | 219·47 |
| Blood corpuscles, | 382·95 |

In the blood of two oxen,—

|  |  |
|---|---|
| Fibrin, | 14·52 |
| Albumen, | 195·24 |
| Blood corpuscles, | 623·36 |

Adopting the estimates of Becquerel and Rodier for the remaining blood constituents, Flint computes the proportions of all the constituents of human blood to be as follows :—

|  |  |  |
|---|---|---|
| Blood corpuscles, | | 495·59 |
| Plasma { | Water, | 155·42 |
| | Fibrin, | 8·82 |
| | Albumen, | 329·82 |
| | Fatty extractive matters, salts, | 10·35 |
| | | 1000·00 |

### ON THE REDUCTION AND OXIDATION OF THE COLOURING MATTER OF THE BLOOD. BY G. G. STOKES, M.A., SEC. R.S.

Professor Stokes, in verifying the experiments of Hoppe on blood analysis by the spectrum, found that the colouring matter is immediately decomposed by acids, and more slowly by caustic fixed alkalies—the coloured product of the decomposition being the hæmatin of Lecann. He proposed to himself the attempt to imitate the change of colour of arterial into that of venous blood, on the supposition that it arises from reduction. As acids affect the colouring matter, the reducing agent selected was one which is compatible with an alkaline solution. If to a solution of protosulphate of iron enough tartaric acid be added to prevent precipitation by alkalies, and a small quantity of the solution, previously rendered alkaline by either ammonia or carbonate of soda, be added to a solution of blood, the colour is almost instantly changed to a much more purple red, as seen in small thicknesses, and a much darker red than before as seen in greater thickness. The change of colour which recalls the difference between arterial and venous blood is striking enough, but the change in the absorption spectrum is far more decisive. For an account of this we must refer the reader to the original paper. If the purple solution be exposed to the air in a shallow vessel, it quickly returns to its original condition, as seen by the spectrum. That the change which the iron salt produces in the spectrum is due to a simple reduction of the colouring matter, and not to the formation of some compound of the colouring matter with the re-agent, is shown by the fact, that a variety of reducing agents of very different nature produce just the same effect.

The inference to be drawn from these facts is, that *the colouring matter of blood*, like indigo, *is capable of existing in two states of oxidation distinguishable by a difference of colour and a fundamental difference in the action on the spectrum. It may be made to pass from the more to the less oxidized state by the action of suitable reducing agents, and recovers its oxygen by absorption from the air.* As the term hæmatin has been appropriated to a product of decomposition, Professor Stokes proposes to name the original colouring matter of the blood *cruorine ;* and to distinguish it in its two states of oxidation the terms *scarlet* and *purple* cruorine may be employed.

If a watery extract of blood is left aside in a corked bottle, or even in a tall narrow vessel open at the top, it presently changes in colour from a bright to

a dark red, decidedly purple in small thicknesses. If this be analyzed optically, the spectrum is found to be that of purple cruorine. On shaking the solution with air it immediately becomes bright red, and now presents the optical characters of scarlet cruorine. It thus appears that scarlet cruorine is capable of being reduced by certain substances, derived from the blood present in the solution, which must themselves be oxidized at its expense.

When acids are added to blood, a brownish-red colour is produced, the spectrum of which entirely differs from that of scarlet or purple cruorine. This colouring matter (hæmatin) is a product of the decomposition or metamorphosis of some kind of the original colouring matter. Like cruorine, too, it is capable of reduction and oxidation, and in each condition it is characterized by a special spectrum. The two forms may be named respectively *brown hæmatin* and *red hæmatin*,—the former or oxidized substance being the hæmatin of chemists.

It has been a disputed point whether the oxygen introduced into the blood in its passage through the lungs is simply dissolved, or is chemically combined with some constituent of the blood. Now, it has been shown that we have in cruorine a substance capable of undergoing reduction and oxidation, more especially oxidation; so that if we may assume the presence of purple cruorine in venous blood, we have all that is necessary to account for the absorption and chemical combination of the inspired oxygen. Seeing that the change of colour from arterial to venous blood, as far as it goes, is in the direction of the change from scarlet to purple cruorine, that scarlet cruorine is capable of reduction even in the cold by substances present in the blood, and that the action of reducing agents upon it is greatly assisted by warmth, we have every reason to believe that a *portion* of the cruorine present in venous blood exists in the state of purple cruorine, and is re-oxidized in passing through the lungs. It is probable that it is only a small portion of the cruorine present in venous blood which exists in the state of purple cruorine. Were it otherwise, any extensive hæmorrhage could hardly fail to be fatal, if, as there is reason to believe, cruorine be the substance on which the function of respiration mainly depends; nor could chlorotic persons inhale as much carbonic acid as healthy subjects, as is found to be the case. The author believes that the dark colour of venous blood is due to a partial reduction of scarlet cruorine in supplying the wants of the system, and not to the small additional percentage of carbonic acid which it contains.—*Proceedings of the Roy. Soc.* 1864.

OBSERVATIONS UPON THE NATURE OF THE RED BLOOD-CORPUSCLE.
BY LIONEL S. BEALE, M.B., ETC.

ON THE GERMINAL MATTER OF THE BLOOD, WITH REMARKS UPON THE FORMATION OF FIBRIN. BY LIONEL S. BEALE, M.B.

Applying his general views upon "germinal matter" and "formed material" to the blood, Dr Beale believes that the white blood-corpuscle consists of the former, the red corpuscle of the latter, and that in red corpuscles possessing nuclei, the nucleus represents the germinal matter. In regard to a cell-wall Dr Beale urges some striking observations disproving its existence. Taking, for illustration, the red blood-corpuscle of the frog, the outer part which cannot be coloured by carmine, is "formed material" which was once in the state of "germinal matter," and was capable of producing matter like itself, and of being resolved into coloured formed material. But this formed material once produced cannot form matter like itself, although it can be changed, dissolved, and converted into other substances. It is the seat of physical and chemical actions alone. Vital changes are restricted to the germinal matter. In the nutrition of the blood-corpuscle, pabulum passes through the outer coloured portion into the germinal matter, where it acquires the same vital powers which the germinal matter already existing possesses. When blood becomes stationary the white corpuscles and the nuclei of the red increase in size and subdivide. In such circumstances "granular corpuscles" may be developed from the white or from young red corpuscles. In adult mammal's blood are certain corpuscles, granular in appearance, colourless, and much smaller than the

ordinary red corpuscles; so small, in fact, that they can only be seen by powers magnifying upwards of 1000 diameters. These corpuscles, the author believes, to consist of germinal matter which has just commenced to undergo conversion into red formed material. The red corpuscles vary in size much more than is commonly supposed, and they differ very much in transparency and refractive power, some being only just visible in consequence of the extreme transparency of the material of which they are composed.

In the second paper quoted above, Dr Beale advances some very remarkable views. He believes in the existence of very minute particles of germinal matter (less than the $\frac{1}{30000}$th of an inch in diameter) in the blood. These masses of germinal matter multiply by a protrusion taking place from the surface, and final separation into two separate masses. Nuclei may arise in these masses.[1] In inflammation of the vessels of the frog's foot, the increase of white corpuscles is not alone dependent on arrest of the circulation, but the corpuscles actually multiply in the clot. In regard to the development of cells in exudations, the author advances the view that " exudations," although clear fluids, really contain a multitude of extremely minute particles of living matter, which are intimately related to the white blood-corpuscles, and that these grow and become one source of small " granular cells" or corpuscles. Further, living or germinal matter may retain its vitality, so that particles may be formed in one organism and be carried to another where they may grow. It seems to the author probable " that many contagious diseases are due not to the propagation and transference of vegetable organisms, but to small particles of living animal matter which have descended from the germinal matter of one organism, and have been transferred to another." Finally, in reference to the formation of fibrin, Dr Beale holds that it is " the result of a vital process," that it is " formed material," and " that living germinal matter becomes fibrin."— *Journal of Microscopical Science*, vol. iv. 1864.

SOME OBSERVATIONS ON THE BLOOD, CHIEFLY IN RELATION TO THE QUESTION, IS AMMONIA IN ITS VOLATILE STATE ONE OF ITS NORMAL CONSTITUENTS? BY JOHN DAVY, M.D.

Dr Davy is opposed to the idea that the volatile alkali ammonia is in anywise concerned in the coagulation of the blood. From experiments on man and animals he believes that ammonia is exhaled from the lungs, and excreted by the skin. Further, that it exists in larger proportion in venous than in arterial blood, and that it is probable that in those animals in which the blood is least thoroughly aerated, such as the batrachians and other allied genera, the proportion of this alkali is greater than in those animals, such as birds and mammalia, of higher temperature and more complete pulmonary respiration. Dr Davy supposes that the source of the ammonia in the blood is the stomach and intestines, or rather the ingesta they contain.—*Edin. New Philosoph. Journal*, vol. xix. 1864.

A CASE OF LIPÆMIA. BY C. SPECK.

(*Archiv für Wissenschaft Heilkunde*, 1864.)

The case observed by Speck was one in which there was an enormous increase of the fatty matter of the blood, particularly of the serum. The patient was a man of a rather corpulent habit; but at the time the observation was made he remarked a reduction of his fat. The blood (obtained by cupping) was more of a yellow than of a red colour, and speedily separated into two equally thick layers, of which the upper was white, cream-like, and full of fat particles. Ether extracted from the dried blood 7·3 per cent. of fat; the cholesterine did not appear to be greater in quantity than in normal blood. This condition of the blood continued several weeks, diminishing in intensity. During it the man felt aversion to fatty food, and the condition could not be explained by any increased fat absorption from the intestines. Perhaps the

_____
[1] The precise function of nuclei is not stated.

state of the blood was in relation with the reduction of the man's corpulence. His general health was but little disturbed.—*Henle u. Meissner's Bericht*, etc., 1864.

### ON THE CHANGES IN THE COLOUR OF THE BLOOD FROM OXYGEN AND CARBONIC ACID.   BY ALEXANDER SCHMIDT, OF DORPAT.

### ON THE CRYSTALLIZATION OF BLOOD.   BY THE SAME.

The effects of oxygen and carbonic acid in changing the colour of the blood were referred by Harless and Scherer to a change of form which the blood-corpuscles undergo—to a physical action of the gases.   This view was opposed by Bruch, who made the observation, that highly diluted blood in which colouring matter was free, is similarly affected by the two gases.   He inferred that the action was a chemical one upon the colouring matter, and that the changes of colour were not due to alterations of form in the blood-corpuscles.   In the paper before us, Schmidt gives the results of experiments upon solutions of blood entirely free (by filtration) from blood-corpuscles.   The result he arrives at is, that carbonic acid acts chemically in changing the colour; but oxygen, by displacement of carbonic acid, restores the natural colour without producing any chemical change.   Oxygen certainly exerts a positive effect upon the colour of the blood, since it makes it brighter than it is when deprived of gas by the air-pump; but this effect depends upon a physical process, viz., upon change of form of the blood-corpuscles.   The effect of oxygen in brightening the colour of the blood is very marked, when blood-corpuscles are present, but in their absence the change produced by oxygen is but slight and slowly effected.   In the action of acids and alkalies, when they dissolve the blood-corpuscles (alkalies), or make them invisible (acids), the marked change of colour occurs chiefly in a chemical way, by material alterations of the hæmatin.   Neutral salts, ether and alcohol in small quantities, do not affect the hæmatin, but act by altering the optical relations of the corpuscles.

In reference to the crystallization of the blood, Schmidt remarks that the destructibility of the blood-corpuscles by oxygen corresponds to the readiness with which it may be crystallized.   He believes also that blood-corpuscles are more readily destroyed by oxidation than is generally thought.   In the first stage of oxidation, the blood-corpuscles become colourless, the pigment passing into the intercellular fluid; in the next stage, the colourless remains of the corpuscles are dissolved.   In the first stage, the hæmato-globulin is crystallizable; in the second it is not.   In order to have crystallization, the hæmato-globulin must first be dissolved in the serum, and must next be precipitated by a diminution of the solvent fluid, as by evaporation or by addition of such salts as the sulphate of soda.   Alcohol and ether produce both effects.   The globules disappear entirely under their influence, but after the addition of alcohol, concentrated solutions of albumen, sugar, or neutral salts, render them visible again, as pale, colourless discs.   This is not the case after the addition of ether. Alcohol caused crystallization in dog's blood only, not in that of the horse or ox.   Blood-crystals being easily soluble in dilute acids or alkalies, the alkali of the blood tends to hold them in solution, and hence neutralizing this alkali by carbonic, acetic, or other acids, facilitates their deposition.   It is in this way only that the addition of carbonic acid acts in producing crystallization; a further deposit always ensues on evaporation.   A partial oxidation prepares the blood for crystallization, by separating the hæmato-globulin from the corpuscles; but carried further, it altogether prevents it.   Both effects are rapidly produced by ozone, as by shaking the blood with a few drops of ozonized turpentine.   When the blood thus shaken loses its darkened colour and transparency, and becomes greenish and turbid, or exhibits white flakes, the period of crystallization is past.   The most favourable time is when the hæmato-globulin is dissolved out of the cells, but the latter are still distinguishable.

Schmidt observed, also, that if the corpuscles of horse's blood were allowed to subside, and then the serum, being carefully drawn off, were shaken with so much ozonized turpentine that the corpuscles were dissolved in one or two

hours, the blood formed a jelly-like coagulum. Allowing this to contract, by separating it carefully from the walls of the vessel, a fluid containing crystallizable haemato-globulin is pressed out. The clot contains no corpuscles, only flakes, which exhibit a remarkably fine granular structure. Thus by active oxidation of the corpuscles these are not only dissolved out, but a process is commenced which has a certain resemblance to the coagulation of fibrin. The crystallization of the blood by means of an electric current is probably also due to an oxidizing process, effected by the ozone which the current produces from the oxygen contained in the blood.—*Virchow's Archiv*, band xxix. p. 14, 1864.

### ON OZONE IN THE BLOOD. BY DR W. KÜHNE AND DR G. SCHOLZ.

Agreeing generally with the proposition of Schmidt as to the presence of ozone in the blood, Kühne and Scholz direct special attention to an experiment of Schmidt's, which appears contradictory to the general conclusions. They found that blood deprived of all its oxygen by carbonic oxide gas, gave decided indications of the presence of ozone—gave an ozone reaction when no oxygen was present. When the experiment was performed with certain precautions, the result, however, was very different. Into the cork of a large flask four tubes were made to pass. Through one of these, carbonic acid gas was passed for two hours, the gas finding exit through another tube which was connected with a bottle containing pyrogallic acid and solution of soda. In this way every trace of atmospheric air was banished from the apparatus, the proof thereof being found in the pyrogallic acid remaining uncoloured. By means of a third tube a platinum wire suspending a slip of paper could be dipped in guiac tincture (within the apparatus) and held free in the flask. A fourth tube connected, through a canula and stop-cock, with the carotid of a large dog, had the stop-cock opened, and some pounds of arterial blood were now admitted to the flask. Again the air was tested to ascertain the absence of atmospheric air. No change was observed in the colour of the guiac paper. The next step consisted in passing into the blood a current of pure carbonic oxide from a gasometer. Immediately the blood (previously rendered dark by the carbonic acid) became of a bright red colour, and the pyrogallic acid testified that oxygen was being expelled from the blood. Notwithstanding, the guiac tincture did not exhibit the slightest change of colour. The attempt to extract ozone in the gaseous condition from the blood entirely failed, as previous attempts made by this had also done. Further experiments led Kühne and Scholz to the conclusion, that when blood saturated with carbonic oxide is perfectly free from oxygen, and incapable of absorbing oxygen, it nevertheless possesses the property of ozonizing the oxygen with which it is in contact. The blood-corpuscles, or rather the haemo-globin, behave towards oxygen in the same way as finely divided platinum. They ozonize oxygen without themselves undergoing any change.—*Virchow's Archiv*, band xxxiii.

# Part Fourth.

# MEDICAL NEWS.

## TRIAL OF MARY BAIRD OR DOWNIE FOR CHILD-MURDER, WITH REMARKS.

The following case of infanticide, tried at the recent Glasgow Circuit, on Thursday, October 5, 1865, before Lord Ardmillan and a jury, presents several interesting features from a medico-legal point of view, chiefly as illustrating

the difficulty of arriving at anything like precise conclusions as to the cause of death in cases of this nature, from post-mortem examination of the body of the child. The ease with which infant life may be destroyed, and the danger of forming hypothetical opinions in an offence, to which, theoretically at least, a capital sentence is appended, is no doubt the best excuse for this state of things. But it cannot, we think, fail to be matter of regret alike to the medical and the legal professions, that in a crime which recent revelations prove to be daily becoming more common amongst us, and to which we have the authority of Mr Fitzjames Stephen[1] for stating, a large proportion of the murders committed in England belong, the uncertainty of the signs of murder in the case of newly-born children should be, at the present day, practically speaking, as great as it was in 1783, when Dr William Hunter[2] wrote his celebrated essay on the subject. Whether this arises from any defect in the provisions of our law on the matter, or from the unsatisfactory condition of medical science in the investigation of cases of this nature, we cannot now stop to inquire. But it is impossible to avoid the conclusion, especially considering the small proportion which the cases which come to trial bear to the total number of child-murders committed during the year, that some means ought to be taken by the legislature to prevent the rapid spread of this moral pestilence, and to diminish the waste of infant life which is the result.

The prisoner Mary Baird or Downie was placed at the bar charged with the crime of child-murder, in so far as, on the 9th of April last, in a field in the parish of Mearns and shire of Renfrew, known by the name of Shawhill Park, having been delivered of a living female child, she did wickedly and feloniously attack and assault the said child, and did compress or cover up its mouth and nostrils with her hand or hands, and did thus or by some other means to the prosecutor unknown, choke or suffocate the said child; by all which, or part thereof, the said child immediately or soon thereafter died, and was thus murdered by the said Mary Baird or Downie.

The pannel pleaded not guilty, and was defended by William Ludovic Mair, and J. R. Buntine, Esqs.; for the Crown, J. Burn Murdoch, Esq., acting A.D., assisted by W. A. Brown, Esq.

The general outline of the case may be thus shortly given :—The prisoner was married to James Downie, a labourer, on New Year's day last; previous to his marriage he had had connexion with her. In consequence of a quarrel which Downie had with his wife's mother, with whom she lived, he left her a day or two after the wedding ceremony. It was attempted to be proved, on the part of the Crown, that the cause of Downie's separation from his wife was some doubt which had been thrown on the paternity of the child with which she was then pregnant. This, however, failed, owing to the want of an important letter which Mary Downie had received from her husband, and which was nowhere to be found. The marriage took place in January, and the child was born in the April following. The pannel, although she seems to have avoided proclaiming her situation to all beyond her own family circle, does not appear to have made any concealment of it. Every preparation was made for the event, caps were sewn, chemises bought; all that was wanting was a little piece of flannel, and the prisoner's mother thought she had an old

[1] General View of the Criminal Law of England, p. 122.
[2] On the Uncertainty of the Signs of Murder in the case of Bastard Children, published in the Medical Observations by a Society of Physicians in London.

petticoat in the house that would do. On the morning of Sunday, 9th April, as she states in her declaration, she rose early to pluck nettles for the kail, thinking, as she was not feeling very well, that a walk might do her good. She had hardly, however, got the length of the park behind her mother's house, when she was suddenly seized with the pains of labour, and lying down on the grass, at a place where the field slopes gently downwards, was there delivered of a female child. She then became unconscious, and in that condition remained for about two hours. On coming to herself she saw the body of the child lying at some distance from her, and the after-birth coming away and drawing towards the child. She could not say whether the child was born alive or not; but, on lifting it, it was dead. Hastily taking up the body, she was going with it towards her mother's house, when seeing some men standing at the door, she turned aside, and with a bit of slate scraped a hole in the midden at the back of the house, laid the body in it, and covered it up, where it remained for some days. On being charged with having had a child, by the officers who were sent to inquire into the murder, she at first denied it, but afterwards led the way to the dungstead where the infant was buried, and pointed out the place to the police-officers, who, procuring a spade, found the body hidden about ten inches below the surface.

The medical evidence, on which the whole case hinged, was as follows:—

*Dr Walter Boyd M'Kinlay,* examined by Mr Brown.—I am a doctor in Paisley. I received, on the 15th April, instructions to inspect the prisoner. I did so with Dr Richmond, and prepared the following report:—

*Paisley, 15th April 1865.*
This day, within the County Police-Office, Paisley, we, the undersigned, examined the person of Mary Baird or Downie, aged 22 years, usually residing at Gateside, Mearns, then in custody. The breasts were large, and upon their being pressed milk issued from the nipple, around which there was a large dark areola. On examining the vagina, it was found to be dilated, and the uterus felt larger than usual. From the private parts a quantity of bloody discharge was issuing, and this had evidently been doing so previously, as not only her thighs but her underclothing were copiously stained with it. On the abdomen were a number of lineæ albruantes, indicating previous distention of the belly. We are of opinion that the said Mary Baird or Downie had given birth to a child, and that within ten days of our examination of her. This we certify, on soul and conscience.                   W. B. M'KINLAY, M.D., F.R.C.S.
                              DANIEL RICHMOND, M.D.

This is a true report. I first examined the prisoner, and then said to her she had had a child. She denied it. I afterwards, on the 17th April, made a post-mortem examination of the body of a female child, along with Dr Richmond, and prepared the following report:—

*Paisley, 17th April 1865.*
On Saturday, 15th instant, within the Dispensary of the Paisley Infirmary, we, the undersigned, made a post-mortem examination of the body of a female child, which was produced to us by Robert Hunter, chief constable of Renfrewshire, and Neil Gilmour, sergeant in the Renfrew county police.

The extreme length of the body, which was that of a well-formed child, was 19 inches, while it measured from top of head to umbilicus 10 inches, and from umbilicus to sole of foot 9 inches. The body was covered with earthy clay. The placenta or after-birth was attached, and the cord, which measured 25 inches, passing up from the umbilicus, was wound round the neck three times, and from where it touched the neck to the body of the placenta it measured

one inch and a half.   The part from the umbilicus up to the neck was loose.
A small quantity of meconium had escaped from the anus and besmeared the
buttocks.   The body weighed six and a half pounds.   The nose and front part
of the face were flattened, and three quarters of an inch in front of the left ear
there was a semi-circular abrasion of the cuticle three-eighths of an inch in
length.   The lips were dark coloured, and the tongue protruded from between
the gums.   The chest was fully arched, and, on opening it, the lungs, which
were of a reddish colour, were found to overlap the pericardium and fill their
respective cavities.   The whole of the contents, viz., thymus gland, heart,
lungs, and attachments, were removed, and weighed three ounces and five
drachms.   Upon being put into a basin of water they floated upon the top, the
lungs not only floating freely, but bearing up the others.   Upon the lungs being
separated and pressed under water, air escaped copiously ; and when they were
divided into small pieces, upon each piece being separately pressed under
water, air escaped to the surface.   The heart was of a natural size, and normal
in structure.   On opening the abdomen the liver was found normal in size and
structure.   The stomach was empty.   The other contents of the abdomen and
pelvis were in a normal state, the lower bowels containing a quantity of meco-
nium.   On opening the head, the blood-vessels of the brain were found to be
slightly congested.   The substance of the brain was normal.   We are of
opinion that the said child had arrived at the full period of utero-gestation ;
that it had breathed fully and freely ; and that death resulted from suffocation,
produced in all probability by compression upon the mouth and nostrils.   This
we certify on soul and conscience.            W. B. M'KINLAY, M.D., F.R.C.S.
                                    DANIEL RICHMOND, M.D.

The child was apparently healthy.   We say in the report that death resulted
from suffocation by compression of the mouth and nostrils.   The mark on the
left cheek, the semi-circular abrasion, and also the flattening of the nose, assist
us in this opinion.   I think the cord was wound round the neck most probably
artificially, because from the navel to the neck the cord was loose, and had not
left indentations on the neck ; and had the cord been naturally there, that is
before birth, the child would not have breathed, considering the tightness with
which it was twisted.   If it had been done naturally, the cord would have been
tight between the umbilicus and the neck.   The distance of the after-birth
assists in this opinion.   I should have expected it would have been further
away from the neck of the child.   It was only one and a-half inch from the
neck of the child.   The winding of the cord so close to the neck would be im-
possible in the natural state.   The cord would not be wound naturally round
the neck after birth.   I don't see how it is possible to be done three times.
The child would have required to have made three complete turns after birth.
I do not think it possible, under any circumstances, that the cord should be
wound the whole three times.   *By the Court.*—What do you say to the proba-
bility of the child rolling down a slope.   Is it possible the cord could be wound
by this means ?   Yes ; if the child ran in a slope towards the mother.   It is
possible on a slope, but not three times.   It is not possible if the child roll
*from* the mother.   I have had experience of the cord being wound round the
neck.   Never saw it three times.   Twice is the greatest number I have seen.
I am aware it is stated in our books to have been wound oftener than three
times.   In the present case, on account of the state of the lungs, I don't think
natural winding could be the cause of death.   They were perfectly inflated.
The child must have breathed.   If the cord had been naturally wound, the
lungs would not have been so inflated.   In my opinion, the child was not
killed in birth, but after birth, and after fully and freely breathing—and by
artificial means.   *Cross-examined by Mr Mair.*—It is, I believe, not uncommon
with females seized with the pains of labour to get into an unconscious state ?
—I don't think so.   I know there have been such cases.   For example, the
woman may take a fit ; what we call puerperal convulsions.   I have not, from
my own experience, known of a case where a child partially delivered has died

from compression of the cord before full delivery. I don't know whether this is possible. I can't speak from experience. I have heard that there were such cases. I could not be positive whether I have read or heard of them. *Mr Muir.*—I put into your hands Dr Taylor's Medical Jurisprudence, p. 483, last edition. Please to read the paragraph as to the compressing of the cord. *Witness.*—" When a child is born by the feet or the buttocks, the cord may be so compressed under strong uterine contraction, that the circulation between the mother and the child will be arrested, and the latter will die. The same fatal compression may follow when, during delivery, the cord becomes twisted round the neck. A child has been known to die under these circumstances before parturition, the cord having become twisted round its neck in utero."— (*Med. Gaz., Oct.* 1840, 122.)—I concur in that passage. It does not say any thing about breathing. *By the Court.*—There is another passage about breathing in the book. *Witness.*—When there is a large quantity of fluid in the uterus, the motions of the child turn and twist. What occurs during birth is not twisting, but tightening. Partial respiration may take place after the head of the child comes from the mother. *Mr Muir.*—I show you Casper's Forensic Medicine, New Sydenham Society, Phar. Med. Case, No. 400,[1] as reported at p. 170, vol. iii. Was that a case of head-presentation with the umbilical cord twisted round the neck, and where death took place during delivery ? There has here been partial respiration. One lung has floated. You can't distinguish whether it is by breech-presentation, or head-presentation ? I don't think respiration may be as completely set up in cases of partial delivery as in cases of children actually born. Read this passage from Taylor, p. 457. *Witness.*—" A child may breathe in the uterus or vagina, or with its head at the outlet, and die before its body is born ; the discovery of its having respired would not, therefore, be any sort of proof of its having enjoyed what has been termed extra-uterine life. . . . The death of a child which has respired in the uterus or vagina, from natural causes, before its entire birth, is a possible occurrence." I concur in that ; but the respiration could not be so complete as in the present case. This passage says " fully born ; " but the child might have fully been separated from the mother, and yet the lung only partially inflated. I perfectly agree with the passage. *Mr Muir.*—One word about the cord. You say cases have been known of the child being presented with the cord twisted round the neck. Could it be so twice ? Yes ; it is quite a probable case that the cord may have been twice twisted round the neck before birth,

---

[1] " A mature male child was brought before us (in January) quite fresh, and with the quite fresh umbilical cord, which was thirty-three inches long, coiled round its neck ; the funis was not tied, and its edges were serrated and uneven (torn). The mother was never discovered. The body was seven pounds and three quarters in weight, and twenty inches and a-half in length. Its cranial diameters were rather large, and were respectively three inches and a-half, four inches and a-half, and five inches and a-half. The diameter of the shoulders was also five inches and a-half. There was no trace of injury on the head. There was no proper mark of strangulation round the neck, only on its nape there was a whitish stripe two inches long and three lines broad, not depressed, unecchymosed, and soft to cut. On the right side of the neck there were, close to one another, six excoriated patches, each the size of a pea, bright red, and soft to cut, evidently the marks of finger-nails ; at the angle of the left lower jaw there was a blue and actually ecchymosed patch the size of a sixpence, and on the left cheek another small excoriation like those described. There was nothing remarkable in the abdomen ; the urinary bladder was empty ; the large intestine, however, was full, and the anus bespattered with meconium. The right lung was of a uniform liver-brown, wholly retracted, and it sank completely in water, even to its smallest portion. The left lung, on the other hand, almost covered the pericardium ; was of a bright rosy red, mottled with blue, and gave vent to crepitation and bloody froth on being incised, which was not the case with the right lung ; it also floated perfectly. Within the cranium there was not only a very evident cerebral hyperæmia, but there was also the remarkable phenomenon of an extravasation of dark treacly blood upon the *basis cranii.* There was no evidence of any other particularly external or violent cause of this apoplexy than the coiling of the funis, and none other was required. From the great development of the child, the birth might well be supposed to have been somewhat tedious, and it seemed justifiable to assume, that the external injuries upon the neck and face already described were the result of the parturient woman's own efforts at self-delivery."

and also that the mother, seized with the pains of labour on the upper part of a slope, and becoming unconscious, may, in her state of unconsciousness, have rolled down the slope and over the child. If the mother had done so, the child would in that case have been on the upper part of the slope; but it is impossible, I think, that the child could roll after the mother, and for the cord to get twisted or coiled a third time. That is only my opinion. We don't say in the report that it is not possible, with all the post-mortem appearances of this case, that the death of this infant may not have been caused otherwise than by compression on the mouth and nose. It is quite possible for the child to have died from accidental causes after having been born alive, and still show these post-mortem appearances. Overlaying is the more probable of these accidental causes; and if the woman is unconscious during or after delivery, overlaying is not improbable. The appearances would not have been the same if suffocation had been caused by compression of the cord. The face would have been darker, I think, as you find it in strangling. I don't think it is possible for the cord, twisted round the neck three times before birth—that is, after the child is separated—to become tightened after birth. It is possible for it to be so tightened in consequence of the after-birth being dragged from the mother, so as to cause the death of the child. *To Mr W. A. Brown.*—Leaving out of view the consideration whether the tightening is natural or artificial, we include winding of the cord in the report as the cause of death. Notwithstanding the passage read from Taylor, p. 457, I draw a distinction between partial and complete respiration. And the complete respiration referred to by Taylor is that of children living only a few hours. It is different from that of healthy children. I do not think death occurred by the child being accidentally suffocated during an unconscious birth, because the child would not have breathed. The heart was in its normal state. Fœtal circulation had ceased. Overlaying *might* be the cause of death. I hardly think it possible that the mother may have rolled over the child. If the child had been overlaid by the mother, and she unconscious and the face of the child pressing on the ground, we would have found other abrasions on the outside of the child. *To the Court.*—Abrasions on the back and on the neck of the child. *To Mr Brown.*—I think the abrasion on the left cheek was likely caused by a finger-nail. *To the Court.* —If that had been done in delivery, the woman aiding herself, the scratch would have been in the other direction. *By Mr Mair.*—Necessarily so? Not absolutely so. *To the Court.*—In our report we exclude the suffocation which might be produced by the twisting of the cord. It was not that species of compression which consists in winding a thing round the neck that caused death. I do not think the flattening of the nose might take place during delivery. The head is sometimes hurt, but it is the top of the head, the *caput succedaneum*. The respiration was not partial, but perfectly full and free. No doubt of that. We cut the lungs into small pieces and pressed them under water, and air issued from them.

The next witness called was *Dr Daniel Richmond*, of Paisley, who identified the reports. He was not furthered examined. There was then called for the defence,

*Dr Thomas Johnson*, who was examined by Mr Mair, deponed,—I am a surgeon in Glasgow, and have been in practice for several years. I have had experience in midwifery cases. I have read the report of Drs M'Kinlay and Richmond. The post-mortem appearances described in that report are not incompatible with the death of the child arising from natural or accidental causes. The child might have died during the process of birth, by natural tightening of the cord during birth, and the same post-mortem appearances be present. Or it might have been accidentally overlaid by the mother. This is quite possible, more especially in unassisted delivery. Or, assuming the statement of the mother to be true, the child might have died from the cold of an early April morning; and in this case, I also think the post-mortem appearances would be the same. Yesterday, I looked over an accurately kept record of the weather in Glasgow, and found the thermometer on the 9th of April at 31° in Glasgow,

that is one degree below the freezing point. I should suppose it even lower in the Mearns. Another cause of death might be that the child was naturally feeble, and have died from constitutional weakness. *To the Court.*—The child in the report is said to have been healthy, but examined eight days after death I don't think anybody could judge accurately as to this. *To Mr Mair.*—It is quite possible for the child to have been suffocated after delivery, if it was laid on its face on the grass.—and that accidentally. Any hollow in the ground might have produced this. I have repeatedly found it the case that a woman unassisted in delivery becomes unconscious. In one case, insensibility remained ten hours. That happened within my own experience. The child is said to have been delivered in the open air, and in a field, and the prisoner says that she was unconscious. It is not at all improbable that the child may have died by suffocation by the mother in a state of unconsciousness ; and I think in such a case, with all the appearances described in this report. It is probable that in an unassisted delivery, the female herself may have accidentally suffocated the child before it was completely expelled, and with the same post-mortem appearances. Drs M'Kinlay and Richmond say that three-quarters of an inch in front of the left ear there was a semi-circular abrasion of the cuticle three-eighths of an inch in length. It is quite possible that this may have been occasioned by the mother in the act of self-delivery ; that is to say, the presentation of the head may have been such as to cause this mark. I can't say positively, that assuming that that was not caused by the mother in the act of delivery. it is possible that that abrasion may have been caused by the police officers digging up the child with a spade from the dungstead. *To the Court.*— It is possible to distinguish whether the abrasion was before or after death. *To Mr Mair.*—The nose and front part of the face were flattened. This is very common in cases of first labour, or of labour where there has been a considerable interval. It is possible that the nose may have been flattened by the mother in the act of self-delivery. It is the most likely case of all. if the child after delivery was lying on its face on the grass. It is quite a likely thing that the face may have been flattened by being put into the dungstead after death. It is quite a possible case. if the child presents itself with the cord coiled round the neck, and the mother is seized with the pains of labour, and lies down on sloping ground, and in her unconsciousness rolls over the child, that the cord may have got twisted by the mother rolling over it, or by the child rolling down itself. The report says that the cord was wound round three times, and measured one and a-half inch. and was loose from the umbilicus up to the neck. The circumstance of the cord being loose does not alter my opinion in the least, that the death of the child may have been caused by the tightening of the cord. The circumstance of the placenta being one and a-half inch from the neck of the child, makes it probable that death was caused by accidental compression of the cord. The child is brought so far forward by the process of labour, but enough of compression may have taken place before birth was completed to cause strangulation. It is not unlikely that death may have been caused by cold. If the death of this child had been occasioned by suffocation, the lungs would have been more loaded with blood. I think. Blood, I would have thought, would have been also in the heart, and the congestion of the brain would have been more marked than is spoken to by the reporters. I am of opinion, from the statement of the report as to the placenta having come from the mother attached to the cord, that this was a case of the female having been suddenly seized with the pains of labour. *Cross-examined by Mr Burn Murdoch.*—I question the report on the ground, that the post-mortem examination was made too long after death. I think the difference of seven days renders it much more difficult to say accurately. When I said that the child may have died during the process of birth by tightening of the cord, I quite remembered the completely filled condition of the lungs. There are many cases of this sort on record. I don't remember any in my own experience. The difference between partial and complete respiration is, that the one is only a part of the whole. And quite full enough respiration might have

taken place to produce the appearances described in this report. I don't see why strangulation by compression of the cord should produce a greater amount of congestion of the brain, than by suffocation of the mouth. If the child had died of cold there would not necessarily have been internal congestion. There is quite enough of congestion spoken to. On the supposition that the prisoner had been insensible, it is quite likely that she should have been well shortly after. She might be insensible from five minutes to ten hours. And I see no reason why, if she had been unconscious, she should not walk about after coming to consciousness. *To the Court.*—In my practice I have had a case of the cord twisted three times round the neck, but never with the free end so short as in the report—that twisting being prior to birth. *To Mr Burn Murdoch.*—One of the modes I say death was possible, was by the tightening of the cord during birth, assuming it to have been naturally twisted round the neck. If that were the case, the effect of tightening would be to stop the breathing. And if so, it is possible that there would be the appearances of full respiration, provided the child was sufficiently far advanced to have breathed. I think the child had breathed, and after doing so was strangled during delivery. But in my opinion it is quite a likely case that death was truly caused by the tightening. I have frequently had cases of respiration begun during birth, but not cases altogether identical with this. But such cases are spoken to in our books over, and over, and over again. There is a case in Casper, I think, where the child was strangled by the tightening of the cord, and where there was a full respiration as in this; and another in a German publication brought to my notice last night by Dr Simpson.

*Dr Alexander Russel Simpson*, examined by Mr Mair.—I am a doctor of medicine and surgeon in Glasgow, a fellow of the Society of Physicians in Glasgow, and have had considerable experience in midwifery cases. Before coming to Glasgow I acted for about seven years as assistant to my uncle, Professor Simpson of Edinburgh. I have heard the report read, and to my mind there is nothing in that report incompatible with the death of the child arising from natural or accidental causes. Assuming the death of the child to have been caused by suffocation, it is quite possible that that may have been produced by compression of the umbilical cord being naturally coiled three times round the neck during delivery or after delivery. It is not only possible but entirely likely in the case as reported, where the placenta measured an inch and a half from the neck of the child. There are many such cases on record. It does not alter my opinion as to the cause of death, nor does it enter into the consideration of the question, that the cord was found loose after death from the umbilicus to the neck of the child. Strangulation may have taken place notwithstanding. To describe how suffocation may have been produced during delivery, when the placenta was an inch and a half from the neck of the child, there are two points I would have to think of. First, that the cord was put upon the stretch and compressed, so that the circulation of the blood would be stopped; and we know that compression of the cord makes it necessary that the child should feel desirous to respire. When the head of the child comes into the world it would make such an effort to respire. Then another pain or two going on, the end would be still further tightened round the neck of the child, and that impeding, the inspiratory effort of the child would be fruitless. The air could not get down into the lungs. In this case the post-mortem appearances might be precisely the same as in the report. It is possible, but not likely, that the flattening of the nose may have been caused accidentally by the female in the act of self-delivery. *Cross-examined by Mr Brown.*—The tendency of the tightening of the cord is to render respiration first necessary, and then more difficult. The tighter it becomes the more impossible it is for the child to respire. If the child had met its death in this way I don't see any reason why I should not have expected to find in the report a statement that respiration was complete; because the child was vigorous, and would make vigorous inspiratory efforts. The precise distinction between partial and complete respiration is very much a question of degree. Where the head of the

child has come up, but the child has not fully come into the world, is one of those conditions in which sometimes we meet with partial respiration.  *To the Court.*—Partial respiration is the same as imperfect respiration.  *To Mr Brown.* —I think the language of the report would be equally applicable to a child that had breathed, but had not been fully born as to one fully born.  We cannot say whether or not it is the common case that the child respires as freely before as after it has come into the world, because there is generally some medical attendant by, or some old woman wise enough to relax the cord.  One would expect if the child were freed from every obstructing circumstance round the neck, inspiration would be more perfect.  I have heard that in this case there was a certain amount of congestion of the brain.  If strangulation had been caused by tightening of the cord not artificially, would you not have expected greater congestion?  We can never predicate what we would expect as to what is a greater or what is a less degree of congestion.  One may think congestion much which another would think little.  The precise significance of the distance of the placenta being an inch and a half from the neck is this, that the placenta attaches to the interior of the womb while the head is being put forward, and that the strain begins and increases with the advance of the head.  The report speaks of the child having been a healthy child.  It would depend on the condition of the circumstances in which the child was placed, whether there would or would not be any difficulty in pronouncing to that effect upon a body which had been dead seven days.  *To the Court.*—If it was in a dunghill ten inches down I would expect the witness to be able to say that the child was a healthy child.  I am not prepared to say that I would have expected general internal congestion.  *To Mr Mair.*—A dungstead is not a favourable place to preserve a child.  If there were ashes it would be.  *By the Court.*—Is it pretty well ascertained that one full respiration during birth may so fill the air-cells of the lungs as to induce the appearances here represented?  I can only say that it might.  It is not a point on which, so far as I know, experiment has given decision.  I am not aware that there is any direct fact to show how far one full respiration may go.  It is always a question of probabilities.  There is no evidence in the report that there was any cause of death during birth, unless it was the tightening of the cord.  I am aware that the medical gentlemen who subscribed the report say that the child was suffocated and not strangled.  I do not think there is any sufficient means of post-mortem examination for ascertaining whether death was caused by strangulation or suffocation.  Overlaying by the mother might produce this.

Mr Burn Murdoch having addressed the jury on behalf of the Crown, and Mr Mair on that of the prisoner, Lord Ardmillan then summed up, and the jury, after a short deliberation, unanimously found the charge Not Proven. The pannel was then dismissed from the bar.

---

## THE CHOLERA IN PARIS.

### By Dr Jules Guérin.

Though at first very cautious in speaking of the cholera, we feel that now that that disease is fairly installed in the capital, we may speak of it openly and without any reserve.  But before describing the actual state of the epidemic, it may be useful to explain our reasons for acting as we have done. At the beginning of an epidemic, and while it is still uncertain whether it will cease or go on progressing, it is prudent not to give rise to premature disquietude.  But when the evil is at its height and threatens to continue so, it would be an illusion to believe that silence or an attenuation of facts can long deceive the public.  Indiscreet revelations, and, above all, the increasing number of funerals would soon dissipate any such illusion.  The imagination, instead of stopping at the reality, exaggerates facts, and multiplies them far above what they really are.  It is this which happened in the epidemics of

1832, 1849, and 1853–54. People begin by saying, as at present, it is no great thing; then, as the great increase of mortality became apparent to all, the authorities had no other way of moderating the panic than declaring the whole truth, publishing every day the number of patients admitted into the hospitals and the number of deaths. This measure, besides putting an end to all exaggeration, has another advantage; it forces the public to keep on its guard. Now that we know that the cholera gives warning several days before its attack, more attention is paid to the prodromata; they are treated, and the patient is cured. When, on the contrary, there is a false confidence, the early stage of the disease passes unperceived, and the aggravation of the malady soon dissipates the deceitful security. It is better, then, to warn the public frankly, to tell them exactly how the matter stands, especially when we can temper the alarm arising from a knowledge of the danger, by indicating the means for avoiding it. If it is true that cholera is raging with intensity; it is no less true that it can almost always be checked in its development by combating the diarrhœa with which it commences. Silence as to the sanitary condition of the city has another disadvantage; it may encourage the absent to return to the capital, and so expose them to a danger which might have been avoided; accordingly, while waiting for information from the authorities, we have collected the following which we believe to be exact.

Since the first of this month the epidemic has gone on constantly increasing. In the first few days there were from 30 to 40 cases admitted into the hospitals, and from 20 to 25 deaths. The admissions gradually rose to 100, and the general mortality to 150 to 200 a-day. On the 10th, the number of admissions was 137, and the total number of deaths 194. During the following days the figures remained nearly stationary, but the epidemic went on spreading and becoming more general. Thus, while the admissions to the hospital Lariboisière barely reached the number of 15, those of the Hôtel-Dieu, the Charité, and the St Louis, much exceeded that number, at least in regard to the first of these establishments; thus, on the 12th, 33 cases of cholera were admitted into the Hôtel-Dieu, 19 into the Charité, 10 into the St Louis. It is the same with the military hospitals. Unfortunately, the epidemic has broken out in the barracks, so that on the 11th, 4 cases were sent from the Napoléon barracks to the Val-de-Grace; and on the 12th, 28 cases were admitted into that hospital. There is therefore no possibility for concealment; the cholera is established in Paris; but, let us hasten to add, that there is good reason to hope it will not attain the proportions of former epidemics.

Having described the amount of the evil, let us recall the methods of stopping and preventing the disease. The authorities have taken wise measures; the troops are marched out, and the barracks are aired and cleaned. This is good; but it is not enough. It ought to be made universally known that diarrhœa is the prelude to cholera, and that by treating it the development of cholera is prevented; daily inspections should be ordered, if this has not been already done; finally, the regiments should be scattered as much as possible, and the proportion of numbers to localities diminished. The same is true as to all masses of individuals, workshops, even hospitals. Then we consider it an evil to assemble a large number of cases in one locality, such as a special ward. The hospital authorities have no doubt done well in not admitting cholera patients into wards containing others, but it is very desirable that provisional hospitals should be established round Paris, so as to spread the cases as much as possible. It has always been noticed that the mortality has been greater in the neighbourhood of large hospitals. The neighbouring population would therefore gain by not having around it great centres of infection. This is why we have always protested against the system of large hospitals, against central hospitals, if not against the hospitals themselves. To descend from public establishments to families, it is desirable that, in the absence of domiciliary visits, the authorities should publish short directions, and that these should be widely distributed. Every head of a family should exercise a strict superintendence over his own household, and this would be equivalent to the domiciliary

visits which have been so useful in England. A recommendation which cannot be too much insisted on, is the cleaning of public water-closets. Powdered charcoal, sulphate of iron, chlorine water, quick-lime, are the proper means for destroying the choleraic emanations accumulated in these localities.

With regard to the treatment of the premonitory diarrhœa, when it has once shown itself, the following is the method of treatment which may be considered as the product of the most enlightened experience. From the very first, food should be altogether abstained from; two or three cups of weak tea with a little cognac or rum; in the evening of the first day, a clyster with some laudanum; next morning, two glasses of Seidlitz water, followed by the clyster. These means, each of which has a reason for being in the place and the order indicated, are generally sufficient to arrest and dissipate the prodromata. We think it right to insist upon the association and the order in which they should be administered. The most generally received opinion is, that cholera is a kind of poisoning. The toxic agent introduced into the system tries to eliminate itself by the gastro-intestinal canal, the great emunctuary of the economy. The preliminary diarrhœa is a first manifestation of this effort at elimination. We must favour this initial effort by starvation and rest, by slightly stimulating drinks. When the intestine is freed from all residual alimentary matter, the abnormal movements may be quieted by opiates. But if we were satisfied with quieting this movement by stopping it, we should run the chance, as the saying goes, of shutting up the wolf in the sheepfold. This is not a mere hypothesis; several patients, treated exclusively by opiates, have had relapses, and finally a real attack of cholera. It is proper, therefore, to aid the expulsion of the poison; this may be done without danger by saline purgatives. We say without danger: the experience of the three preceding epidemics has given the proofs of it to those physicians who have not allowed themselves to be stopt by vain apprehensions of adding to an intestinal irritation which did not exist. This is certain, and never to our knowledge has any inconvenience arisen from a saline purgative seasonably administered. There is a form of prodromata which is usefully combated by ipecacuanha; this is, when along with the diarrhœa, there is nausea and vomiting. In this form ipecacuanha stops the dispositions of the stomach which are the analogue, and have no doubt the same signification as the intestinal excretion.

The last scientific communications to our two Academies have taught us nothing which deserves to be brought prominently forward. We may except only a sign pointed out by our learned colleague, M. Gibert, as helping to distinguish the choleraic from other species of diarrhœa. In the choleraic diarrhœa, the tongue is flat, large, moist, whitish, and cold; in bilious or irritative diarrhœa, the tongue is elongated, pointed, more or less red at the sides and at the tip. This distinction, of which experience will indicate the value, appears to us to rest on the nature of things; it answers perfectly to the idea we entertain of the atonic depressing character of cholera.—*Gazette Médicale de Paris*, 14th October.

---

## THE CHOLERA MORBUS IN EGYPT.

### By Prince J. ZAGIELL, Physician in Cairo.

IN the month of March 1865, the cholera showed itself on the banks of the Ganges, its perpetual bed, and, during twenty-two days, without leaving the country, assumed the epidemic character; then, acted on by a south-west wind, it began to move, dividing itself into two atmospheric columns. The one advanced towards Arabia; accompanying the Indian pilgrims as far as Mecca and Medina, taking from among them, during their journey, a large number of victims. The other, not less terrible, advanced by Affghanistan towards Cashmere and Bokhara, and from thence forced a passage into the Russian provinces of Asia, and into Russia in Europe, accompanying the caravan of merchants.

The south-west column only remained fifteen days in Arabia, because the south wind (Khamsin) and the dry tropical heat of Arabia Felix were unfavourable to the development and persistence of the scourge. Then, the epidemic, entering the ranks of the Egyptian pilgrims, accompanied them into their country as it had followed the Indian pilgrims. Its first halt was at Alexandria, a damp city, situated between the sea and the lake Mareotis, surrounded by immense marshes, where it remained for three weeks, and committed great ravages. Greedy of new victims, it spread into the towns and villages of Egypt, spreading everywhere mourning and death.

The epidemic remained at its height in Alexandria for twenty-three days; whilst in Cairo, and in the other towns of Egypt, where the air is drier, it only lasted twelve days. At the end of this period the epidemic began to diminish day by day.

*Atmospheric Causes of Cholera.*—All are agreed that there is something peculiar in the condition of the air during an epidemic of cholera, and to this the word Miasma is given. Let us inquire, in the first place, what is the chemical and physical composition of miasmatic air. According to MM. Schœnbein, Boekel, and Wolf, there is absence of ozone. Is there not rather an excess of carbonic acid? During the last epidemic I made numerous examinations for ozone in Syria, at Beyrout, and the ozonometric paper—prepared according to Schœnbein's method—became of a very deep blue colour during the period of greatest mortality. At Cairo and at Choubral, in the house of Prince Halim-Pacha, the ozonometric paper assumed a similar colour at the last period of the disease, when the mortality was only three a-day. I am therefore led to believe that ozone has nothing to do with the miasmatic character of the air during an epidemic of cholera.

The atmosphere at Cairo, and in the neighbourhood, is excessively dry; and on all sides, for a space of twenty square leagues, there is a complete absence of marshes. If we seek in miasmata the causes of the epidemic, we ought, above all, to know of what materials the miasmatic element is composed. Why does the epidemic follow a current of air? Why does a quarter of the town to which the current is directed suffer more than others? Why are damp seasons, a low-lying country, the northern hemisphere, marshy, dirty, confined, ill-aired localties, especially hospitals, more favourable to the development of cholera, than dry places in a tropical climate, and their having an altitude of 1200 to 3000 feet on calcareous mountains? To this question (which is a mystery to science) we may reply (according to my theory) that, undoubtedly, low, damp situations, dirty, ill-aired localities, cellars, prisons, etc., where the air contains at least 4 per 1000 of carbonic acid (a half-fermented atmosphere), give birth to different species of animalcules, of infusoria, a certain and incontestible cause of the epidemic in some countries; an epidemic which persists till the air is purged of this infection.

In 1849 and 1852 I observed in Russia that in all the low and damp localities—in the hospitals, in the prisons—the epidemic reigned with such violence that the greater number of persons inhabiting these nests of miasmata were attacked by the disease; whilst in higher situations, in sandy soils, in towns and villages surrounded by fir-trees, the epidemic, baffled, as it were, was almost powerless, to such a degree that, during the presence of the cholera during two years in these countries, several towns, situated as described, were entirely free from it. Another example: the cholera remained in Petersburg for four years. Why? Because this town is built on marshes, and is completely surrounded by them. In my opinion, then, the principal cause of epidemic cholera is an atmosphere where there is excess of carbonic acid, and where is developed a certain kind of animalcules different from all those which are found in the atmosphere when normally constituted.

These animalcules, absorbed by the respiratory passages, poison, more or less, all persons breathing the air when the epidemic commences to spread, and augment in consequence of the mortality which they, in their turn, increase.

The progress of cholera follows the direction of the wind. Sailors, according

to Ehrenberg, often meet with a kind of dust containing the remains of eighteen species of infusoria, with siliceous envelopes, at the distance of 380 nautical miles from the coast of Africa, and at the height of Cape de Verde. It is an historical fact that the ashes of Vesuvius have been carried to Venice and to Greece. In 1794, the ashes of Vesuvius enveloped in a thick cloud the extremity of Calabria, a distance of fifty leagues. In 1766 (according to Oloffen), the ashes of Mount Hecla produced such a degree of darkness at Glaumba, a town at the distance of fifty leagues from the volcano, that the inhabitants walked about with difficulty. The ashes of the volcano of Consiguina, in Guatemala, were carried in January 1835 as far as Jamaica, a distance of more than 800 miles. De Candolle, during his residence on the coast of Brittany, habitually exposed to south-west winds, says that he found on the trees at Quimper-Corentin two lichens, the Stricta crocata and the Physica flavicans, which had never before been found except in Jamaica. Are not all these examples proofs that molecular miasms, obeying the same force of locomotion, may be thus transported from one end of the world to another by the force of a current of air, with a special tendency to follow the north-west direction? and it is worthy of remark that the direction of the epidemic has never been observed to be contrary to this current. In this I am at one with the observations of M. Marc d'Espini. We know that a wind barely sensible moves at the rate of a little more than two miles an hour, a gentle breeze at about five, a very strong breeze at twenty-five, a violent gale at forty. We can therefore understand the different degrees of activity with which an epidemic may pass from one locality to another.

Why does the epidemic in its course pass over without attacking certain towns and villages? The reason is very simple; it is because the molecular miasmata, propelled by a violent wind, are retained at a distance from the ground, in exactly the same manner as locusts, which, carried by the wind, traverse certain localities without committing the least ravages. It is on this account that we always see the first attacks of the epidemic manifest themselves during the night, because at that time in general the wind falls, the molecules descend to the earth, and poison during the calm of sleep the individuals who inspire them. Why, too, does the epidemic not develop itself on mountains above the height of 3000 feet? Because, on the one hand, the diminished atmospheric pressure is opposed to the existence of miasmatic animalcules; and on the other, because above 1200 feet air is almost free from carbonic acid, which is essential to their development.

There is another observation worthy of remark, which I made in Russia in 1849 and 1852, and this year at Beyrout; it is, that during the presence of cholera there is a certain special, as it were, aromatic smell (odor cholerica); probably this smell depends upon the presence of organic matters in a state of suspension in the vapours of the air, and produced by the molecular matters, carried away by the water volatilized by the solar heat.

*Is cholera contagious?* According to my observations, cholera cannot be communicated by the touch; it cannot be transmitted from the sick to the healthy, either by individual contact, or by touching the clothes of the patient; and I maintain that the disease is exclusively communicated by the special air brought by the epidemic. The morbific principles are not of the nature of a virus, but depend upon molecular miasmata diffused through the atmosphere, which poison by contact with the mucous membrane of the respiratory passages, and with the cutaneous system. Cholera is transmitted from individual to individual independently, up to a certain point of atmospheric conditions. The places where the pathogenetic principles are disengaged, are the centres which act only on the individuals within their sphere of activity, and in whom there already exists a special predisposition, accompanied by normal influences which favour the action of a contagious miasm. Then, individuals once attacked by this disease, become themselves centres of infection, pouring into the atmosphere the animalcules capable of transmitting the epidemic to other individuals equally predisposed. Therefore, it is not by the

contact of individuals that cholera poisons, but by altering the surrounding air which is breathed by the individuals inhabiting infected localities.

*Treatment.*—1*st, Preventive.*—In order to ward off attacks of cholera, a careful regimen should be adopted ; in particular, green fruits should be avoided, as well as an excess of vegetables. Water should not be drank unless mixed with a small quantity of some alcoholic fluid, such as brandy, but especially rum ; at meals, wine should be drunk, but in moderation ; cold baths should be avoided, flannel should be worn, currents of air within doors should be avoided ; during the day the house should be well aired by opening the windows, which should be closed during the night.

2*d, Treatment during the period of Incubation.*—At this period I have, in Russia in 1849–52–53, at Beyrout and Egypt in 1865, been completely successful by employing the following remedy :—Acetate of lead, vinous extract of opium, of each 6 grains; extract of guaco, extract of monesia, of each 60 grains ;[1] syrup of ginger, a sufficiency to make a mass to be divided into 24 pills. One pill to be taken every hour till the effect is produced. In general it is not necessary to administer more than five pills in order to stop the choleraic diarrhœa. When the action of these pills has not been quite satisfactory, I have ordered from 15 to 20 drops of the following preparation, to be taken every half-hour in half a cupful of infusion of balm :—Tincture of guaco, laudanum, tincture of nux vomica, ethereal tincture of amber, of each half an ounce.

3*d, Treatment of the Disease when confirmed.*—In the period of confirmed cholera I order the following :—Essence of cajuput, essence of chamomile, of each 15 minims ; sulphuric ether, half-an-ounce ; ammoniated tincture of valerian, Fowler's arsenical solution, of each 60 minims. From 12 to 15 or 20 drops to be taken every ten minutes in a tablespoonful of hot infusion of balm, until reaction takes place. I order the following external application against cramps :—Tincture of piper longum, ethereal tincture of mustard, spirit of ammonia, of each an ounce ; veratrine and aconitine, of each 8 grains.

The limbs are to be rubbed briskly with this by means of flannel, till the cramps completely disappear. At the same time the patient should drink a warm infusion of peppermint, strongly aromatized with *Eau de Mélisse de Carmes.*[2]

In fine, according to my experience, the most satisfactory treatment of cholera is by means of stimulants, astringents, mild narcotics, and antispasmodics. By these means I have obtained the most brilliant and most satisfactory results. Under this treatment the intestinal mucous membrane is strengthened ; the decomposition of the blood is arrested ; the transudation of the serum of the blood and coagulation in the large vessels are prevented ; the semi-paralyzed vaso-motor nerves are stimulated ; the circulation of the blood in the peripheral vessels, and, at the same time, the animal heat are restored. —*Gazette des Hôpitaux,* 10th October 1865.

---

## MEDICAL STUDENTS.

FROM a return just made for the information of the Inspector of Anatomy, it appears that there are no less than 1014 gentlemen pursuing their professional studies at the eleven metropolitan hospitals, exclusive of those studying for the Dental Profession ; of the former number 321 have commenced this session. The number of students at the different hospitals varies very much ; one school has a class of 249, whereas another has only 30, the number of new entries at the former amounting to 80, and at the latter to 9 only. It is stated that about 20 students are pursuing their studies for the diploma of Dental Surgeon. —*Medical Times and Gazette.*

[1] *Guaco,* derived from Mikania guaco, a composite plant ; *Monesia* from a species of chrysophyllum, a native of Brazil. Both plants are somewhat bitter and astringent, and might no doubt be replaced by any others having similar properties.

[2] The formula for this well-known French preparation is,—leaves of fresh balm, three handsful ; fresh lemon peel, nutmeg, coriander seed, cloves, of each an ounce; strong white wine, rectified spirit of wine, of each a pint and a half. To be allowed to macerate for twenty-four hours, and distilled over a sand bath.

## ROYAL COLLEGE OF SURGEONS.

At a meeting of the Royal College of Surgeons of Edinburgh, held on the 18th October, the following office-bearers were elected for the ensuing year:—

*President*, James Dunsmure, M.D. *Secretary*, James Simson, M.D. *Treasurer*, John Gairdner, M.D. *Librarian*, Archibald Inglis, M.D. *President's Council*—James S. Combe, M.D.; Andrew Wood, M.D.; Robert Omond, M.D.; Benjamin Bell; James D. Gillespie, M.D.; James Spence. *Ex-officio*, John Gairdner, M.D. *Examiners*—James Simson, M.D.; Richard Huie, M.D.; William Dumbreck, M.D.; Archibald Inglis, M.D.; Andrew Wood, M.D.; Robert Omond, M.D.; James Dunsmure, M.D.; Peter D. Handyside, M.D.; James D. Gillespie, M.D.; Henry D. Littlejohn, M.D.; Patrick H. Watson, M.D.; David Wilson, M.D. *Assessors to Examiners*—James S. Combe, M.D.; James Syme; William Brown; James Spence. *Conservator of Museum, and Registrar of Students' Tickets*, William R. Sanders, M.D. *Officer*, John Dickie.

---

## PUBLICATIONS RECEIVED.

Aitken.—Science and Practice of Medicine. By Professor William Aitken, M.D., etc. Fourth Edition. London, 1865.

Anderson.—Contributions to Dermatology. No. III. Psoriasis and Lepra. By T. M'Call Anderson, M.D., etc. London, 1865.

Annandale.—The Malformations, Diseases, and Injuries of the Fingers and Toes, and their Surgical Treatment. By Thomas Annandale, F.R.C.S. Edin. Edinburgh, 1865.

Bennet.—Winter in the South of Europe: or, Mentone, The Riviera, Corsica, Sicily, and Biarritz. By J. H. Bennet, M.D., etc. London, 1865.

Boismont.—Du Suicide et de la Folie Suicide. Par A. Brierre de Boismont.

Browne.—Epileptics: their Mental Condition; A Lecture. By W. A. F. Browne.

Chambers.—Lectures, chiefly Clinical. By Thomas K. Chambers, M.D., etc. Fourth Edition. London, 1865.

Ewart.—Descriptive Catalogue of Pathological Preparations in the Museum of the Medical College, Calcutta. London, 1865.

Gamgee.—Amputation at the Hip-Joint. By J. Sampson Gamgee, Surgeon. London, 1865.

Hargrave.—Ligature of the Left Common Iliac Artery. By W. Hargrave, M.B., etc., Dublin.

Higginbottom.—The Use of the Nitrate of Silver in the Treatment of Inflammation, Wounds, and Ulcers. By John Higginbottom, F.R.S., etc. London, 1865.

Hinds.—Clinical Memoranda. By William Hinds, M.D., etc. No. I. Birmingham, 1865.

Hunter.—Speedy Relief of Pain and other Nervous Affections by means of the Hypodermic Method. By Charles Hunter, Surgeon, etc. London, 1865.

Invalid.—San Remo as a Winter Residence. By An Invalid. 1862-65. London, 1865.

Lee.—Health Resorts of the South of France. By Edwin Lee, M.D., etc. Second Edition. London, 1865.

Lee.—Nice, and its Climate. By Edwin Lee, M.D., etc. Second Edition. London, 1865.

Madden.—Climate of Malaga in the Treatment of Chronic Pulmonary Disease. By T. M. Madden, M.D. Dublin, 1865.

Navy, Statistical Report of the Health of the, for the year 1862.

Nevins.—Prescriber's Analysis of the British Pharmacopœia. By J. Birkbeck Nevins, M.D., etc. Third Edition. London, 1865.

Peacock.—Valvular Disease of the Heart. By Thomas B. Peacock, M.D., etc. London, 1865.

Penny and Adams.—On the Detection of Aconite by its Physiological Action. By F. Penny, F.R.S.E., and James Adams, M.D., Glasgow.

Pollock.—Elements of Prognosis in Consumption, with Indications for the Prevention and Treatment. By James E. Pollock, M.D., etc. London, 1865.

Price.—The Diseased Conditions of the Knee Joint which require Amputation of the Limb, and those Conditions which are favourable to Excision. By P. C. Price. Edited by Henry Smith, F.R.C.S., etc. London, 1865.

Report of Coroner's Inquest on David Bell. Jamaica, 1865.

St Bartholomew's Hospital Reports, Vol. I. London, 1865.

Taylor.—Principles and Practice of Medical Jurisprudence. By A. Swaine Taylor, M.D., etc. London, 1865.

Warter.—Observation in Medicine; or, The Art of Case-Taking. By J. Southey Warter, M.D. London, 1865.

# Part First.

## ORIGINAL COMMUNICATIONS.

ARTICLE I.—*Hydatid Tumours of the Liver: their Dangers, their Diagnosis, and their Treatment.* By CHARLES MURCHISON, M.D., F.R.C.P., Joint-Lecturer on the Practice of Medicine at Middlesex Hospital, and Physician to the London Fever Hospital.

(*Read before the Medical Society of London, 16th October 1865.*)

MODERN research has shown that the development of hydatid tumours in the human body is due to the presence of a parasitic animal,—the Tænia echinococcus. This echinococcus is developed from the ovum of a tape-worm, and is itself an imperfect tape-worm. The ova of the tape-worm find their way into the alimentary canal through the medium of either diseased meat or drinking water, and, penetrating the coats of the gut, they become encysted in the surrounding parts. The liver is the organ into which they most commonly enter; in most instances the liver is the only organ in which they are developed; and in the great majority of cases in which other organs are implicated, there is evidence that the liver was first attacked. This rule is not without exceptions. An hydatid tumour may be developed in the spleen, in the omentum, in the areolar tissue of the pelvis, and even in the mamma or in one of the extremities, and yet the liver may be free.[1]  Practically, how-

---

[1] Two of the chief authorities on entozoa, Davaine and Cobbold, have given the following statistics as to the localities of hydatids in the human subject. Dr Cobbold's statistics are founded on cases recorded in the English journals.

|  | Davaine. | Cobbold. |
| --- | --- | --- |
| Liver, | 165 | 51 |
| Lungs, | 40 | 8 |
| Kidneys and bladder, | 30 | 7 |
| Peritoneum and intestines, | ... | 19 |
| Pelvis, | 26 | 3 |
| Brain and spinal cord, | 25 | 17 |
| Muscles of trunk and extremities, | 20 | 5 |
| Bones, | 17 | 10 |
| Mamma, uterus, and ovary, | 12 | 2 |
| Orbit and globe of eye, | 12 | 1 |
| Heart and pericardium, | 10 | 9 |
| Face, mouth, and neck, | 9 | 1 |
| Male organs of generation, | 3 | ... |
| Pulmonary vessels, | 2 | ... |
| Placenta and fœtus, | 1 | 1 |
| Supra-renal capsules, | 1 | ... |
| Blood, | ... | 1 |
|  | 373 | 135 |

It is probable, however, that the liver was also implicated in many of the

ever, the exceptions are few ; and it is chiefly with hydatids ori-
ginating in the liver that the physician is concerned.

In some parts of the world, hydatid tumours are extremely
common.   In Iceland, Eschricht has calculated that about one-sixth
of the entire population are afflicted with them ; and according to
Dr Hjaltelin, of Reykjavik, a seventh part of the human mortality
is caused by hydatid disease.   In this country the disease is fortu-
nately less prevalent.   Out of 2100 post-mortem examinations re-
corded at the Middlesex Hospital between April 19, 1853, and
August 25, 1863, hydatids were found at only thirteen, or once in
161 cases ; and in only seven of the thirteen cases, or once in 300
cases, can they be said to have occasioned the fatal event.[1]   Still,
even in Britain, hydatid tumours are probably more common than
is generally believed, or than might be inferred from the above
calculation, and this is because, in a large proportion of cases, the
tumour attracts little notice before it bursts, or is about to burst,
and then the person often dies suddenly.   My experience of dis-
pensary and out-door hospital practice leads me to believe that
hydatid tumours occur oftener than might be concluded from the
observation of autopsies of patients dying in hospital.

The latent manner in which an hydatid tumour is developed in
the liver is one of the chief peculiarities of its clinical history.   As
a rule, it gives rise to no symptoms, and causes little inconvenience,
until it has attained such a size as to press upon adjoining organs,
or until it excites inflammation of the super-imposed peritoneum
preparatory to bursting.   This peculiarity is remarkably illustrated
by the cases detailed in this paper.   It follows, as a matter of
course, that a small hydatid tumour is rarely diagnosed during
life.

In reference to practice, it is important to have a correct know-
ledge of the natural modes of termination of hydatid tumours of the
liver.   There can be no doubt that many of these tumours undergo
a spontaneous cure.   In consequence of the entrance of bile (Cases
xvi. and xviii.), or of inflammatory action, or of calcification of the
outer cyst preventing its further growth, the echinococci die, the
cyst shrivels up, and its contents are converted into a pultaceous or
putty-like material, in which the hooklets of the echinococci may

cases where other organs are stated to have been the seat of the disease.   The
following are references to examples of hydatid tumours in different parts of
the body, where none existed in the liver :—In the pelvis, Case x., and also
Path. Trans., xi. 155 ; in the pleura, Case xii. ; in the pericardium or heart,
Path. Trans., vi. 108, and x. 80 ; in the supra-renal capsule, *Ib.* xv. 224 ; in
the spleen, heart, and brain, Charcot and Davaine, Gaz. Méd. de Paris, 1862.

[1] This estimate as to the frequency of hydatids is possibly slightly under the
mark, as, in certain cases, where the examination was confined to the obvious
cause of death, a small hydatid may have escaped detection.   The particulars
of the thirteen cases (vii. to xix. included) are given below.   In seven cases
(viii., ix., x., xiii., xiv., xvii., and xix.) death was obviously due to the hydatid ;
and in two (x. and xi.) of the remaining six cases, the hydatids possibly excited
the fatal pleurisy.

be discovered years after every other trace of the original structure has been destroyed. Tumours in the liver answering to this description are met with in the dead-house. But, unfortunately, this favourable result is confined for the most part to hydatids of so small a size that they are not recognised during life. When the tumour is sufficiently large to give rise to symptoms, and be diagnosed, such an event is so rare that it cannot be calculated upon. The tumour then continues to increase in size. Its growth may be slow ; it may extend over years ; but almost as surely as the tumour grows will it one day burst, unless it previously cause death by interfering with the functions of important organs, as happened in Cases x. and xiv. The directions in which an hydatid tumour of the liver may burst are very various, and are chiefly the following :—

1. *Into the Cavity of the Chest.*—This direction is more common than any other. When the contents of the hydatid are discharged into the pleura,[1] or in very rare cases into the pericardium,[2] acute pleurisy or pericarditis is at once excited, and death is the almost invariable result. If adhesions form between the diaphragm and the base of the right lung prior to the bursting of the hydatid, the contents of the latter may escape by the bronchial tubes, and the patient may recover ;[3] but even here, in most cases, fatal inflammation is set up in the lung,[4] or the patient dies from exhaustion, owing to the profuse discharge from a large cavity, or from many small cavities, excavated in the lung.[5] Fatal pleurisy may result from an hydatid tumour of the liver, without any perforation of the diaphragm ;[6] and again, acute pleurisy appears to be sometimes excited by the development of hydatid tumours on the upper surface of the right side of the diaphragm, independently of any hydatids in the liver (Case xii). From Case iv. also, it would seem that an obsolete hydatid cyst of the liver may inflame, and, after establishing a communication with the bronchial tubes, may give rise to all the phenomena of gangrene of the lung.

2. *Into the Peritoneum.*—The tumour collapses, and violent and almost always fatal peritonitis is at once excited.[7] This accident

---

[1] See Cases viii. and xiii. ; also Frerichs, Dis. of Liver (Syd. Soc. Ed.), ii. 235 ; Ogle, Path. Trans., xi. 299 ; Bristowe, Path. Trans., iii. 341 ; H. Davies, Path. Trans., i. 278.

[2] See case by Wunderlich, Med. Times and Gaz., Nov. 12, 1859, p. 488.

[3] For examples, see Bright, op. cit., p. 49 ; Todd, Med. Times and Gazette, Jan. 5, 1854 ; Path. Trans., iv. 44 ; v. 303 ; viii. 92 ; ix. 28.

[4] See case by Peacock, Path. Trans., ii. 72 ; Pollock, *Ib.*, xvi. 155.

[5] Frerichs, op. cit., ii. 264 ; Peacock, Path. Trans., vol. xv. p. 247.

[6] See Case x., and case by Dr Pollock, Path. Trans., v. 301.

[7] See case by Dr Burrows, Med. Times and Gaz., 1855, i. 111 ; and by Dr Brinton, Lancet, 1854, i. 124. Bright, however, records a case where what appeared to be a large hydatid tumour of the liver burst into the abdomen, without being followed by a fatal result (op. cit., p. 47). Ogle also mentions the case of a patient who recovered after the symptoms of peritonitis resulting from the rupture of an hydatid cyst in the omentum (Path. Soc. Trans., xi. 295).

must not be confounded with the attacks of partial peritonitis, which are so common before the tumour bursts in any direction. The rupture of the sac is often caused by external violence, in the form of a blow, fall, or strain. In the museum of St Mary's Hospital is the calcified cyst of an hydatid taken from the body of a man who dropt down dead after receiving a slight blow on the epigastrium from a comrade with whom he was sparring. The blow ruptured the cyst; the contents of the cyst escaped into the peritoneum, and the man died from shock. Three cases of fatal rupture in consequence of a fall are recorded by Mr Cæsar Hawkins.[1] Three similar cases are mentioned by Frerichs; in two the rupture was caused by a fall, and in the third it was due to a strain; in one of the cases, death occurred within a quarter of an hour of the rupture.

3. *Through the Abdominal Parietes or Lower Intercostal Spaces.*— This is not a common mode of termination, although several cases are on record. The contents of the hydatid may be discharged by an opening at the umbilicus or in some other part of the abdominal parietes, or in one of the lower intercostal spaces,[2] and the patient may get well. Even here, however, the cyst is apt to take on suppuration, and the patient may die from exhaustion; or fatal hæmorrhage may occur from the interior of the sac, as in a case recorded by Dr Bright.[3]

4. *Into the Stomach or Intestine.*—This is the most favourable direction in which the tumour can burst, although death sometimes results from the peritonitis which is set up around the opening, or from secondary abscesses of the liver,[4] and unfortunately it is not a common mode of termination. According as the tumour opens into the stomach or the intestine, the hydatids are vomited or evacuated *per anum ;*[5] sometimes they escape in both directions. The opening is usually small, so that the hydatids are discharged slowly and at long intervals.

5. *Into the Bile-ducts.*—This is so rare a mode of termination that some writers have expressed doubts as to its occurrence. The contents of the tumour may be discharged through the bile-duct into the bowel, and recovery may take place, but a contrary result is much more common. In either case, jaundice will probably be induced. The bile entering the cavity may excite suppuration and convert the tumour into an abscess. A remarkable case is recorded by Dr Hillier, where an hydatid tumour opened into a bile-duct, and where the patient died in consequence of hæmorrhage from the wall of the cyst, the blood passing along the duct into the stomach and

[1] Med. Chir. Trans., vol. xviii., p. 124.

[2] Budd, Dis. of Liver, 3d ed., p. 443; Frerichs, op. cit., ii. 237; Hawkins, Med. Chir. Trans., xviii. 163.

[3] Bright, op. cit., p. 50.

[4] See a case under Dr Owen Rees, Med. Times and Gaz., June 20, 1857.

[5] For examples, see Frerichs, op. cit., ii. 237; Budd, op. cit., p. 452; Bright, op. cit., p. 49.

intestines.[1]   Two cases are recorded by Dr Wilks, where an hydatid cyst opened into a bile-duct, but where death was caused by peritonitis ;[2] in one of the cases, hydatid cysts were vomited and passed from the bowel before the occurrence of inflammation.   Frerichs mentions a case where most of the contents of an hydatid had escaped by the bile-duct, but where the common duct ultimately became obstructed, and fatal rupture of the gall-bladder was the result.[3]   Case ii. is a rare example of recovery after the discharge of the contents of an hydatid cyst through the bile-duct into the bowel.   The recovery appeared to be complete ; but, several months afterwards, the passage of some of the remaining contents of the tumour along the duct gave rise to severe pain and vomiting, and the efforts to vomit tore across some of the old adhesions : the result was fatal peritonitis.   In rare cases, an hydatid tumour is developed in the bile-duct.   Dr Dickinson has recorded the case of an hydatid developed in the right hepatic duct, where obstruction of the common duct was caused by a portion of the cyst together with inspissated bile.[4]

6. *Into the Vena Cava Inferior.*—In exceptional cases, an hydatid tumour of the liver bursts into the inferior vena cava, and its contents, reaching the right side of the heart, become impacted in the pulmonary artery, and cause instant death.   Three cases of this sort are mentioned by Frerichs.[5]

But, independently of rupture, there are various ways in which an hydatid tumour may destroy life.

1. *By pressure upon important organs and interference with their functions.*—An hydatid tumour of the liver may compress the vena cava so as to cause anasarca and varices of the lower extremities,[6] or the portal vein, so as to induce ascites, and necessitate recourse to paracentesis.[7]   An hydatid tumour of the pelvis behind the bladder may compress the bladder and ureters, and induce pyelitis and fatal uræmia, as happened in Case x.,[8] or it may compress the rectum so as to produce gangrene of the gut and fatal constipation.[9]   Sometimes an

[1] Path. Trans., vii. 222.        [2] Path Trans., xi. 128.        [3] Op. cit., ii. 231.
[4] Path. Trans., xiii. 104.
[5] Op. cit., ii. 238.   It is a curious fact that crystals of hæmatine are occasionally found in the interior of hydatid cysts, the source of which cannot be discovered.   See two cases in Path. Trans., iv. 166, and v. 304.
[6] A case of this sort is recorded by Dr Habershon, in Guy's Hospital Reports, 3d ser., vol. vi. p. 182.
[7] See a case by Dr Barker, Path. Trans., vol. vii. p. 225, and by Dr Budd, Dis. of Liver, p. 451.
[8] See also Case xvii.   This appears to be a not uncommon situation for hydatid tumours.   Two cases are recorded by Dr Bright (Abdom. Tum., Syd. Soc. Ed., p. 41), and one by Dr Habershon (Path. Soc. Trans., xi. 155).   In one of Dr Bright's cases, death was caused in the same way as in Case x.   For other instances of hydatid tumours in the same locality, see Path. Trans., v. 311 ; Med. Times and Gaz., 1855, i. 159 ; and 1860, Ap. 7.   The pyelitis in Case iii. was probably due to the pressure of the enlarged liver.
[9] See case by Obre, Path. Trans., v. 302.

hydatid tumour of the liver becomes so large that it distends the entire abdomen, and by pressure upwards interferes with the function of respiration. This result was observed in Case xiv., which was further remarkable from the circumstance that an enormous fluctuating swelling filled the front of the abdomen, while the epigastrium and both flanks were tympanitic on percussion.

2. *By suppuration of the cyst, or external to the cyst, and pyæmia.*— Many cases of this sort are on record.[1] In some of the cases, the blood appears to become directly contaminated, owing to a communication between branches of the hepatic vein and the suppurating cavity.

3. *By the formation of secondary hydatid tumours.*—Secondary tumours may form in the liver or mesentery;[2] and, if they be large or numerous, they may interfere with the patient's nutrition, and cause death by exhaustion. Not uncommonly they form in the lung, and destroy life by inducing pneumonia. Case xx. is an instance of a secondary hydatid tumour compressing the spinal cord and causing paraplegia.[3] Dr Barker relates the particulars of a case where death was due to the formation of a secondary hydatid tumour in the brain.[4] An interesting case is recorded by Dr Wilks, of a girl, aged 19, who died suddenly, having previously been in good health. An hydatid tumour was found in the liver, and another at the apex of the left ventricle of the heart. The latter had burst and discharged a loose hydatid into the cavity of the left ventricle.[5]

From the above remarks it is obvious that the risks to which a person with a large hydatid tumour of the liver is liable are many, and that the chances of his escaping them are few. If the tumour attain a size sufficient to be recognised, there is little probability of its undergoing what is called a "spontaneous cure." If the tumour be let alone, it is almost certain, in the long run, to burst in one direction or another; the act of bursting is almost inevitably fatal, except the opening take place into the digestive canal or a bronchial tube, and even then the accident is very far from being devoid of danger; and, lastly, there are many ways, independently of bursting, in which a large hydatid tumour may destroy life. It is true that the tumours may remain stationary for years, and the patient

---

[1] For example, see Bright, op. cit., p. 37 ; Budd, op. cit., p. 444 ; and Frerichs, op. cit., ii. 245.

[2] See cases recorded by Bright, op. cit., pp. 13, 23, and 30 ; Jones, Path. Trans., v. 298 ; Peacock, *Ib.*, xv. 247 ; Gibb, *Ib.*, xvi. 157.

[3] Another case of an hydatid tumour of the spinal column pressing on the cord is recorded by Dr Ogle, Path. Trans., xi. 299.

[4] Path. Trans., x. 6.

[5] *Ib.* xi. 71. See also *Ibid.* xv. 247. Cases of hydatid tumours of the heart, without any implication of the liver, are recorded by Habershon (Path. Trans., vi. 108), and Budd (*Ib.* x. 80). In Budd's case, an hydatid tumour at the apex of the heart had burst, and loose hydatids were found in the right ventricle and in the pulmonary artery.

may enjoy tolerable health, but it is no less true that he is in constant risk from one of the accidents which have been described, and that he can never calculate on a moment; although apparently in good health, a strain or a slight blow on the abdomen may cause instant death. It becomes, therefore, a matter of great importance to determine what are the resources of medical art for averting these great and many dangers.

In the first place, then, it must be confessed that little or no dependence can be placed on any medicinal agent for effecting any change in the size or in the structure of an hydatid tumour. Among the many remedies which have been proposed, common salt and iodide of potassium are the two which have been most relied on for destroying the life of the hydatid, but there is no evidence that either the one or the other is endowed with such a property. It is difficult to conceive how chloride of sodium can be unfavourable to the growth of an hydatid, when it is remembered how large a quantity of this salt is contained in the fluid contents of the cyst, and that, therefore, it must be compatible with, if not necessary to, the healthy existence of the parasite. And with regard to the preparations of iodine, there is not only no proof of their power to destroy the life of the parasite,[1] but there is positive evidence that the iodine does not reach it. Frerichs was unable to discover a trace of iodine in the fluid of an hydatid cyst, removed from a woman who had taken iodide of potassium for many weeks, and a similar observation was made in Case i.

But although medicines be of little or no avail, there is happily one expedient which holds out a fair chance of effecting a permanent cure, and that is puncture of the cyst and removal of its contents. It is now many years since hydatid tumours of the liver were tapped by Sir Benjamin Brodie, and the patients made a good recovery.[2] Successful cases were afterwards published by Dr Bright,[3] and by many other observers. It is only of late years, however, that the operation has been often resorted to, and even still it is very doubtful if most practitioners would not prefer leaving the patient to the very uncertain chances of a spontaneous cure, or would limit the operation to cases where the tumour is of a size rarely attained. The fears expressed are not unnatural, for in not a few cases the operation has been followed by dangerous symptoms or even death. The dangers of the operation are mainly two, viz.: 1. Acute peritonitis owing to the escape of a portion of the hydatid fluid into the peritoneal sac; and, 2. Suppuration of the cyst owing, in some degree, to the admission of air. But both of these dangers may be in a great measure avoided by employing a very fine

---

[1] The following are references to instances in which iodide of potassium was thought to have effected the cure of an hydatid cyst. Med. Times and Gaz. 1860, Ap. 7, p. 344, and 1855, Feb. 17; Lancet, Oct. 16, 1858:—In one, at least, of the cases, the disappearance of the tumour appeared to be due to its having burst.

[2] Med. Chir. Trans., vol. xviii.            [3] Op. cit.

trocar.[1] The dangers in question have mainly arisen in cases where an opening has been made with a scalpel or a large trocar, on the mistaken supposition that it was necessary to remove the secondary cysts as well as the liquid, or because the tumour was thought to be an abscess. It is now known that the removal of the liquid, which is as thin and limpid as water, suffices to destroy both the parent hydatid and its offspring. The subsequent injection of such substances, as alcohol, iodine, or bile, is quite unnecessary, and may be injurious. As additional precautions, it is well to remove the canula before all the fluid has been drawn off, and in doing so to press the punctured portion of the abdominal wall against the cyst. For forty-eight hours the patient ought to be kept in the recumbent posture, and every movement of the body be strictly prohibited; and if there be the slightest pain, an opiate ought to be administered.

The patient often experiences immediate relief from the sensation of tension and other unpleasant symptoms, from which he may previously have suffered. Sometimes the operation is followed by a feeling of uneasiness in the tumour, or by considerable pain and constitutional disturbance, but if the above rules be attended to these symptoms soon pass off and the patient makes a good recovery. The fluid does not re-accumulate as happens when an ordinary serous cyst is tapped, and the hydatid cysts do not undergo putrefaction, a circumstance which has been attributed to their strong impregnation with salt.

The safety of the operation may now be regarded as established. In Australia and in India, I have been informed that the operation is resorted to with almost uniform success. Of 135 hydatid tumours in different parts of the body (many of them in the liver), collected by Dr Cobbold from the English Journals, 35 were cured by surgical operations. Of 20 cases of hydatid tumours of the liver in which paracentesis was performed, collected by myself,[2] all recovered but three; and in one of the three cases, death was due to a miscarriage; in the second, to three other enormous cysts in the liver; and in the third (Case vi.), the sac was inflamed, and the patient was almost moribund when the operation was performed. The tedious suppuration of the cyst in several of the cases was probably due to the large size of the opening.

A careful consideration of the whole matter, of the dangers of the

---

[1] Dr G. Budd further recommends, with the object of preventing the entrance of air, that the fluid be drawn off by means of an exhausting syringe, adapted to a fine canula. See Med. Times and Gaz., May 19, 1860, p. 494.

[2] These cases are as follows:—Three cases recorded by Mr Cæsar Hawkins, Med. Chir. Trans., vol. xviii. p. 119; two cases by Dr Bright, op. cit.; one by Owen Rees, Guy's Hosp. Rep., ser. ii., vol. ii. p. 17; case by Mr Holthouse, Med. Times and Gaz., Jan. 6, 1855; case by Mr Birkett, Lancet, Oct. 16, 1858; case by Dr G. Budd, Ib., May 19, 1860; case by Mr Cock, Ib., 1855, i. 57; three cases by Dr Garrod, Dr Goolden, and Dr Willshire, Lancet, Sept. 1, 1860; case by Dr Hjaltelin, Path. Trans., xiv. 176; three cases by Mr Hutchinson, Lancet, Oct. 11, 1862, and Brit. Med. Jour., Feb. 20, 1864; case by Frerichs, op. cit., ii. 267; cases i. and vi. of this paper.

disease when left alone, and of the inutility of medicines on the one hand, and of the success hitherto obtained from the operation on the other, leads to the practical conclusion that, in all cases where an hydatid tumour is large enough to be recognised during life, and is increasing in size, it is well to puncture it at once. If the tumour appear to be diminishing in size, it may be well to wait, but it is unnecessary to wait for the formation of adhesions, or to endeavour to induce them. An hydatid tumour is not prone to form adhesions over its outer surface, like an abscess. By the time that adhesions form in the natural way the tumour has attained a large size, and is probably eating its way into some of the adjoining cavities; its walls also are much less elastic than at an earlier stage, and a puncture through them will close up less readily, so that there is a greater risk of fluid escaping into the peritoneum after removal of the canula, if the adhesions be not sufficient to prevent it. While the walls are still elastic, the opening may be expected to close immediately that the instrument is withdrawn, so that the existence of adhesions is unnecessary. In the 14th volume of the Pathological Transactions (p. 133), I have recorded a case which has an important bearing on this matter. A distended sacculus of the urinary bladder was punctured through the abdominal parietes with a small trocar, and eight ounces of urine were drawn off. The patient had no bad symptom consequent on the operation, but died four days afterwards from other causes. After death there was no increased vascularity or exudation in any part of the peritoneum, and in fact it was impossible to discover any trace of the puncture. It is obvious that not a drop of urine could have escaped into the peritoneal cavity. By using a fine trocar, there is no reason why the liquid contents of an hydatid cyst should not also be prevented escaping.

But before having recourse to puncture, the diagnosis of an hydatid tumour ought to be tolerably certain. Its peculiarities are, that it is a smooth, globular, elastic, painless tumour, which grows slowly, without giving rise to any constitutional disturbance, and without, as a rule, causing jaundice or ascites. One other character is thought to be of great diagnostic value, and that is " hydatid vibration." This sign—which is usually associated with the name of Piorry, but to which the attention of physicians was first called by M. Briançon, in 1828—is a peculiar trembling sensation, which is felt most distinctly when three fingers of the left hand are laid flat upon the tumour, and the middle one then struck abruptly with the middle finger of the right hand, or by using a pleximeter, and allowing the point of the percussing finger to rest upon it for a few moments after the stroke. The value of the sign, I am inclined to think, has been greatly exaggerated; in many cases it is undoubtedly wanting. When present it indicates, in my opinion, the existence of a cyst which is in all probability hydatid, but it does not indicate, as some have thought, the existence of secondary cysts in the interior.—(*Vide*

Davaine, Recherches sur le Frémissement Hydatique, Gaz. Méd. de Paris, 1862.)

The diseases which are most readily confounded with hydatid of the liver are abscess, distended gall-bladder, effusion into the right pleura, aneurism, and cancer.

An abscess of the liver may resemble an hydatid tumour in size and shape, but it is distinguished by its more rapid development, by the co-existence of rigors, fever, and pain, and by impairment of the general health. A suppurating hydatid, or one with peritoneal inflammation of its outer surfaces, may give rise to the same symptoms as an abscess, and then a diagnosis can only be formed by an accurate knowledge of the previous history.

A distended gall-bladder may closely resemble a pendulous hydatid of the liver, and may also be free from pain. It is recognised by its shape and position, by its development being usually accompanied by attacks of biliary colic, and by the fact that in most cases there is jaundice, owing to the obstruction being in the common duct.

Extensive effusion into the right pleura, with bulging of the ribs, and obliteration of the intercostal spaces, may closely simulate a large hydatid tumour; but, on the whole, an hydatid tumour of the liver is more likely to be regarded as an example of pleuritic effusion, than pleuritic effusion mistaken for hydatid. The hydatid is mainly distinguished by its insidious growth, and by the absence of constitutional symptoms. The chief physical distinction is derived from the upper boundary of the dull space. In pleuritic effusion this is horizontal; in hydatid tumour it is arched, being at a higher level in the infra-axillary space than it is close to the spine or near the sternum. The possibility of an hydatid tumour of the liver co-existing with pleuritic effusion must not be lost sight of.—(See Cases viii. and xiii., and p. 495.)

An aneurism of the abdominal aorta, or of the hepatic artery, may present a smooth, globular tumour, very like that of an hydatid. Its main distinctive characters are pulsation, bellows-murmur, and the fact that it is usually the seat of acute neuralgic pains, owing to pressure on the branches of the solar, or of the hepatic, plexus. An aneurism of the hepatic artery is further distinguished by its being invariably accompanied by jaundice from compression of the bile-ducts.

Cancer of the liver is mainly distinguished by its irregular surface, tenderness and hardness, and by the absence of elasticity or feeling of fluctuation. The diagnosis may be embarrassed by the circumstance that several hydatid tumours projecting from the surface of the liver may impart to it an uneven surface, or that the nodules of medullary cancer may exhibit a degree of elasticity approaching to fluctuation. Under such circumstances, the diagnosis of hydatid must mainly depend on its slower growth and on the absence of constitutional cachexia.

In cases where a soft fluctuating tumour of the liver can be felt,

but where there is some doubt as to the diagnosis, owing to the obscurity of the history, or to other circumstances, there can be rarely any objection to making an exploratory puncture with a grooved needle or a fine trocar. If the tumour turn out to be hydatid, it can be emptied at once, and a cure is likely to follow : if it be not, there is little chance of harm resulting. It would not be difficult to adduce cures where even cancerous tumours and aneurisms have, under such circumstances, been punctured with impunity. It is to be borne in mind that the fluid which escapes from an hydatid tumour, even if it contain no echinococci or shreds of hydatid membrane, will reveal its nature with absolute certainty. If the sac be not inflamed, it is limpid, has a specific gravity of 1007 or 1009, and contains no albumen, but throws down a copious precipitate with a solution of nitrate of silver, owing to its strong impregnation with common salt. These characters apply to no other fluid in the body, whether healthy or morbid.[1]

Lastly, it is necessary to add that even a large hydatid tumour of the liver may occasionally escape detection, if, for example, it project downwards from the posterior part of the liver. (See Case ii.) Again, the diagnosis must always be difficult when the tumour compresses or obstructs the bile-duct or the large veins, so as to give rise to jaundice, ascites, or anasarca of the lower extremities.

CASE I.—*Hydatid Tumour of the Liver, threatening to burst. Paracentesis. Recovery.*

On 3d August 1864, Hannah S., a very nervous woman, æt. 31, consulted me about a tumour in the region of the liver. She was a cook in a medical man's family. In the summer of 1863 she had been laid up for three weeks with a pain across the stomach ; but, with this exception, she had never suffered from any symptom of abdominal disease until about nine weeks before she came to me. She was then seized suddenly with acute pain in the region of the liver, which lasted for about two hours. For several days she vomited after everything she ate, and she had great pain in her side when she attempted to cough or to turn in bed. She kept her bed for a week, and did not resume her work until after three weeks. The liver was then first observed to be enlarged and prominent, but the patient was unable to say whether this enlargement had existed before the attack of pain or not. On examination, there was found to be a slight bulging in the right hypochondrium below the ribs, this bulging being apparently continuous above with the liver, extending to half an inch below the umbilicus, and, transversely, from one inch to the left of the mesial line to about 3½ inches to the right. The vertical hepatic dulness corresponding to the right nipple was 7 inches, 4¼ inches of the dull space being below the edge of the ribs. The tumour was tense, but elastic, and almost fluctuating. It was slightly tender on deep pressure. It did not appear to be adherent to the abdominal wall. Posteriorly, the hepatic dulness did not extend higher than usual, and its upper margin was horizontal. The respiratory sounds at the right base were normal. The patient was slightly sallow, but had no decided jaundice. The tongue was clean, the appetite good, and the bowels regular. There was no ascites and no anasarca ; the urine contained neither albumen nor bile-pigment. Pulse 84.

---

[1] The contrast between the fluid contained in the hydatid cysts described in Case xix., and the fluid in the surrounding peritoneal fluid, in which they were floating, is worthy of notice.

On 7th August, the patient had a return ot pain in the tumour, accompanied by vomiting and purging, lasting for two or three days. For several days after this attack the tumour was tender, and over its surface coarse friction could be both heard and felt during the respiratory movements.

On 19th August, Hannah S. was admitted under my care into Middlesex Hospital, and placed on iodide of potassium, five grains three times a-day.

On 24th August, the tumour was noted as more tense and tender. On the night of 2d September, patient had an attack of acute pain in right side, greatly increased by pressure, movement, or a long inspiration, and accompanied by much nausea, but by no vomiting or rigors. Pulse 96. Under the use of opium, poultices, and rest, these symptoms gradually subsided, but the tumour continued tender, the friction was again distinguishable for several days, and the pulse did not fall below 96. On 9th September, the patient had another similar attack of pain, but more severe ; the pulse rose to 116, and the friction returned. On 14th September, the pain was less, but the tumour was observed to extend more to the right side, and was less rounded. On 17th September, there was another severe attack of pain ; and indeed, since 24th August, the tumour had never been free from tenderness, while the patient felt herself gradually getting weaker, the pulse being rarely below 108.

Although there was no evidence of firm adhesions over the tumour, it was now determined to puncture it. From the first, the tumour had been diagnosed as an hydatid, and indeed the object of the patient's admission into hospital was to have it punctured. All who examined it were agreed that it contained fluid, and the only other affections at all likely to produce the appearances observed were a distended gall-bladder and an abscess of the liver. The tumour did not occupy quite the situation, and latterly did not present the shape, of a distended gall-bladder, and there had never been jaundice. The persistent pain and tenderness noted for several weeks pointed rather to abscess, but there had been no rigors or perspirations, and, moreover, the tumour had not increased much in size since it was first observed. Supposing the tumour to be hydatid, there was reason to fear that it was about to burst.

On 20th September, my colleague, Mr Hulke, tapped the tumour with a fine trocar, the cannla of which was scarcely so large as a No. 1 catheter, and drew off about twelve fluid ounces of clear limpid fluid, the specific gravity of which was 1009. No echinococci or hooklets could be discovered in it, but it was found to contain a large amount of chloride of sodium and no albumen. Neither did it contain a trace of any salt of iodine, although iodide of potassium had been taken almost continuously for several weeks.

On removing the canula, the abdominal parietes were pressed down against the tumour, and the puncture was covered with collodion. The patient was kept on her back for forty-eight hours, and not permitted to move on any consideration. Twenty drops of laudanum were administered immediately after the operation, and for two days an opiate was given about once in four or six hours.

The night after the operation, the patient slept well. On the following day, the urine was retained, and was drawn off by catheter; and, on 22d September, the abdomen was greatly distended and tympanitic, the skin hot and dry (temperature 101°), the pulse 120, and there was much thirst. Still there was much less pain and tenderness over the tumour than before the operation. The bowels had not been open for two days. An enema of turpentine and confection of rue brought away a large quantity of flatus, and the patient at once began to improve. On 26th September, the pulse was 96, the tongue clean and moist, and the appetite was returning. For the first time for several weeks, the patient could tolerate free manipulation of the tumour, the dimensions of which were much reduced. On 27th September, pulse 84. The collodion was removed from the wound, from which not a drop of discharge had escaped. On 30th September, the patient was able to get up. Convalescence was retarded by an attack of facial rheumatism and other trifling ailments; but, on 22d November, the patient was able to leave the hospital. The dimensions of the tumour were.

gradually diminishing, so that the dulness from the upper margin of the liver to the lower end of the tumour did not exceed 5¾ inches. The tumour also was quite soft and free from tension, and could be manipulated without causing pain. The tongue was clean and moist, the appetite and digestion good. Pulse 100.

*3d June* 1865.—The patient has now almost regained her ordinary health. For the last four or five months she has been in service as a cook, and she can walk several miles without feeling fatigued. There is still a fulness in the region of the tumour, but the lower margin of the liver is quite an inch above the umbilicus. The patient is also sensible from her clothes of a great diminution in the girth of the lower part of her chest, and she is also able to wear stays, which she has not been able to do for nearly twelve months.

Case II.—*Hydatid Tumour of the Liver, bursting into the Bile-duct. Jaundice. Discharge of innumerable Hydatid Membranes per Anum. Recovery. Attacks of Biliary Colic from passage of Cysts remaining in Liver through the Bile-duct. Rupture of old Adhesions of Liver during act of Vomiting. Peritonitis. Death.*

On the 29th of October 1861, I was consulted by Mr G. W., a solicitor, æt. 53. For some weeks he had been suffering from flatulence and a feeling of tightness and oppression after meals, and three days before he had been attacked with severe pains in the abdomen, resembling colic. The countenance was somewhat sallow ; the motions were pale but contained bile ; there was no bile in the urine, which was scanty and dark, having a specific gravity of 1027, and depositing much lithic acid. The vertical hepatic dulness in the right mammary line extended about an inch below the edge of the ribs, and all along the right hypochondrium there was slight tenderness on pressure. Pulse 64. His digestion had always been good except once about seven years before, when he had several attacks of colicky pain in the abdomen, similar to those from which he had recently suffered. The medicines prescribed by myself and afterwards by Dr Watson, who met me in consultation, failed to give any relief.

On 24th November, the patient had an attack of vomiting, followed by an aggravation of the dyspeptic symptoms, and by increased tenderness in the right hypochondrium.

On 6th December, he was much worse. The tenderness in the right side had increased greatly, and there was also constant pain there, which became very acute when he took a long breath or coughed. The tongue was furred and moist. The bowels were very costive, and there was considerable tympanitic distension of the abdomen and increased sallowness, but no sickness. The pulse had risen to 88, and the respirations were 30, and thoracic. Fifteen leeches were applied to the seat of pain ; twelve more on 8th December, and eight more on 10th December, with poultices in the intervals, and the bowels were kept open by castor oil and turpentine enemata.

On 12th December, the pain was much less, but there was still considerable tenderness and a stitch in the right side on taking a breath or coughing. The countenance was very sallow, but there was no decided yellowness of the conjunctivæ, and the motions, though pale, contained bile. The vertical hepatic dulness in the right mammary line was five inches. Nothing like a defined tumour could be felt, and there was no bulging of the ribs. The breathing at the base of the right lung was normal. Pulse 88.[1]

On 16th and 17th December, the patient passed, for the first time, several hydatid cysts in a bilious motion.

On the 18th December, he was much worse. There was decided jaundice of the integuments ; the urine was loaded with bile-pigment, and there was not a trace of bile or of hydatid membranes in the motions. There was a constant pain in the right side, in addition to occasional paroxysms, like colic ; lips parched ; tongue furred ; much perspiration in the night, and great prostration.

[1] From this date until 25th January, I attended Mr W. in conjunction with Mr F. Davies, of Upper Gower Street.

Pulse 100. The treatment consisted in the constant application of poultices to the side, and in the administration of quinine, and of blue pill and opium.

*19th Dec.*—Is much easier. Has passed innumerable hydatid vesicles, from a pin's head to an orange in size, per anum. Skin and urine still jaundiced, and no bile in stools.

*20th.*—Fæces to-day are tinged with bile, and still contain numerous hydatid cysts.

*21st.*—Jaundice almost gone. Motions still contain hydatids and abundance of bile. Below and to the left of the right nipple, there is tympanitic percussion over a space the size of a crown-piece. Both above and below this there is hepatic dulness. Pulse, 88; pain much less; tongue cleaning.

The patient continued to pass a few hydatid vesicles with each motion up to 31st December, and the tympanitic percussion sound was noted a few days later than this. Had occasional sharp but temporary attacks of pain in the abdomen, resembling colic. On January 6, 1862, he was quite convalescent: pulse 72. The tympanitic sound noted above could no longer be distinguished, and the upper border of hepatic dulness was an inch lower than before. At the end of January, Mr W. was able to drive out; and, on 19th February, he went to Ventnor for change of air, returning to London on 11th March.

Once, while at Ventnor, he had a severe attack of colicky pain, lasting for an hour and a half, and "bending him up double." He had a similar attack, but less severe, a few days after his return to London. Both attacks were unaccompanied by vomiting. Every day he gained strength, and on his return to town he was able to resume his business. On the 2d of April, he went down to Essex on business. He walked about the country several miles every day, feeling none the worse, and returned to town on 6th April.

On 8th April, he went to his business as usual, and walked several miles. Shortly after dinner, about 7 P.M., he was suddenly seized with severe pain in the abdomen, which returned in paroxysms, and this time was accompanied by vomiting. There was slight tenderness at the epigastrium, but no jaundice. The pulse was only 84. Repeated doses of opium and chloric ether were prescribed, and poultices were kept constantly applied over the abdomen.

On the following day, the paroxysms of pain had ceased, but there was more tenderness at the epigastrium and in the right hypochondrium, and considerable pain when he coughed or moved. The vomiting had not quite ceased. There was slight sallowness, but the stools contained bile. Pulse 86. Ten leeches were ordered to be applied to the side, and the poultices and opiates were to be continued.

The patient did not apply the leeches, as he felt better. In the afternoon, he had two severe attacks of rigors, after which he felt so much better and free from pain that he thought it unnecessary to send for me.

On the morning of 10th April, he said that he felt so much better that he had eaten a good breakfast, and wished to get up and go down stairs; but he was in a state of extreme prostration, and evidently sinking. The pulse was 120 at the elbows, and imperceptible at the wrists. The sickness had ceased, but the features were pinched, and the skin was cold and covered with clammy sweat. He gradually sank, and died at 8 P.M.

*Autopsy.*—The abdomen only was examined. On opening this cavity the intestines appeared healthy, but distended with gas. There was no exudation or increased vascularity in the general cavity of the peritoneum. The large intestines contained a quantity of pulpy material of the colour of cream, and without any tinge of bile. The small intestines contained bile.

The left lobe of the liver was healthy and non-adherent. Both the upper and under surfaces of the right lobe were connected to the adjoining parts by firm adhesions. Near the right edge of the liver a few of the bands of adhesion fastening it to the ribs appeared to be ruptured, and at this point there was a patch of recent lymph not larger than a square inch, with slightly increased vascularity round about. In the substance of the right lobe was an irregularly-shaped, collapsed cavity the size of a large orange. The walls of this cavity

were partly formed by the ribs and the surrounding adhesions. The inner sur-
face of the cavity consisted of indurated hepatic tissue, presenting a shreddy
appearance, and was not lined by any hydatid membrane. The cavity was
almost empty; but it contained four or five collapsed hydatid vesicles about
the size of a shilling. Communicating with it was a greatly dilated bile-duct,
passing directly on to the common duct. The entire duct, from the cavity to
the orifice in the duodenum, was large enough to admit the tip of the little
finger. Further back in the right lobe, and quite distinct from the cavity now
described, was another, about the size of a plum, which was lined by an obsolete
and cribriform hydatid cyst, presenting a tough opaque yellow appearance.
The contents of this cavity had escaped during the hurried division of the liver.
(This tumour was probably the source of the symptoms from which the patient
had suffered seven years before his death.)

CASE III.—*Hydatid Tumour of the Liver. Pyelitis. Pus in the Urine.
Sudden Death.*

Ellen C., æt. 21, came under my care as an out-patient at the Middlesex
Hospital, in April 1861. She stated that for about eighteen months she had
been getting very weak and losing flesh, and that latterly she had suffered from
dyspnœa. She had no cough, but her father had died of consumption. She had
also suffered from irregular menstruation and leucorrhœa. On examining the
chest, there was found to be a bulging of the right side commencing at the
upper border of the fifth rib, attaining its maximum at the false ribs, and then
as gradually declining. The hepatic dulness in the right mammary line extended
for three inches below the margin of the ribs, and its total length was $6\frac{1}{2}$ inches.
The bulging below the ribs occupied the right hypochondrium and epigastrium,
and extended over to the left hypochondrium. It was somewhat tender, and
presented an elastic, almost fluctuating consistence, and on percussion com-
municated to the finger the peculiar sensation known as "hydatid vibration."
These characters were most marked in the epigastrium. The superficial veins
about the epigastrium and hypochondrium were much enlarged. The move-
ments of respiration were mainly confined to the left side of the chest. On the
right side, the respiratory murmur could not be heard below the fourth rib in
front, or below the lower angle of the scapula posteriorly. Above this the
breathing was harsh, and the expiration was prolonged. On the left side there
was also dulness and absence of respiration up to within half an inch of the
lower angle of the scapula. The patient could give no information as to
the length of time the tumour had existed. In fact, she was quite ignorant of
the existence of any unusual swelling until it was pointed out to her. Her
complexion was slightly sallow, but she had never suffered from jaundice or
vomiting, and her bowels were regular; her appetite was very bad. In addi-
tion to the tumour on the right side, a painful swelling, apparently an extension
of the left lobe of the liver, could be felt in the left lumbar region in the
situation of the kidney, and there was a copious discharge of pus in the urine.
    The patient remained under my observation for nearly twelve months. The
dimensions of the tumour did not alter much, but, on the whole, they became
slightly larger. From time to time she suffered from severe pain in the swell-
ing in the left lumbar region. At these times the urine was clear, or almost so,
and relief was always attended with a sensation of bursting and a return of the
pus in large quantity. The urine was repeatedly examined with the microscope,
but no pus-casts or trace of echinococci could be discovered.
    The treatment—which consisted in the administration of tonics and iodide of
potassium, and the external application of iodine—failing to give relief, the
patient was admitted into the hospital on 14th January 1862, with the object
of having a puncture made into the tumour in the right hypochondrium. After
remaining in the hospital for six weeks she refused to give her consent, and
was discharged at her own request.
    I did not see the patient after this; but I have ascertained that, on 6th No-

vember 1863, she was admitted into University College Hospital, under the care of Dr Hare, to whom I am indebted for the particulars noted while she was under his observation. Towards the end of 1862, she had first suffered from pain in the region of the tumour in the right hypochondrium. The pain was intermittent in its character, ceasing after a few days. For this she had been treated at the Female Hospital in Soho Square. The dimensions of the tumour noted in University College Hospital showed that it had increased considerably. Although the right costal angle was still greater than the left, there was bulging of the ribs on both sides as high as the nipple, and dulness on percussion up to the third rib on the right side, and up to the third intercostal space on the left side. The heart was displaced upwards, its apex beating in the third left intercostal space. The vertical hepatic dulness in a line with right nipple was $11\frac{1}{4}$ inches; in the median line, $9\frac{1}{2}$ inches; and in a line with left nipple, $9\frac{3}{4}$ inches. Distinct fluctuation could be felt in the epigastrium over a space measuring $4\frac{1}{2}$ inches transversely, and $2\frac{1}{2}$ inches vertically; but there was no hydatid fremitus. There was no œdema of the legs. The patient was sallow, her urine contained no bile-pigment, but was still loaded with pus. She still suffered from the attacks of pain in the region of the left kidney, which were always relieved by a sensation of bursting and a copious discharge of pus in the urine. On admission, there was a considerable amount of pain and tenderness in the region of the tumour near the umbilicus. This pain recurred from time to time, but was always relieved by leeches, poultices, and morphia. The patient also had an attack of pain and stiffness in the left groin and knee, accompanied by enlargement of the lymphatic glands in the groin, and slight œdema in the upper part of the thigh. On 26th January 1864, it was noted that she was free from pain, but that she had lost flesh and strength. On 9th February, she was discharged for unruly conduct.

The patient was confined to bed after leaving the hospital, and died rather suddenly and unexpectedly at the end of ten days. An hour before death she seemed tolerably well, and the probability is that the fatal event was due to the bursting of an hydatid cyst.

CASE IV.—*Old Hydatid (?) Tumour of the Liver, communicating with Base of Right Lung. Lobular Pneumonia and Gangrene of the Lung.*

Robert J., æt 72, was sent to the London Fever Hospital, 21st August 1864, as a case of "fever." On examination, he was found not to be suffering from any form of idiopathic fever. The man stated that he had had a bad cough for two months, and had kept his bed for two days. His breath had a most decidedly gangrenous odour. His sputa were of a dirty greenish muco-purulent character, and extremely fetid. Dry bronchial râles were audible over the chest, and at the right base there was slight dulness, increased vocal resonance, and large moist râles, but nothing approaching to cavernous breathing. Pulse 96; respirations 36. No change took place in the physical signs of the chest, but the tongue became dry and brown, diarrhœa supervened, and the patient gradually lost flesh and strength until death on 11th September.

On *post-mortem* examination, there was found to be lobular pneumonia of the lower lobe of the right lung, and quite at the base there was a gangrenous portion about the size of an orange. The lung was here firmly adherent to the diaphragm, and the diaphragm to the liver, and the broken-down tissue of the gangrenous lung communicated by several openings with a cavity in the upper part of the right lobe of the liver, measuring about three inches in diameter. This cavity contained much calcareous matter, and a quantity of dirty greyish, very fetid, pultaceous substance. On careful examination, no hooklets of echinococci could be discovered. The rest of the liver and the intestines were healthy.

The absence of hooklets may be thought to negative the opinion that the tumour of the liver was originally an hydatid. But though these hooklets resist the changes which occur in the interior of the body for an indefinite period,

they do not resist the putrefactive changes resulting from exposure to atmospheric air, and such exposure must have existed here for many weeks before death.   An obsolete abscess is the only other lesion which could have produced the appearances described, but the man had never suffered from the symptoms of abscess of the liver.

CASE V.—*Death from Acute Tuberculosis.   Hydatid Tumour in the Liver.*

Lydia L., æt. 21, an attendant at a lunatic asylum, was admitted into the London Fever Hospital on 20th March, and died on 25th March 1863.   Her symptoms were those of tubercular meningitis, and the immediate cause of death was hæmorrhage from the lungs, producing suffocation.   She had no jaundice, nor any sign of hepatic derangement.

*Autopsy.*—Miliary tubercles were found in the pia mater, and also scattered through both lungs.   The upper part of the right lung contained several old tubercular cavities ; both lungs were much congested, and many of the bronchial tubes were filled with coagulated blood.   Embedded in the left lobe of the liver, and partly projecting from its under-surface, was a globular hydatid cyst about four inches in diameter.   The portion projecting from the surface of the liver was nowhere adherent.   The walls were formed by two distinct layers, having no organic connexion, so that they were easily separable—an outer or fibrous layer, and an inner layer, consisting of the gelatinous tissue of the parasite.   The cavity was filled with clear limpid fluid, having a specific gravity of 1007, and containing not a trace of albumen, but throwing down a copious white precipitate when treated with a solution of nitrate of silver.   There were no secondary cysts, but the inner surface of the primary cyst was studded with innumerable echinococci.   The cyst is preserved in the Museum of the Middlesex Hospital.

CASE VI.—*Hydatid Cyst of Liver.   Entrance of Bile.   Inflammation.
Paracentesis.   Death.*

This patient was under the care of Mr Moreton, of Tarvin, and Dr Dobie, of Chester, by whom the fluid removed from the cyst was sent to me for examination.

Joseph B., æt. 58, a publican and huckster, had led rather an intemperate life, and had been in the habit of eating much pork procured at second-rate eating-houses in different parts of the country.   For three years he had often suffered from attacks of vomiting and pain in the stomach, but he never had jaundice.   On 29th October 1864, he was seized suddenly with severe pain in the stomach greatly increased at intervals, and accompanied by tenderness on pressure, and incessant vomiting of glairy mucus.   Calomel and opium were given in full doses, and warm fomentations applied.   Next day the symptoms were much relieved, and for a fortnight the patient seemed to be improving.   At the end of this time, the pain and tenderness of the abdomen had almost left him, and the vomiting had quite ceased ; but the tongue remained coated, the appetite was bad, and the motions unnatural.   On the 14th of November, hiccup set in, which gradually became incessant, and about the same time the left lobe of the liver was noticed to become gradually enlarged.   Ten days later there was unmistakable fluctuation over a space three inches in diameter, situated in the median line, two inches above the umbilicus.   The man's condition became worse ; incessant vomiting was substituted for the hiccup, and aphthæ formed on the tongue and cheeks, but there was no jaundice, and the motions always contained bile.   On 23d November, he had some shivering sensations, but at no time distinct rigors.

On 1st December, a small trocar was introduced in the median line, two inches above the umbilicus, and about $1\frac{1}{2}$ pint of fluid was drawn off ; and on 6th December, a still larger quantity was removed.   The patient, however, experienced no relief.   He gradually sank, and died on 10th December.

The fluid removed on 1st December, after standing, consisted of (1.) a clear, slightly yellowish, supernatant liquid, containing a mere trace of albumen, but a

large quantity of chloride of sodium; and (2.) of a copious yellowish-brown sediment, containing cholesterine and bile-pigment, but no pus.

The fluid removed on 6th December was of the consistence of thin pus, and of the colour of turmeric; and it contained numerous pus-corpuscles. No deposit or change occurred on standing.

No echinococci or hooklets could be discovered in either specimen.

No post-mortem examination of the body was permitted; but notwithstanding the absence of echinococci, the characters of the fluid removed on 1st December left little doubt in my mind that the tumour was an hydatid, which had become inflamed from the entrance of bile.

### CASE VII.—*Acute Phthisis. Hydatid Cyst in the Liver.*

Sarah A., æt. 16, was admitted into the Middlesex Hospital on 6th February 1854, with the symptoms of acute phthisis, but with no sign of hepatic disease, and died on 13th February.

*Autopsy.*—Both lungs were condensed, owing to the universal infiltration of semi-transparent grey tubercle. The under-surface of the diaphragm, particularly over the spleen, was also covered with miliary tubercles. The anterior portion of the right lobe of the liver, immediately above the gall-bladder, contained an hydatid cyst the size of an orange. The liver weighed 51 ounces, and was somewhat fatty.

### CASE VIII.—*Hydatid Tumour of the Liver, opening into the Right Pleura.*
### *Empyema. Pericarditis.*

George K., æt. 54, a gardener, of sober habits, was admitted into the Middlesex Hospital, under Dr F. Hawkins, 25th April 1854. He had always enjoyed good health until four months before admission, when he was suddenly seized with pain all over the abdomen, but particularly in the right hypochondrium, and extending thence to the right shoulder. About the same time, he became slightly jaundiced. The pain and jaundice continued; and at the time the patient came under observation, he was very weak and emaciated, and suffered from incessant cough. The liver was much enlarged, extending down to the umbilicus. There was considerable bulging of the right side of the chest, which was universally dull on percussion, and devoid of respiratory murmur, except at the upper and back part close to the spine. The patient gradually sank, and died on 10th May.

*Autopsy.*—On removing the sternum, the right pleural cavity was found to be filled with a yellowish, turbid, semi-purulent fluid, containing masses of a gelatinous substance, which proved to be hydatid cysts. The right lung was compressed and flattened against the vertebral column, and at its base was firmly bound by adhesions to the diaphragm. It did not crepitate in the least; it sank in water, and was completely carnified. The liver was enormously enlarged, extending downwards as far as the umbilicus, and weighing 90 ounces. It was firmly adherent to the diaphragm. In the posterior part of the right lobe was a cavity as large as a swan's egg, lined with an hydatid cyst, and containing similar cysts in its interior. The upper wall of this cavity was formed by the diaphragm, and here there was a large opening by which the cavity in the liver communicated with the right pleura. The liver was much congested. The pericardium was glued to the heart by recent soft adhesions. The left lung, spleen, and kidneys were healthy.

### CASE IX.—*Hydatid Tumour of Liver opening through Diaphragm and into a Bile-duct. Adhesions of Pleura, Pneumonia, and Abscess of Lung. Dilated Bile-ducts. Jaundice.*

Mary Ann R., æt. 38, a servant, not married, was admitted into the Middlesex Hospital, under Dr F. Hawkins, 14th July 1854. Her general health had not been good. The catamenia had been very scanty, and five years before she had an attack of tertian ague, lasting five weeks. She had never suffered,

however, from any symptoms referable to the liver, until seven weeks before admission. She was then seized with severe pain in the epigastrium, lasting for forty-eight hours, accompanied by vomiting of bilious matter and followed by yellowness of the skin, which disappeared in a few days after taking medicine. Six days before admission, the pain, sickness, and jaundice returned. On admission, the skin, conjunctivæ, and urine were tinged with bile. The patient still suffered from vomiting and from constant pain in the region of the liver, which was considerably enlarged both upwards and downwards, and very tender on pressure. The bowels were constipated. After entering the hospital she experienced some relief, but on 24th July she had another attack of bilious vomiting, attended with great prostration, and followed by a distressing cough, and by pain on inspiration in the right side of the chest. On 26th July, the lower part of the right lung was ascertained to be solidified. After this there was urgent vomiting and increasing prostration until death on 6th August.

*Autopsy.*—The greater part of the right lung was solidified, owing to the presence of numerous irregular patches of red and grey hepatization. Its surface was universally and firmly adherent to the costal pleura ; and, on endeavouring to remove the organ, a large hydatid abscess, partly in the lower lobe of the lung, and partly in the upper part of the right lobe of the liver, was laid open. The lung and liver were inseparable, almost all trace of diaphragm being lost. The cavity contained about two pints of thick bilious-looking fluid, in which floated numerous hydatid cysts varying in size from that of a pea to that of a large walnut. The bile-ducts throughout the liver were distended with yellow bile, which oozed out freely when a section was made. One bile-duct, large enough to admit the finger, opened directly into the hydatid cavity and passed forwards in the direction of the gall-bladder. (No cause for the distension of the bile-ducts and jaundice is recorded.) Apart from the abscess, the right lobe of the liver was of the normal size and shape ; the left lobe was enlarged. The left lung, the heart, and the kidneys were healthy.

CASE X.—*Hydatid Tumour of Pelvis, compressing the Bladder and Ureters. Dilated Urinary Passages. Diseased Kidneys. Uræmia and Death.*

William S., a labourer, æt. 38, was admitted into the Middlesex Hospital, under Dr H. Thompson, 5th April 1859, and died on the following day. His main symptoms were stupor passing into coma, with epileptiform seizures, but no paralysis. There was no dropsy, but the urine was alkaline and contained a small quantity of albumen. He had also epistaxis and hæmorrhage from the gums.

*Autopsy.*—The cavity of the pelvis was almost entirely filled by a firm rounded tumour as large as an infant's head. Posteriorly this tumour pressed upon the rectum, and anteriorly upon the bladder, which was forced upwards two inches above the pubes. The ureters wound round the tumour and were compressed and flattened by it. Above this the urinary passages were much dilated ; the tissue of the kidneys was dense, their surfaces were granular, and their capsules adherent. The tumour seemed to have originated in the areolar tissue between the bladder and rectum. On laying it open it proved to be a cyst containing a quantity of puriform lymph, in which floated loosely an echinococcus cyst as large as an orange. The lining membrane of the latter was covered with innumerable small cysts, and a few hooklets were discovered. The heart, lungs, liver, spleen, and supra-renal capsules were healthy. In the brain there was a large accumulation of sub-arachnoid serosity ; the lateral ventricles were very large and filled with clear serum.

CASE XI.—*Hydatid Tumour undergoing Disintegration, projecting from the Liver, and firmly adherent to, but not perforating, the Diaphragm. Death from Acute Pleurisy of the Right Side.*

John G., æt. 41, was admitted into the Middlesex Hospital, under Dr H. Thompson, 13th September 1860. He had always enjoyed excellent health

until thirteen days before admission, when he was suddenly seized with a severe pain in the right side of his chest, increased by taking a long breath. This pain was followed by cough, gradually increasing dyspnœa, and exhaustion. On admission, there was ascertained to be absolute dulness and absence of vesicular respiration over the whole of the right side of the chest excepting the supra-spinous fossa. No friction or crepitation could be heard. There was no jaundice, nor any symptom of hepatic derangement. The patient gradually sank, and died on 15th September.

*Autopsy.*—Four and a half pints of serous fluid were found in the right pleura. The pleural surface of the right lung, and the corresponding surfaces of the wall of the chest and the diaphragm, were covered with a thick layer of recent lymph, bands of which passed from the lung to the ribs and diaphragm, enclosing the fluid in imperfect cysts. The right lung was compressed against the vertebral column, and was non-crepitant and carnified throughout. The left lung contained traces of old tubercle, but was otherwise healthy. The liver was displaced downwards, so that its lower margin reached the umbilicus, but it was not really enlarged. It weighed sixty ounces. Projecting from the posterior margin of the right lobe, between the folds of the lateral ventricle, and partly embedded in the substance of the gland, was a cyst the size of an apricot. Its projecting part was firmly adherent to the diaphragm. On laying open the tumour, it was found to contain a clear fluid with numerous shreds of membrane, which were apparently the remains of the parent hydatid. Numerous hooklets, but no perfect echinococci were discovered under the microscope. Floating loose in the fluid was one perfect hydatid cyst the size of a cherry. The heart, spleen, kidneys, stomach, and intestines were healthy.

CASE XII.—*Chronic Bronchitis and Emphysema. Adhesive Pleurisy of Right Side. Hydatid Tumour attached to the wall of the Chest, and projecting into the Right Pleural Cavity.*

Bridget D., æt. 49, a cook, was admitted into the Middlesex Hospital, under the care of Dr Stewart, on 4th January, and died on 11th January, 1861. She had long suffered from a winter cough; and while she was under observation, she had all the signs of bronchitis and emphysema, coupled with enlargement of the right side of the heart and œdema of the legs.

*Autopsy.*—The right side of the heart was much dilated, and its walls were slightly thickened. Both lungs presented the pathological characters of bronchitis with a moderate amount of emphysema. In addition to these morbid signs, the right lung was firmly adherent, particularly at its base; and between the lower and back part of the chest was an hydatid cyst the size of a plum, containing a clear fluid and innumerable ecchinococci. This cyst was attached to the wall of the chest, and was not at all adherent to the lungs, the corresponding surface of which was perfectly smooth. All round the tumour, however, the lung was connected by firm adhesions to the wall of the chest. There was no thickening of the capsule of the upper surface of the liver, and no adhesions between it and the diaphragm. The spleen, kidneys, stomach, and intestines were congested, but in other respects healthy.

CASE XIII.—*Hydatid Tumour of the Liver, bursting into the Right Pleura. Empyema. Death.*

Louisa R., æt. 17, was admitted into the Middlesex Hospital, under Dr H. Thompson, 23d March 1861. She was a servant, and until a fortnight before she had continued at her work, enjoying good health, and not suffering from any pain or uneasiness. She was then suddenly seized with acute pain in the upper part of the abdomen and on both sides of the chest, which was increased by inspiration, and was accompanied by cough, dyspnœa, febrile symptoms, and great prostration. On admission, pulse 112, small and weak. Slight cough. Dulness and absence of breathing over whole of right side of chest, except in

infra-clavicular space.  There was likewise dulness, with feeble breathing at the base of the left lung.  The hepatic dulness in the right mammary line extended nearly four inches below the margin of the ribs.  There was no jaundice and no ascites, but the urine contained albumen.  Hectic fever, with great prostration, set in, and death occurred on the 8th of April, one month after the first symptom of illness.

*Autopsy.*—The heart was normal.  The left lung was firmly and universally adherent : its lower lobe was hyperæmic, and near the base its tissue sank in water ; but it was not granular on section, and it was unusually tenacious.  The right pleural cavity was filled with pus, floating in which were innumerable hydatid vesicles from the size of a pin's head to that of an orange.  The right lung was completely collapsed and carnified, except at the apex, which contained a little air.  The liver was much depressed, its lower margin reaching to more than half-way between the umbilicus and the pubes.  Projecting from the posterior margin of the right lobe was a cyst, as large as a child's head, which was firmly connected to the diaphragm ; the liver was not adherent at any other part of its surface.  At the upper part of the cyst there was a rupture through the diaphragm, measuring one inch and a half in diameter, by which the cyst communicated with the right pleura.  The interior of the cyst was lined with an hydatid membrane ; its cavity was filled with pus and vesicles.  A large number of the vesicles were examined with the microscope, but no echinococcus or hooklet could be discovered.  There was no other hydatid tumour either in the liver or in any organ of the body.  The pelvis and calices of right kidney and upper part of the right ureter were dilated, apparently owing to the pressure below of the displaced liver ; the secreting tissue of the right kidney was atrophied ; the left kidney was normal.

CASE XIV.—*Enormous Hydatid Cyst of the Liver, passing down through the Foramen of Winslow, and filling almost the whole of the Abdominal Cavity. Paracentesis.  Pleurisy.  Tubercle of Lungs.  Death from Exhaustion.*

Elizabeth C., æt. 15, was admitted into the Middlesex Hospital, under Dr Greenhow, 26th August 1862.  She had been a very healthy infant, but at the age of three she had a severe fall on her right side, and since then she had never been well.  For nine or ten years a swelling had been observed in the right side of the abdomen.  Three years before she had been a patient in a London hospital, but she had left on account of some operation having been proposed. The tumour increased gradually in size without causing pain, while at the same time the patient herself became thin and weak.  Four weeks before admission the patient had been attacked with scarlatina, and during convalescence, or for the last few days before admission, a very rapid increase had taken place in the size of the tumour, and there had been occasional pains in the abdomen.  At the time of admission, the face and extremities were greatly emaciated ; the counte-nance had a haggard, anxious expression, and the conjunctivæ were slightly tinged with yellow.  The whole abdomen was enormously enlarged, and yielded dis-tinct fluctuation ; but the remarkable fact was that there was  resonance on percussion in both flanks, as well as in the epigastric and both hypochondriac regions.  The patient suffered from attacks of dyspnœa and of severe pain in the abdomen.  Pulse 100, and feeble ; no abnormal sound with heart ; respira-tions hurried and thoracic ; appetite good ; bowels regular ; urine very scanty, and loaded with bile.  On 3d September, the abdominal pain and dyspnœa had become so distressing that the operation of paracentesis abdominis was per-formed, and 248 ounces of a brownish fluid were drawn off.  The fluid was un-fortunately not submitted to the microscope or to chemical reagents.  The immediate effect of the operation was great relief to the pain and dyspnœa ; but within three days the swelling was observed to be rapidly increasing, and on 26th September its dimensions were larger than before the operation, although the dyspnœa was not nearly so urgent.  On the following day, the patient died from exhaustion.

*Autopsy.*—On dividing the abdominal parietes about fourteen pints of straw-coloured serum escaped. The greater part of the abdominal cavity, as far down as the pubes, was lined with a closely adherent gelatinous membrane, forming part of an enormous hydatid cyst, by which the stomach and intestines were pressed up closely against the under surface of the diaphragm and liver, where they were matted together, their peritoneal surface being considerably injected. Floating in the fluid, in the large abdominal cyst, was a secondary cyst containing about a pint of fluid, and what appeared to be the debris of other cysts. Several cysts of smaller size were likewise found in the cavity of the large sac. On tracing the large primary cyst it was found to be continuous, with a cyst about the size of a child's head projecting from, and attached to, the under surface of the liver. The two cavities, in fact, constituted one cyst, with an hour-glass constriction, the channel of communication being large enough to admit three fingers, and apparently corresponding to the foramen of Winslow. The gall-bladder was compressed, empty, and atrophied. Attached to the anterior border of the left lobe of the liver, by a thin fibrous peduncle, was another tumour about the size of a goose's egg, which, on being opened, was found to contain a crumpled-up hydatid cyst filled with a putty-like material, in which were numerous hooklets of echinococci. A third tumour was found attached to the upper surface of the right lobe of the liver, and firmly adherent to the under surface of the diaphragm, which pressed up into the cavity of the right pleura. This tumour was lined with a cyst containing about a pint of straw-coloured serum, and the inner surface of which was studded with echinococci. The right pleural cavity contained about a pint of semi-purulent fluid, and the opposed surfaces of the pleura, at the base of the right lung, were coated with a deposit of recent semi-organized lymph. Both pleural cavities were much diminished in calibre by the elevation of the diaphragm, and both lungs contained a considerable amount of scattered miliary tubercles. The heart was small, but, in other respects, normal. The spleen was pale and shrunken. The kidneys were large and congested.

CASE XV.—*Acute Tuberculosis. Hydatid Tumours in the Liver and in the Lesser Omentum.*

William F., æt. 29, was admitted into Middlesex Hospital, under Dr Good-fellow, 17th April 1864, with all the symptoms of tubercular meningitis and of tubercle in the lungs, and died on the following day. There were no symptoms of hepatic derangement.

At the autopsy, on 21st April, lymph was found at the base of the brain, and numerous granules of tubercle in the pia mater. Both lateral ventricles contained a large quantity of fluid, and their walls were softened. Both lungs were infiltrated with grey miliary tubercles, and contained old cretaceous tubercles at their apices. Projecting from the upper surface of the left lobe of the liver was a cyst the size of a large orange, about half of which was embedded in the substance of the liver. It consisted of a tough outer fibrous coat, and a delicate gelatinous inner one, containing about two ounces of clear colourless fluid and numerous secondary cysts, filled with similar fluid and innumerable echinococci. The structure of the liver appeared normal. Between the folds of the omentum, near the transverse fissure of the liver, were two solid tumours the size of walnuts. On section they were found to consist of a mass of putty-like material, in which many hooklets of echinococci were discovered on microscopic examination.

CASE XVI.—*Death from Pyæmia. Hydatid Tumour of Liver, undergoing Spontaneous Cure.*

Robert W., æt. 22, died in Middlesex Hospital, on 26th September 1864, of pyæmia supervening on necrosis of the femur. There were no symptoms of hepatic disease.

Projecting from the upper surface of the right lobe of the liver was a thick-walled cyst, the size of a small orange. It contained no fluid, but in it was a ruptured hydatid cyst crumpled up, which, when spread out in a basin of water, appeared of much larger size than the containing fibrous envelope. The matter scraped from the inner cyst contained numerous hooklets of echinococci and scales of cholesterine.

CASE XVII.—*Hydatid Tumours of the Liver and Peritoneum, compressing the Ureters and causing Degeneration of the Kidneys.*

Mary Ann W., æt. 45, was admitted into the Middlesex Hospital, 15th December 1864, under the care of Dr H. Thompson, and died 15th January 1865. For a year before death she had suffered from headache and impairment of the mental faculties, and seven weeks before death she had a fit of unconsciousness followed by right hemiplegia, involuntary evacuations, and bed sores. There were no symptoms referable to liver.

The arteries at the base of the brain were atheromatous, and there was an apoplectic cyst, with a patch of white softening in the left corpus striatum. The liver, spleen, and diaphragm were adherent by fibrous bands. In the adhesions between the spleen and liver was a cyst the size of a walnut, filled with soft putty-like matter, and lined by portions of a gelatinous echinococcus membrane. In the right lobe of the liver was another cyst, the size of a small cocoa-nut, partly embedded in its substance, and partly projecting from its upper surface, where it was firmly adherent to the diaphragm. Its outer wall was partly calcified, and its anterior was full of fragments of secondary gelatinous cysts, and soft, putty-like matter. The secreting tissue of the liver was healthy. In the folds of the mesentery of the small intestine were three partly calcified cysts, varying in size from a hazelnut to a walnut, and containing putty-like matter and secondary cysts. The greater part of the pelvis was occupied by another large cyst, situated behind and above the uterus, which was forced down, so as to appear at the vulva. This cyst contained a clear fluid and innumerable small cysts, varying in size from a pea to a walnut, all of them gelatinous and filled with a clear fluid. There was another cyst, not so large, in the right side of the pelvis. The ureters were compressed by these cysts, and the pelves of the kidneys were somewhat dilated. The kidneys were small and granular, and the cortices were wasted and hardly distinguishable from the cones. All of the cysts in the abdomen contained numerous teeth of echinococci.

CASE XVIII.—*Hydatid Cyst in the Liver, containing Bile.*

Jane F., æt. 36, admitted into the Middlesex Hospital, under the care of Dr Stewart, died on 9th April 1865, of recent tubercle and pleuro-pneumonia of both lungs. There was no symptom of abdominal disease.

The right lobe of the liver was found to be firmly adherent to the diaphragm. Attached to, and partly embedded in, the right border of the right lobe, was a thick-walled hydatid cyst, the size of a cocoa-nut, lined by a gelatinous membrane, and containing numerous fragments of other cysts mixed up with a bilious fluid and some solid concretions of inspissated bile. Running in the outer fibrous wall of the cyst were numerous large bile-ducts, but no distinct opening could be discovered. Large numbers of echinococci were found in the contents of the cyst, all of them granular and more or less altered. The rest of the liver seemed healthy, and there was no hydatid elsewhere.

CASE XIX.—*Hydatid Cysts of the Peritoneum.    Ascites and Anasarca of Lower Extremities.    Albuminuria.    Death.*

Catherine C., a hawker, aged 45, was a patient in the Middlesex Hospital, under Dr Goodfellow, from 10th January 1865 until her death on 21st June. With the exception of an attack of rheumatism, she had enjoyed good health

until about a month before admission, when she was seized with violent pain in the abdomen and loins, and at the same time her legs and abdomen began to swell. While in hospital, she suffered from ascites and great anasarca of the lower extremities. The urine contained albumen. She was treated with diuretics and purgatives, and her legs were punctured.

On examination of the body, the legs were observed to be very œdematous, and the abdomen was greatly distended. Both lungs were very œdematous, and the right lung was firmly adherent and carnified at its base.

The peritoneal cavity contained upwards of a gallon of clear serum, floating about in which were six nearly transparent hydatid cysts, with tremulous gelatinous walls, the largest about the size of a hen's egg, and the smallest about that of a walnut. The fluid in the floating cysts had a specific gravity of 1010, and contained no albumen ; that in the peritoneal cavity had a specific gravity of 1020, and was highly albuminous. The left lobe of the liver was partly atrophied, and between it and the spleen, and firmly adherent to both and to the stomach was an hydatid cyst, the size of a fœtal head, containing a little clear fluid, and innumerable smaller cysts of various sizes pressed together. In the great omentum were three or four similar cysts the size of chestnuts, and attached to the right kidney was another cyst as big as an orange. Numerous echinococci were found in the larger cysts. Both kidneys were much enlarged and fatty.

CASE XX.—*Hydatid Tumour of the Liver. Secondary Hydatid Tumours in the Spinal Canal. Paraplegia.*

The preparation of this case is in the Museum of Middlesex Hospital (V. 15), and the following particulars are extracted from the Catalogue :—

" Vertebræ with spinal cord from dorsal region. The canal and dura mater laid open. The pleura is separated from the ribs and the sides of the bodies of the vertebræ by two hydatid cysts, one on each side. The hydatids have been opened in sawing through the laminæ of the vertebræ ; but their walls remain, and the spinal cord is at this place considerably smaller than elsewhere.

" The patient was a woman aged 40, who had been admitted into the hospital with paraplegia and retention of urine. She died with a large slough on the sacrum, and the bladder was found to be inflamed. There was also a large hydatid cyst in the liver."

ARTICLE II.—*Remarks on the Retentive Power of the Abdomen.*    By J. MATTHEWS DUNCAN, A.M., M.D., F.R.S.E., etc., etc.

So far as I know, the abdominal cavity has hitherto been regarded as possessing no other mechanical qualities than those of a closed box or bag, with walls of various and variable degrees of mobility, firmness, and softness; and the various viscera have been held as being maintained *in situ* under every condition by their ligaments and by their apposition or packing within this cavity or bag. The object of these diffuse notes is to show that, while all this is true, it does not express the whole truth ; that the abdomen is not a mere bag packed full ; that in health or in ordinary circumstances it has another mechanical quality which has important physiological bearings ; that this additional mechanical quality may be present in an extraordinary degree, and that it may be diminished below the ordinary degree or entirely absent, and that these states of increase

or lessening of this quality have important physiological and patho-
logical bearings.

This quality I name "retentive power," because one of its chief
functions is to aid in maintaining the viscera in their natural position
in the cavity.

The same mechanical quality is possessed, in what may be
almost called an infinitely higher degree, by the thorax; but there
it does not merit the same name, as its functions in connexion with
the respiration and circulation are paramount and reduce to insigni-
ficance any mere retentive quaiity which gives the name to its
abdominal analogue. This higher degree of analogous power in
the thorax depends on the greater solidity and methodized mobility
of its walls and on its more complete closure or more complete
potential closure.

Evidence of the existence of the retentive power of the abdomen
is obtained by a study of the position of the unimpregnated uterus.
This organ is furnished with ligaments which permit its movement
in every direction. In its healthy or ordinary condition, these
ligaments are not on the stretch.[1] It may be pulled down to a
small extent without tightening them; it may be elevated to a
small extent without tightening them; and it may likewise be
moved in any other direction. Were there no retentive power in
the abdomen, the uterus would of course fall down till it received
support from the ligaments above or from the subjacent parts. It
does not do so, but floats in the pelvis. When by the post-mortem
opening of the abdomen, and the same would be true of the living
female, the retentive power of the closed cavity is annihilated by
admission of air, then in ordinary circumstances the uterus does fall
down from its natural position to a position in which it is supported
by the ligaments or by the floor of the pelvis. These facts are
inexplicable on any theory that I can conceive, unless they are
accounted for by supposing a retentive power in the abdominal
cavity.

Many other uterine phenomena are in a like manner explained,
as the ordinary descent of the organ when its weight is, by any
cause, increased, as by early pregnancy and by tumours; as also the
ordinary ascent of the uterus when its weight is diminished, as in
the atrophy of old age, when it is well known to draw up the
vagina into the conical shape of a tent. These phenomena may
own either of two immediate causes, namely, increased or dimi-
nished weight of the organ, or increased or diminished retentive
power of the abdomen; but in both cases it is necessary to suppose
that the retentive power exists.

Another illustration of the physiological or natural use of this
retentive power is, I believe, to be found in the study of what is
called the ascent of the pregnant uterus, from the pelvic into the
abdominal cavity. I do not deny that this phenomenon may

[1] See Edin. Med. and Surg. Journal for 1854, p. 327.

sometimes be all effected by the mere gradual increase of bulk
of the uterus, favoured by occasional alterations in the pregnant
woman's position. But it appears to me that, were this generally
the case, uterine intumescence, even though gradual [and whether
natural or morbid], would very frequently cause, by pressure,
much suffering, which we know not to be the necessary or ordinary
companion of the changes. As the uterus enlarges in early preg-
nancy (sometimes also as it enlarges in disease), the base of the
thorax of the female is expanded, the capacity of the abdominal
cavity is increased, and the uterus, instead of forcing its way up
against the influence of gravitation, is probably actively drawn
up, at least to some extent, into its new site. In some cases,
as I shall hereafter mention, this drawing up is abnormal or extra-
ordinary in force, and the uterus is then entirely sustained by the
drawing-up forces, being removed from solid subjacent support.

If we now direct our attention to the uterus after delivery, a solid
mass of about two pounds in weight, we find strong circumstantial
proof of the action of this force. Delivery being completed, the
contracted uterus is left by the accoucheur partly in the cavity of
the pelvis, partly projecting above it. It soon loses its intense hard-
ness, changes in shape and increases in bulk, its active contractions
no longer going on. Let us suppose the patient to be kept lying
on her back: this weighty mass soon no longer rests in its former
situation; it has risen out of the pelvis, and occupies a large part of
the abdomen below the umbilicus; it may even be higher. It has
no corporeal connexions with the middle of the abdomen by which
it could be elevated thither; its connexions are all at its lower
extremity, about the roof of the pelvis, and, if they act at all, can
only do so by drawing it down. It has ascended against the
gravitation of its own weight, against the resistance of superincum-
bent bowels, and anterior abdominal flap, all of which would keep
it down; perhaps also against the influence of a tight binder. It
has ascended almost vertically if the woman be regarded as in the
erect position: with the body horizontal, it has ascended in a
direction oblique to the horizon. Were there no such force as the
retentive power of the belly, it appears to me impossible to conceive
an explanation of this phenomenon. If we study the abdomen as
possessing a retentive mechanism, the phenomenon is easily
explained, and at the same time a solution is acquired of many
cases of retention of urine in women newly delivered who have
no paralysis of the bladder, no spasm and no swelling of the urethra
or vulva. In advanced pregnancy, the base of the chest is greatly
expanded, and the capacity of the upper abdomen greatly increased:
from this resisting or moderately firm thoracic circle descend the
walls of the abdomen, to join inferiorly a quite solid ring of pelvic
bone. To expel the foetus, straining efforts have been vigorous:
these first annihilate retentive power, and continuously increasing
gradually produce increasing expulsive force. The uterus is

evacuated, the abdomen greatly emptied also. This sudden emptying, while the woman lies, is generally followed by complete annihilation of retentive force, the abdominal walls being perfectly relaxed. The uterus rests in the pelvis, and bleeding is prevented by active uterine contraction. But, as the abdominal walls resume tone, they re-establish a retentive mechanism for the abdomen. The woman breathes easily, expands her chest : the uterus is drawn up into the abdominal from the pelvic cavity, and simultaneously with the cessation of active uterine contraction, the tendency to bleeding is partly arrested by the retentive action of the belly aiding the natural course of the blood.

Taking it for granted that, contrary to the general rule, the uterus when increased in weight, even when very heavy, does not rest on any underlying support [as it must do were there no retentive power in the abdomen], we have in such an occurrence illustration and proof of the occasional excess of retentive power or extraordinary amount of it. That clinical experience furnishes and demonstrates such phenomena, I do not here stop to insist ; satisfying myself with stating, what I believe all experienced obstetricians will confirm, that cases of early pregnancy and of fibrous tumour of the uterus (and of ovarian dropsy), do rarely occur in which the organ floats high among the small intestines, taking no sustaining assistance from the brim of the pelvis. That such heavy masses should be elevated into the abdominal cavity, and remain so suspended without any solid support, appears to me insusceptible of being otherwise accounted for than on the theory here advanced.

The existence of this power, not as maintaining a condition of the abdominal organs, but dynamically effecting changes, is illustrated by the following phenomena. After straining efforts to empty the bladder, a catheter being in the urethra, the evacuation of the urine is occasionally followed by the inward rush of air into the bladder when the straining ceases. The same is noticed sometimes when, instead of straining, there is pressure exerted over the hypogastrium followed by its removal. The presence of air in the bladder I have observed also under other circumstances, apart from catheterism or straining, and dependent on the position of the body on the side, while the anterior abdominal wall is relaxed. Everybody knows what will happen if, before an inspiratory effort, an opening is made in the walls of the chest. The rushing of air, above mentioned, into the bladder is an analogous phenomenon, and the familiar occurrence of the same kind at the end of a tapping in a case, say of a unilocular ovarian cyst, is still another. In the cases where I have observed this to happen, it has been easily explicable. During the evacuation of the last of the cystic contents, the anterior wall of the abdomen and specially the recti muscles are, by the hand or a bandage, pressed backwards towards the spine, and come to form a crescentic concavity, whose extremities are the anterior part of the base of the thorax and the pubic bones.

Contraction of the recti muscles tends to destroy this concavity, to increase the capacity of the abdominal cavity above what is, by other alterations on it, compensated for, and to produce the rushing in of air to fill the partial void, just as air rushes into the wounded chest on its expansion in the act of inspiration.

In connexion with the physiological uses of the retentive power of the abdomen, I think the maintenance of the position of the whole viscera deserves careful study. The case of the weighty liver situated in its upper regions is well adapted for this purpose, but its apposition to the diaphragm superiorly introduces yet an additional element into the causes of its maintenance of its position. At first sight, the ligaments appear to be unequal to the task they are generally supposed to have to perform. Alone they appear very slender, and to consist of structures which are not adapted to support for long periods of time, unyielding, such a weighty lump as the liver. Moreover, they have no attachments at either extremity such as would seem to indicate their function. And if so ill-adapted for the continued support of the liver when the body is at rest, how much less are they able to resist the sudden impulses communicated to the organ in running and jumping,—impulses which, given to so consistent and heavy a mass, must have great force,—impulses which are not resisted by any solid subjacent structure, but by the mass of air-containing, and therefore elastic, bowels. In cases of great violence, such as certain falls, the momentum of the liver, when the body is arrested in its course, is so great as to detach it from its diaphragmatic connexions and position, and lodge it in the pelvis, in spite of resistance of every kind. When the abdomen is laid open and air admitted above the liver, between it and the diaphragm, so as to leave it to the support of its unaided ligaments, and if, then, the trunk be held vertically and jerked with violence downwards, the insertion of the falciform ligament into the upper surface of the organ is readily detached, not by laceration of the ligament, but by its elevation from the subjacent liver-tissue. Were there no means of support for the organ during life but the ligaments and adjacent elastic viscera, the like effect would surely result from similar violence applied in jumping and the like. Without being able to adduce positive proof that, besides the ligaments and the adjacent viscera, the liver has other means of support, I have no hesitation in asserting my belief that it has, generally, force maintaining its position from the retentive power of the abdomen. It is attached to the inferior surface of the diaphragm by at least the pressure of an atmosphere, there being no intermediate elastic fluid such as air, and it being supposed that there is no intermediate layer of watery fluid: besides, it has for its support whatever above this atmospheric pressure may be the special retentive power of the abdominal cavity. Without these adjuvants, I cannot imagine the residence of the organ in its locality, in the erect position of the living man, without producing discomfort, and,

in certain frequently-occurring circumstances, danger. The pecu-liarity of the liver's position immediately beneath the diaphragm secures for it always, if nothing intervenes between it and the dia-phragm, the support of the pressure of an atmosphere, whatever may be the state of the retentive power of the particular abdomen in which it is. This peculiarity may render special security to the liver and spleen apart from the general retentive power of the abdomen exerted on them and on the other viscera as well. If there be any truth in these views, the ligaments of the liver and the diaphragm may assist in preventing displacement of the viscus; but they never do, in any sense, bear the weight of it: it is supported by aerostatic pressure more than equivalent to the office.

The statement just made, that the liver is attached to the inferior surface of the diaphragm by at least the pressure of an atmosphere, is true only if no fluid lie between the diaphragm and liver. If a layer of watery fluid exist between the upper surface of the liver and the diaphragm, then the atmospheric pressure will be diminished between the diaphragm and liver only by a small amount depend-ing on capillarity, and this small amount will give a little support to the liver, but to an extent quite insignificant. In more familiar terms, this presence of a layer of watery fluid is very nearly equiva-lent to a layer of air between the liver and diaphragm. If, then, contrary to the assumption in last paragraph, a layer of watery fluid exists between the liver and diaphragm, the weighty viscus depends, for the maintenance of its position, almost entirely on the retentive power of the abdomen in addition to the forces generally recog-nised and named.[1]

It can scarcely be considered out of place to direct attention to a physiological function of this retentive power, although it does not come directly within the scope of these notes, as it cannot be de-scribed as a retaining or maintaining in position in any sense. Phy-siologists have, I believe, often felt the difficulty of accounting for the ascent of the column of blood from the lower limbs to the venous opening in the diaphragm. The *vis a tergo*, the influence of contractions of neighbouring muscles, and the action of the valves, seem all, taken together, inadequate to the effect; and this inadequacy is the more striking if attention is directed to the great column of blood in the vena cava abdominalis in a man in the erect position.

If the abdomen be admitted to have the retentive power here claimed for it, then two great forces are brought into play, which will aid the ascent of fluids, of blood, and of chyle, into the thorax. For if the abdomen be supposed to be merely a closed, and prac-tically an unyielding box, without any positive or active retentive power, the actions of the heart and chest in inspiration will attract the abdominal blood, not merely in the vicinity of the diaphragm,

---

[1] For the suggestion of this paragraph, and for other criticism, I am indebted to Professor Tait.

as it attracts that in the jugular veins, but along the whole course of the vena cava, and even as far as the utmost limit of the abdomen. The soft and apparently easily depressible condition of the anterior abdominal wall may appear sufficient to destroy all such remote influence of the thoracic actions; but this cannot, of course, be the case if the retentive power is held to be proved. Besides, the analogy of the brain seems to afford conclusive evidence in favour of the possibility of such a transmission of influence to a great distance. The denuded brain is known to exhibit the respiratory pulse, changes dependent on the same thoracic actions and transmitted through the blood-vessels of the neck, which do not present any mechanical advantage for this object over the abdominal vessels. Further, in addition to this influence, or force, which will operate if the abdomen has any retentive power, the ascent of the blood into the abdomen, and through it, will be favoured in proportion to the degree of the retentive power. The first force acts if there be any retentive power; the scond is an addition to the first, and is in direct proportion to the amount of the retentive power. As we believe in the existence of this retentive power, so we believe that both these forces are adjuvant to the venous circulation. The congestion of the vessels of the intestines observed on opening the abdominal cavity may be dependent on the influence of the loss of these adjuvant forces felt even in the horizontal position. And it may be anticipated that further researches will show a connexion between modifications of these forces and various states of the bowels, as well as some abortions, menorrhagia, metrorrhagia, hæmorrhoids, and other like diseases depending, partly at least, on conditions of the vascular system.[1]

Having shown that this so-called retentive power exists and has important functions, it would be proper for me now to enter upon the description of its mechanism, its degree or force, the causes of its variations, and the means which might be employed to increase or diminish it, as medical practitioners might desire. In the meantime, I do not discuss these subjects, each specially, having stated in the course of the considerations submitted in the paper most of what I know or believe in regard to them. This knowledge is very scanty and deficient. Further inquiries will serve to extend and improve it, or perhaps to afford another explanation of the whole phenomena, a result which will be as advantageous to science and practice as the other alternative. In the rest of this paper I shall

[1] The following statement, indicating a suspicion of some abdominal influence upon the venous circulation, is by Ludwig [Lehrbuch der Physiologie des Menschen, ii. Band, S. 147]:—"Die Verkürzung oder Erschlaffung der Bauchmuskeln, wodurch der Inhalt der Unterleibshöhle sehr verschiedene Spannungen erfährt, muss natürlich auch unterstützend oder hemmend auf den Blutstrom wirken, da in der Unterleibshöhle grosse Gefässe eingeschlossen sind. Die Beurtheilung der Verhältnisse bietet keine Schwierigkeit." On the same subject see some remarks by Berard, who also gives some references.—[Cours de Physiologie, tome iv. p. 72.]

add illustrations of the force from various sources, natural and mor-
bid; and it must be remembered that, among the early supports of
a novel theory, one of the most satisfactory consists in the number
of old difficulties and incongruities which it gathers around it to get
their resolution or reconcilement.

The power here described appears to me to be ordinarily in-
creased by the erect position, and by exertions made in that posi-
tion; and assuredly there is need for its increase in these cir-
cumstances. The abdominal walls are rendered tense, and the
inspiratory effort will therefore have greater adspiratory power in
the abdomen. No doubt the descent of the diaphragm appears, at
first sight, to produce lessening of the abdominal cavity, and there-
fore diminution of retentive power. But this lessening is very
doubtful; for while this muscle descends and diminishes the vertical
depth of its own concavity, the attachments of it are elevated and
the base of the chest expanded, a change which will compensate for
the lessening by the so-called descent of the diaphragm, or perhaps
more than compensate. If, in experiment, one tries to destroy the
retentive power of the abdomen, and produce its opposite, that is
straining, as at stool, one cannot fail to see what an extended dis-
tance there is between the descent of the diaphragm in mere
inspiration and its descent with combined action of the muscles of
the anterior abdominal wall, as in the mildest straining efforts.

The increased capacity of the abdomen in certain positions leads
to dynamical exhibition of its retentive power, as in drawing air
into it. In some attitudes, as resting on the knees and elbows, air
rushes through the anus into the rectum in some individuals. In
Sims' position for the operation for vaginal fistula, air passes into
and distends the vagina. I have repeatedly observed the vagina
become filled and distended tightly with air in making examina-
tions or performing minor operations, the woman lying in the
ordinary position on her side. The mechanism by which, under
these circumstances, air is actively drawn into the vagina, I do not
attempt to describe. That it is a powerful adspiratory abdominal
effort there can be no doubt; and I have no hesitation in suggest-
ing its probable important interference in various natural and
morbid processes. Of these I shall mention some. Its presence
during copulation will contribute to, if not produce, the ascent of
the semen into and through the uterus and tubes, at least as far as
there is a free or available passage. The relaxation of the os uteri,
if not its openness, is a condition which physiologists have often sup-
posed to exist during copulation : if it does exist, then the mechanism
we have mentioned explains at once the ascent of the semen
to the fundus uteri; and if the tube is open, even to the ovary.
Here it is worth while to allude to the erection of the uterus as de-
scribed by Rouget. This erection, in an extreme degree, produces,
according to this experimenter, changes which may act as a local
adjuvant force in two ways, producing rigidity of the uterine tube

and separation of its walls; and, in doing so, producing a vacuum in the uterine cavity, which will attract free fluid into it by an injaculation contrastible with male ejaculation. Describing the changes produced by post-mortem injections, Rouget says:— " L'uterus devient plus convexe en avant et en arrière surtout ; ses bords, précédemment amincis, s'arrondissent et se developpent, de telle façon, que l'organe, après l'injection, présente un volume de moitié au moins plus considérable qu'à l'état de vacuité ; en même temps les parois de la cavité utérine s'écartent comme Günther et Kobelt l'ont montré pour les parois de l'urètre."—[Journal de Physiologie, tome i., 1858, p. 339.]

The same mechanism may, in certain abnormal conditions, to which I have elsewhere made allusion (see the number of this Journal for November), explain the backward flow of blood, or of lochia, or of pus, from the uterus into the peritoneal cavity : it may also explain the abnormal wanderings of the ovum, or its abnormal position in any case of extra-uterine pregnancy. If air or lochia may pass into the uterine sinuses, the phenomenon may receive an identical explanation.

In the natural performance of the functions of defecation and urination, there appears to me to be nothing of the nature of straining or expulsive abdominal effort. Efforts are made which, if at once increased and continued, would be straining or expulsive. But, in analyzing my own actions as far as I can, I detect no expulsive effort, but only such an amount of like exertion as destroys the retentive action of the abdomen. And it appears to me that, in many recently-delivered women, the cause of retention of urine may be found in the above circumstance, that in the recumbent position the abdominal retentive power is so great that these slight straining efforts do not reach even the length of its annihilation in their special condition. And the same explanation may apply to the difficulty of urination and defecation while lying, felt by many men as well as women.

This retentive abdominal action I have already mentioned as favouring the natural course of the blood. Its diminution or absence will be to some extent a cause of varicose veins of the lower limbs and of hæmorrhoidal congestion. A further stage than mere absence of retentive action, straining, produces engorgement of hæmorrhoids. In disease, it frequently happens that the mere absence of the retentive power produces hæmorrhoidal bleeding, as in easy going to stool. In women suffering from menorrhagia or metrorrhagia, there may be little loss even in the erect position, except when the retentive action of the abdomen is suspended. Such women often lose very little while walking about, but in urination or defecation bleed copiously.

It is easy to suggest the great influence of this function of the abdomen in such diseases as prolapse of the pelvic viscera, retroversion of the gravid uterus, hernia, versions and flexions of the

uterus. In the meantime, I conclude with the hope of returning to the more detailed consideration of these topics, and of the improvements in practice which this study may be the ground of suggesting.

Before concluding, I must point out one evident source of great difficulty in stating the problem of the mechanism of the retentive power, namely, the varying amount of flatulence in the distensible and movable bowels. This variation is not only in different individuals, but in the same individual at different times. At present we have no estimate of the increased tension of intestinal gases produced by the muscular walls of the bowels, nor have we any estimate of the diminished or altered tension of the whole abdominal elastic fluids produced by its retentive mechanism. In connexion with the amount of intestinal gases comes to be considered the facility or rapidity of their secretion or development, and the force of the closure of the intestines against the admission of air from without.

---

ARTICLE III.—*Account of a Remarkable Outbreak of Enteric Fever.* By T. SHERIFF, L.R.C.P. & S.E., Ratho.

THE present age is proud, and not without reason, of the triumphs it has achieved in almost every field of thought and action. The progress made in the various departments of the arts and sciences is unprecedented, and supplies the strongest incentives to increased efforts. Nor has sanitary science been stationary amidst the general movement onward. The laws of health have been carefully investigated and sedulously taught. The importance of pure air, and the necessity of removing offensive and pernicious matters from the vicinity of human habitations have been enforced. The sale of unwholesome or adulterated food has been prohibited, and the evils of overcrowded dwellings and workshops have been exposed and partly remedied. Attention has also been bestowed upon the important subject of an abundant supply of pure water, especially to the inhabitants of our large towns.

But while very much has been done or attempted—and the diminished death-rate of our country proves that these labours have not been in vain—there yet remains much which requires amendment. The *Times* recently pointed out the disastrous results of contaminated water, as manifested in the high rate of mortality which Chichester has exhibited, and letters calling attention to the state of some other cities in England have subsequently appeared in the columns of that journal. There is much reason to fear that this is a source of disease which exists to a great extent in our country, rendered all the more dangerous and destructive because it is unheeded or unsuspected. The condition of the cesspools and

drains, and their position relative to the well or reservoir whence water is supplied, is a most important subject of inquiry, when investigating the salubrity of any place of abode, or in determining the cause of a sudden outbreak of disease. That this is a point which is often neglected is too certain, and hence many villas in the neighbourhood of our towns, and not a few gentlemen's seats throughout the country, are in a condition which requires only the occurrence of certain circumstances or influences to render them extremely dangerous places of residence. A striking illustration of the truth of these statements has recently been afforded by a sudden outbreak of gastric or typhoid fever, which occurred under the care of Dr Craig of Ludgate Lodge, Ratho, to whom, and also to the proprietor of the house, I take this opportunity of tendering my hearty thanks for the permission which they so readily and kindly granted me of bringing the facts of the case under the notice of the profession.

The house in which the outbreak took place was built about forty years ago, by one of the ablest architects of the time. No expense was spared to render the edifice as complete as possible, and there is probably no mansion-house in the county more carefully constructed, in every respect, except as to drainage. But that department of the builder's art was then either very imperfectly understood, or grievously neglected.

From the time the house was built, until about nine months ago, it was generally occupied, and no indications of insalubrity were observed. In April last the property was purchased by the present owner, and a number of workmen were immediately employed to prepare it for the residence of the gentleman and his family. Thus, for some weeks, several masons, upholsterers, painters, and others, were at work in the house during the day, but they lodged, and had their victuals prepared, in the adjoining village.

The family took possession of the house in July, and on the 23d of that month Dr Craig was called in to see two of the members of the family, and found that for several days they had been exhibiting the symptoms indicative of fever. One of the servants was similarly affected. On the following day, another member of the family was seized; and within little more than a week Dr Craig had under his charge six of the members of the family, and two of the servants. Other two of the domestics were subsequently taken ill, and one is said to have gone home suffering from the same disease. Several of the work-people engaged about the house, also, were more or less affected. Out of those sent by one upholsterer, seven men and one woman exhibited symptoms of gastric derangement. On the whole, not less than nineteen persons were seized.

It will be sufficient to give a general description of the character and course of the disease, as in all the cases the symptoms were similar, only varying in their intensity. The commencement of the attack was marked by rigors, which were followed by a feeling of gene-

ral soreness and disinclination to exertion, together with slight head-
ache and loss of appetite. As the disease advanced, there was a
feeble and soft pulse, ranging from 90 to 120 ; but that variableness
which is often noticed in the pulse during enteric fever was not ob-
served. The tongue was covered with a thick fur ; the margin and
tip, red ; the skin was hot and dry ; and there was considerable thirst.
The bowels were at first constipated, but about the end of the second
week the evacuations became watery and ochrey-coloured. In only
two cases was there any decided tendency to diarrhœa, and it was
easily kept in check. The abdomen was distended, and there was
much pain on pressure. The urine was, as usual, small in quantity,
and high coloured. In all the cases—with one exception, to be
afterwards more fully described—the countenance of the patients
had a marked appearance of congestion, and epistaxis was a promi-
nent symptom. No rose-coloured spots were found, though sought
for carefully.

The treatment was not *officious*, though every indication and
phase of the disease were sedulously and carefully watched. At
first a mild aperient was given; then a simple fever mixture, composed
of sweet spirits of nitre and acetate of ammonia, was administered four
or five times a-day, and at bed-time a Dover's powder, with a slight
dose of Henry's saline solution when necessary. About the end of
the second week, quinine, combined with tincture of orange-peel,
was exhibited—first as a febrifuge, and afterwards as a tonic. To
allay the gastric disturbance, mustard and warm poultices were
largely employed. In all the cases, Dr Craig's treatment, aided
by most careful and judicious nursing, proved eminently beneficial,
and strikingly showed the propriety of watchfully assisting the
efforts of nature, rather than of abruptly and violently interfering
therewith.

The duration of the fever was in most of the cases from fifteen to
twenty days ; in three of them about thirty days. The disease, in
general, was of a comparatively mild type, but in one case—previ-
ously mentioned as exceptional—there was considerable danger.
While the other symptoms exhibited by the patients were in this case
more severe, the total absence of epistaxis was worthy of particular
notice. The exhaustion became extreme as the disease advanced,
and fits of delirium supervened during the night. This patient was
seen on the 4th of August, and again on the 7th, by Dr Begbie,
whose opinion, both with regard to the disease and the treatment, en-
tirely coincided with Dr Craig's. In this case also, quinine was ad-
ministered with most salutary results. It is satisfactory to state, that
all the cases treated in the house terminated favourably. The last
patient seized was the footman, who was removed to the Royal In-
firmary, and placed under the charge of Dr Sanders, where all was
done for him that skill and attention could effect. He was naturally
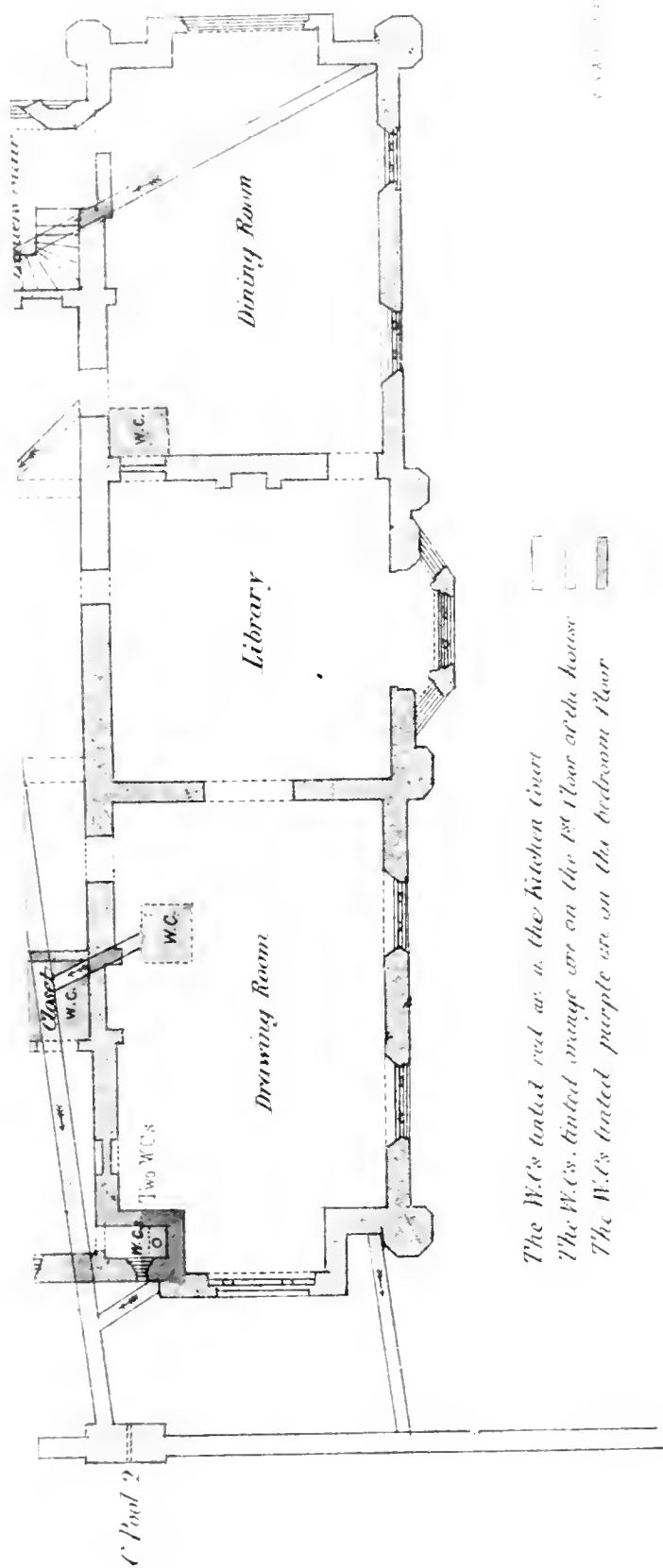delicate, of a strumous constitution, and shortly before this attack

of fever he had suffered from inflammatory sore throat. Several of his relatives had died of phthisis. This case terminated fatally, owing to the supervention of tubercular deposit in the lungs.

It may be added, that in none of the cases was there any marked crisis ; and in most of them the recovery was slow and tedious. A very important point was the marked relief afforded by the epistaxis ; and in the case where it did not occur, the disease was far more severe. There may be difference of opinion regarding the relation between the bleeding and the mildness of the attack, but the fact was evident.

It was important that the cause of an outbreak so sudden, so sweeping, and at the same time so circumscribed, should be carefully investigated, in order that the origin of the fever might be understood, and, if possible, a recurrence of disease prevented. It could not be owing to contagion, for none of the patients had been exposed to any such influences. Suspicion then fell upon the water ; and the relative position of the well and cesspools, and the condition of the drains were carefully examined. The accompanying plan will show the position of the several water-closets, the course of the drains, the cesspools, and will assist in understanding the following brief description of the house :—The well which supplies all the water is situated in the kitchen-court. At the distance of less than four yards is a cesspool (marked 1 in the plan), into which originally ran drains from three water-closets (two in the kitchen-court and one on the drawing-room floor), and also from the laundry and the scullery. This cesspool was built of common rubble work, pointed with Roman cement— the cement being so much decayed that it could be easily detached from the masonry. Very little puddle, if any, had been used to prevent the filtration of fluid from the cesspool into the surrounding sandy soil. To what an extent this had taken place was shown on a trench being dug at a little distance, for the bottom was speedily covered with a dark film of offensive matter, and also, on the mason-work of the cesspool being removed, the quantity of liquid which oozed into the cavity was so great that it required to be baled out repeatedly.

Besides, the drains which ran into the cesspool were so placed, and in such a condition, as to aggravate the evil. They were too near the well, were constructed of common rubble work, and the bottom was formed of pavement imperfectly joined. Hence fluid could easily escape from them also.

Thus, the soil surrounding the well was saturated with deleterious matter. To the eye, however, *when the water first came under our observation*, it appeared perfectly pure, and neither the taste nor the smell indicated anything offensive or injurious. But the work-people (when they first used it) had found that, after the water had been kept for a time in a white vessel, a thin black scum was perceptible on the surface, and a dark deposit at the bottom of the

Dining Room

Library

Drawing Room

W.C.

W.C.

Closet
W.C.

W.C.
O

Two W.C.'s

*C. Pml 2.*

The W.C.'s tinted red are on the kitchen court

The W.C.'s tinted orange are on the 1st floor of the house

The W.C.'s tinted purple are on the bedroom floor

vessel. And when it was subjected to microscopical examination, the presence of organic matter was manifest. In fine, Dr Davidson, the accomplished professor of pathology at Netley hospital—who also saw the patients—kindly got some of the water analyzed in August, and it was found then to contain a considerable quantity of nitrous acid—a sure proof of sewage filtration, and it also contained organic matter.

That the water was fitted to prove prejudicial to health was thus made manifest; but the investigation did not stop at that point, and it soon became extremely probable that other morbific agencies had been co-operating to produce the outbreak of disease. On looking at the plan, it will be seen that a drain runs from the cesspool marked 1, to another marked 2, situated within fifteen feet of the drawing-room windows, and which had never, it appeared, been emptied since the house was built. Into the latter the main drain of the house conveyed what proceeded from the several water-closets on the bedroom floor. Here, it may be noticed, that underneath the floor of the house there is the usual vacant space, to prevent damp and secure ventilation. In the floor of this place the last-mentioned drain is formed. It had been constructed in the same manner as those formerly described, and from it also offensive fluid had escaped into the ground through which it passed, so that the soil underneath the drain, to the depth of two or three feet, was quite putrescent; and when the drain was uncovered ·the smell was positively overpowering. The greater portion of the channels which conveyed the fluids from the baths and water-closets on the upper floors into the main drain were formed of leaden pipes; but, in particular parts, they were deficient to the extent of between three and four feet, leaving only a casing of ordinary mason-work, and at these places they had been so much worn that cavities were formed, in which large quantities of putrescent matter had lodged.

Thus the air, as well as the water, most probably contained noxious constituents.

Now the question may be very naturally asked, How it came to pass that the work-people employed about the house were so little affected by the injurious influences in existence there? To this it may be answered, that they only used the water occasionally for drinking purposes; that they neither had their food cooked in the house, nor slept there; and that the doors and windows were constantly open, and yet that several of them had, in fact, an attack of the fever.

The lessons deducible from the above-mentioned facts are of no little moment, especially at a time like this, when the country is menaced by a formidable epidemic, and when every means should be employed which may avert the scourge, or at least mitigate its severity. The sanitary condition of gentlemen's seats should be carefully examined, as respects water and drainage. This is a duty

of the members of our profession, and it is manifestly alike the duty and the interest of the owners of such abodes.   And how incumbent it is that such an examination should be instituted, even in places that are seemingly above suspicion, is forcibly evinced by this case. Here we have a house esteemed, and justly, inferior to none of its class in the county.   But it ought to be remembered that time tells upon what may have been originally well-constructed; and though the resulting evils may remain dormant for a time, yet sooner or later, under such circumstances, mischief will ensue.

It is scarcely necessary for me to state that, on the defects being pointed out, they were promptly remedied, and that now the system of drainage and the water-supply are most excellent.

It may, with all deference, be suggested that the water should be from time to time examined, both by the microscope and chemical analysis; that disinfectants, though regularly employed,.should not be implicitly relied on; and that the state of the well, cesspools, and drains should be inspected whenever any doubts are justifiable regarding them.   And only when all this is faithfully done, can the hope of exemption from visitations of such diseases be reasonably entertained.

---

ARTICLE IV.—*Cases of Ovariotomy.*   By THOMAS KEITH, F.R.C.S.E.

WITHIN the last three years I have performed ovariotomy thirty-five times, with the result of twenty-six recoveries and nine deaths.   Of the first sixteen operations I lost six of my patients; of the last nineteen, three only have died.   Hitherto no mistake in diagnosis has happened to me.   I have not yet found it necessary to make any exploratory incisions, and I have not commenced an operation without finishing it.   Of those whose lives were saved by ovariotomy, it is now satisfactory to tell, that from the youngest, aged sixteen, to the oldest, aged seventy, all enjoy the best of health.   Of the nine whose lives were shortened by the attempt to save them from a miserable death, I operated on four against my own judgment, and at the earnest entreaty of the sufferers, even when told that the chances were as ten to one against them.   In six of the fatal cases, the weight of the tumours removed was upwards of 50 lbs. In these the disease was far advanced, and the powers of life were very feeble, and death was but a relief from the utter misery that precedes the natural termination of ovarian disease.   Three only of the cases that terminated fatally were looked upon as fair average cases.   Twenty-one of these cases have been already published at length in the Edinburgh Medical Journal for 1863-4.   The following paper contains an account of the others, in the order in which they occurred :—

CASE XXII.—*Large Single Cyst, twice Tapped.    Ovariotomy.*
*Recovery.*

In the end of September 1864, Dr Chalmers, of Thornhill, asked me to see an unmarried dressmaker, forty-four years of age, who had been the subject of a large ovarian tumour for upwards of ten years. Dr Chalmers had delayed tapping as long as possible, but it had become necessary about eighteen months before, when a large quantity of thick dark-coloured fluid was removed, and a similar fluid continued to drain off for several days after the tapping.    The cyst gradually refilled, and she had again become nearly as large as before.    The savings of her industry were exhausted, and her life, for long a useless one, had now become a burden to her.

She was a large, pale, fat woman—not a very favourable-looking subject for ovariotomy.    The tumour was unilocular, and apparently free from adhesion.    As the girth at the umbilicus was $47\frac{1}{2}$ inches, it seemed more advisable to tap first, before proceeding to the extirpation of the cyst.    Nearly four gallons of fluid were removed, and the patient having had some good nights, ovariotomy was performed on the 4th of October.    The tumour consisted of a single thick-walled cyst, and was easily removed through an incision in the peritoneum not exceeding two inches, though, on account of the thickness of the abdominal wall, the external incision was upwards of six inches.    A long slender pedicle, arising from the right side of the uterus, was secured by a small clamp, and the wound was closed by one deep and five superficial silk sutures, the whole proceeding being remarkable only for its simplicity.    Dr Russell, Dr Chalmers, Dr Bogie of Annan, and Dr Scott of Dumfries were present.

She was carefully tended by Dr Chalmers, who took charge of her after the operation.    She recovered without an unpleasant symptom, and three months afterwards Dr Chalmers wrote me that she was in the best of health, and doing a large business.

CASE XXIII.—*Multilocular Ovarian Tumour, weighing 56 lbs.*
*Three Tappings.    Ovariotomy.    Recovery.*

In 1862, Mrs R., aged about fifty, from Rothesay, consulted Dr Drummond of Glasgow, now of Nice, on account of a large multilocular ovarian tumour of about three years' growth.    She had taken a great deal of purgative and diuretic medicines, and had twice been severely salivated.    After the first salivation her girth diminished very considerably, but at the same time she lost strength and became much emaciated.

As there had till then been no successful case of ovariotomy in Scotland, Dr Drummond gave her no encouragement to run the risk of the radical cure.    He advised her to give up hope of getting rid of her disease by medicines, but by simple life and full nourishment to keep up present power, and to delay tapping as long as possible. In February 1864, she was tapped, with much relief, by Dr

M'Lauchlan of Rothesay, and it was necessary to repeat the operation in September last. In the interval she had again seen Dr Drummond, and was placed by him under my care for ovariotomy towards the end of October. Though she had been tapped but a month before, her girth was already 45 inches; and since then she had suffered from severe cough, with much expectoration.

There was dulness of the lower half of the right lung, with general bronchial irritation. She was emaciated, and had a feeble pulse between 90 and 100. She looked ill, and was by no means in a favourable condition for ovariotomy. The chances of success and failure seemed about equal.

In the hope of relieving this congested state of the lung she was again tapped, and nearly 40 lbs. of fluid were removed from two large cysts, leaving a large semi-solid mass behind. Extensive parietal adhesion was now easily diagnosed, for the cyst did not recede farther than an inch below the edges of the ribs. In the course of a week the dulness of the right lung had nearly disappeared, and though the cough was still troublesome, ovariotomy was performed on the 11th of November. The peritoneum was opened to an extent just sufficient to admit the hand, very extensive adhesion above the umbilicus was separated, the largest cyst drawn forward and opened, the hand passed in and the semi-solid portion, consisting of exceedingly small thin-walled cysts, weighing upwards of 17 lbs., was broken up, and the whole withdrawn without enlarging the incision. The clamp was applied to a thin pedicle, the clots removed, the pelvis carefully sponged, and the wound closed by silk sutures.

For forty hours after the operation she remained entirely free from cough, then bronchitis set in very violently and unexpectedly, and for several days her condition was critical. Notwithstanding severe paroxysms of coughing, the wound healed entirely by the first intention. She was out of bed in three weeks, and almost able to go home; and though she had been carefully nursed through the dangers of an unusually hazardous ovariotomy, she would not allow herself to be taken care of during her convalescence, but by her imprudence brought on an attack of peritonitis, followed by effusion. Finding one morning some bulging of the recto-vaginal fossa, I passed a small straight trocar into it close behind the uterus. As only clear serum escaped, I withdrew the trocar, and the fluid was removed by absorption in the course of a fortnight, but it was not till nearly seven weeks after the operation that she was able for the journey to Glasgow.

CASE XXIV.—*Multilocular Ovarian Tumour. Second Tapping, followed by Gangrenous Inflammation of the Cyst. Ovariotomy. Recovery.*

This case was reported in the Lancet of 8th July 1865. It was one of unusual interest, and the operation which was difficult and

embarrassing, was performed under circumstances apparently almost hopeless. I saw this patient some months ago, and at first failed to recognise her, she had become so strong and young looking.

### CASE XXV.—*Multilocular Ovarian Tumour.   Once Tapped. Ovariotomy.   Death from Septicæmia.*

An unmarried lady, forty-four years of age, was recommended to me in September 1864, by Dr Ross of Inverness. On account of illness, I did not see her till the 12th of November following, when her friends placed her under my care for ovariotomy, to which she had made up her mind, and which she had weekly been led to expect at the hands of another. The tumour had been detected little more than three months before, and had hitherto caused little inconvenience, except what arose from its bulk. It filled the abdomen to within three inches of the ensiform cartilage, and was composed chiefly of one cyst, except in the left hypochondrium, where part of it was semi-solid. The girth at the umbilicus was 38 inches, and there was no evidence either of abdominal or pelvic attachment. The uterus was movable, but it lay low and backwards in the pelvis, and the space between it and the tumour was evidently small. For five weeks previous to my seeing her, the patient had taken large doses of the tincture of the muriate of iron. She was rather fat, somewhat anæmic looking; and, under a remarkably calm exterior, she had a good deal of nervousness, for at every visit her pulse became very rapid, and I do not remember ever counting it under 115. Still, her general condition was so good that had she not fully made up her mind previous to my seeing her to have the tumour removed, I should have hesitated in at once recommending ovariotomy at such a comparatively early stage of the disease.

She was tapped on the 27th of November. This was done to relieve acute œdema of the limbs, and to give her some quiet nights before operation, for latterly her nights had been restless. The cyst contained upwards of 19 lbs. of very thick tarry-looking fluid. The semi-solid portion was of considerable size, but there was nowhere any adhesion. The uterus now lay very far back in the hollow of the sacrum.

For two days after the tapping she had a very rapid pulse, with profuse perspirations, but without any abdominal disturbance. These symptoms, though rather alarming looking, were entirely of a nervous character, and disappeared after a few days, leaving her somewhat weak, but otherwise immensely relieved by the tapping. Up till this time the case had been looked upon as a remarkably favourable one for operation; but the disturbance of system after tapping had been so great and unusual that I began to fear that too favourable a prognosis had been given to the patient and her friends, and I at one time thought of advising her to go home for some months. But it soon became probable that the progress of the

disease would be rapid, for in less than three weeks her girth was already 36 inches, showing that the cyst had half-filled.

Ovariotomy was performed on the 16th of December; and before commencing I stated to my friends who were present that I had no expectation of being able to secure the pedicle externally, but that I intended to return it along with the ligatures, after the method so successfully practised by Dr Taylor Smith. There was a very thick layer of fat between the sheath of the rectus and peritoneum, such as I had not met with before. Upwards of a gallon of fluid was removed from the large cyst, which was then drawn forwards, opened to admit the hand, and after breaking down the semi-solid portion, the whole was easily withdrawn through an incision about 4 inches in length. The pedicle was even shorter than I feared, and the thickest I have had to do with. The uterus did not rise from the pelvis, and the base of the cyst was unfortunately upwards of an inch in thickness—too thick to allow me to lengthen the pedicle by placing a clamp round the neck of the cyst. After some difficulty the pedicle was tied in five portions close to the uterus, leaving the ligatures hanging out at the lower end of the wound, for, from the thickness of the mass, it was manifestly unsafe to return it with the ligatures. No sponging of the pelvis was necessary. The patient was then placed in bed, exactly one hour and a quarter after she had begun to inhale the chloroform, for the application of the ligatures had been tedious, and there was a troublesome oozing from the external incision, which delayed the closing of the wound. The cyst walls weighed nearly 6 lbs.

There was a good deal of moaning in the afternoon, which was relieved by two small opiate enemas. By evening she was in a profuse perspiration, and she expressed herself as more comfortable than after the tapping. The night was passed fully as well as it usually is after ovariotomy; but in the morning the pulse was up to 118, and in the forenoon an attack of vomiting brought on pain, which was again relieved by an opiate enema, and she slept quietly in the afternoon. By evening there was fetid red serum trickling along the ligatures. The second night was quiet, and though she required no more opiate, she moaned heavily in her sleep. In the morning the pulse was 120 and feeble, and there was a great deal of fetid red serum. By afternoon she was unconscious, and died early next morning, sixty-one hours after operation. There was no examination. Decomposition set in very rapidly; indeed, it may be said to have commenced before death. The red serum was of a very irritating nature, for the nurse and myself suffered from pustules on the fingers for some time afterwards.

Case XXVI.—*Multilocular Ovarian Tumour, weighing 26 lbs. Ovariotomy. Recovery.*

W. R., æt. 36, unmarried, was sent to me on the 6th of December 1864, by Dr Brown of Coldstream, who had detected an ovarian

tumour two months before. It was multilocular, and reached to midway between the umbilicus and ensiform cartilage. The girth was 36½ inches.

She was a very fair-complexioned, delicate woman, and thin about the arms and shoulders. She had suffered much from flatulence and dyspepsia, and had lost all her teeth before she was twenty years of age. The family history was not good: her mother having died of phthisis a few years ago. As the tumour was small, and as her general health was still pretty fair, it was agreed that she should return home, the risks and advantages of ovariotomy having been fully explained to her.

In three weeks she came back anxious to be relieved of her burden. In the interval she had been almost entirely confined to bed on account of severe pain in the lower part of the tumour, with sickness and vomiting, while the loss of flesh during this short period was very remarkable. The tumour now reached to the ensiform cartilage, and her girth had increased to 39 inches. The general condition was not favourable for ovariotomy; but this was her only resource.

The tumour was removed on the 4th of January, through an incision just sufficient to admit the hand, by first tapping the large and then breaking down the smaller cysts, several of which were in a state of suppuration. The pedicle was broad, but was secured outside with a clamp, without much pull upon the uterus. The wound was closed by deep and superficial silk sutures. Dr Brown of Coldstream and other friends were present.

Though the operation was most simple, a recovery was obtained with great difficulty. She had extensive pelvic peritonitis, with great abdominal distention. She was greatly distressed for some days with flatulence, and the vomiting was very severe, especially on the third night after the operation. This was followed by profuse and fetid discharge around the pedicle, though the rest of the wound healed by the first intention. She required very careful nursing for the first fortnight. In the third week she regained strength rapidly, and returned to Coldstream thirty-five days after the removal of the tumour, having been walking about the house for the previous week.

*(To be continued.)*

---

ARTICLE V.—*Notes on Italian Asylums for the Insane.* By THOMAS HOWDEN, Tertius, M.D., Haddington.

DURING a short tour through Venetia and Northern Italy, in the autumn of 1864, I took advantage of the opportunity afforded me to visit several of the more important lunatic asylums in the towns throughout which I passed. Such visitations I found extremely

interesting, inasmuch as they gave me what I believe to be a very fair notion of the condition and treatment of insane persons in that country.

The first asylum that I visited was the one at Turin, a large establishment within the city, containing both male and female patients. Looking from the asylum windows, on one side you find the houses of the city directly in front, and only separated from you by the breadth of the street and a narrow bit of ground within the asylum gates. From the windows on the other side, you look directly down into a public road, well shaded with trees, and largely frequented by washerwomen, who ply their avocation by the side of what seems little better than a dirty running ditch.

In going through the house, I was much surprised at the large number of patients in the infirmary, many of whom wore the strait-jacket, now so generally discarded in this country. The beds, which were wooden and extremely cumbrous, were all filled with straw, which could be changed daily ; and to each bed was attached a wooden dish or tray, which acted as a receptacle for all fluid that might find its way through the straw, there being a hole in the bottom of the bed to allow of its escape. The windows were all strongly guarded by iron-bars ; some of the stairs also were guarded by strong wire fencing carried up to the roof, seemingly to prevent any one from leaping over. I could not, however, avoid asking myself which was the greater risk, that of a patient leaping the stairs, or that of suicide by hanging from those strangely-arranged fences, the more so that the patients seemed allowed to wander about, or lie on the stairs, according to pleasure, with no one to look after them.

One of the most unpleasant things in the house was the state of the " latrines," the filth of which was abominable ; the patients were evidently allowed to make whatever use they liked of these conveniences, no check being exercised over their filthy habits.

In one of the galleries I found several women busy with the distaff preparing yarn for the spinning-wheel. This gallery, for some reason or other, was lighted entirely by windows near the roof, so that nothing presented itself to the eye of the patient but a large expanse of dead wall, without adornment of any kind. The patients themselves seemed, on the whole, cleanly kept. I was told that a number of them were at a branch establishment in the country, where they could make themselves useful in agricultural operations ; here, however, the only opportunity for open-air exercise afforded the patients was to be had in the by no means large airing-courts, which, from the situation of the building, were of necessity surrounded by very high walls.

The next asylum which I visited was one for females, attached to the general hospital at Venice. The number of patients was 350 ; their condition seemed to me wretched in the extreme. Here were to be seen poor women lying in bed, not only handcuffed, but also with a manacle round one ankle, and firmly fastened to the

foot of the bed ; chairs also, in which the patients could be confined, —but of this species of restraint I shall have more to say hereafter ; strait-jackets too, the construction of which seemed the same in all asylums, viz., a strong canvass jacket, with very long sleeves, so long that, after the arms of the patient were folded across the chest, the sleeves could be tied behind. There was little or no provision made for open-air exercise, the asylum being simply a portion of the general hospital devoted to this specialty.

The male asylum was a most pleasing contrast to the female one. It occupies the whole of the island of San Servulo, and is conducted by a body of monks. The monkish doctor, in his long gown, seemed delighted to show me everything of interest. Everything appeared perfectly clean and orderly. The number of patients at the time of my visit was 436, rather more than the establishment could well accommodate ; that, however, they seemed doing their best to remedy, several new rooms being in process of building. The baths bore no evidence of being constructed with an eye to fastening in the patient, as is too often to be found in foreign asylums : in such cases nothing but the patient's head being visible above-board. I saw nothing in the shape of restraint, and the doctor said it was but rarely had recourse to. One of the few things that conveyed anything like an unpleasant impression to one's mind was the presence of several iron gates between different parts of the house. The gardens and airing-ground were very cheerful and pleasant, with a delightful look-out over the sea and surrounding islands. On one of the neighbouring islands, the Government is at present erecting a very large asylum, which, however, I was told would not be completed for several years. There are workshops on the premises for wrights, blacksmiths, and tailors, and, out of the 436 patients, there were 120 constantly working—a fact which, I think, shows more than anything else how much the brotherhood have the good of their patients at heart. Nor is the necessity for amusement overlooked. There is a good instrumental band formed of patients ; and in the church attached to the establishment the organ is played by a patient, and there is also a choir formed of patients. None of them, however, read. The laboratory, library, etc., were all exceedingly neat and well arranged. The doctor showed me, with considerable pride, a copy of a notice of the asylum by Dr Robertson, extracted from the " Journal of Mental Science." He handed me his prescription-book, in which I found ordered many of our own most recent and favourite drugs. To conclude, I left the asylum delighted with what I had seen, and much pleased by the courtesy extended to me. I brought away with me two reports printed at the Armenian convent.

The next town I sojourned at after Venice was Padua, where, though there is no special lunatic asylum, two wards of the large general hospital are set aside for lunatic patients. In these wards there were about twenty males and as many females. Out of these

forty there were, I think, ten lying in bed with handcuffs or manacles round the ankles, or both ; the handcuffs were attached to a broad belt round the waist, the manacle again being fastened to the foot of the bed. Besides these there were others wearing strait-jackets, and with their feet fastened to the foot of the bed by a strong towel : this latter seemed rather a favourite method of restraining patients. I remember well the effect of the towel on one poor patient suffering from pellagra, the skin of the ankle having been chafed and irritated till it had broken out into a large unhealthy sore. Talking of pellagra, I may mention that the number of patients, both here and at Venice, suffering from that disease was very large. Venice is the head-quarters of all Padua lunatics, of whom there is a larger number in the asylum suffering from pellagra than is sent from any other place, except Treviso and, in some years, Udine. Pellagra seems a disease peculiar to Northern Italy ; in fact, it appears to be almost entirely confined to the famous " Quadrilateral." It is not, so far as I know, to be met with either in Florence or Rome. In the Padua wards, the patients were shut in by strongly iron-grated doors, with no other accommodation than was afforded by the dormitory, there being no separate day-room. Leaving Padua, so interesting to medical men from the antiquity of its medical school, the next town in which I found an asylum was Bologna. The asylum is in the outskirts of the town, and contains some 300 patients, male and female. Under the same roof there is an hospital for skin diseases ; in fact, the whole building was originally meant for an hospital, and not an asylum. The doors and windows were all, as usual, strongly guarded. I found half-a-dozen women shut into a little room by means of an open iron-grated gate : the only reason seemingly for their being there was that they had been troublesome, and that this was a convenient way of getting rid of them for the time being. The beds seemed all made with the intention of fastening down the patients if thought necessary ; though the resident doctor, who showed me the house, declared that there was little or no restraint : he very frankly, however, admitted the arrangement and construction of the house to be bad. Some of the sanitary arrangements were not nice : the corners of several of the passages seemed the common resort of patients whenever they wished to urinate, consequently the odours were by no means sweet. There were said to be thirteen men and eight women in the house epileptic.

The next asylum which came under my notice was that of Bonifazio, in Florence. It is a large building, and labours under the same disadvantage that many others do, viz., that of being situated in the city, surrounded by houses on all sides, and with no means of getting fresh air except in the very small courts attached to the building. Within the walls are both male and female patients, pauper and paying,—in all, between 400 and 500.

One great fault that I had to find with several of those asylums

was the carelessness of the attendants, who, I think, left the patients too much to do as they felt inclined, and this was very much the case in Florence; patients wandered about passages or lay in bed, as fancy prompted them. One man I found, on going into his bed-room, perched up on the window, some six or seven feet from the ground, his legs dangling through the bars on the other side. Handcuffs, etc., were to be found here as elsewhere; but the form of restraint which reigned pre-eminent was that of chairs, the construction of which is somewhat peculiar, and certainly very effectual. It is formed like a large arm-chair, from the front of which a board slants down to the ground. In this slanting board are two holes. The patient's legs, of necessity, rest on this board, and by means of the two holes they are very effectually strapped down. In addition to this the patient wears a pair of handcuffs; from all which, it will be readily seen that there is little chance of much movement. I shall never forget the spectacle that met my eye on entering one of the female wards. Ranged along the wall were half-a-dozen of these chairs, and in each chair was seated a poor woman, one of them a negress, black as jet. Their clothing was by no means over-abundant. One unfortunate was striving hard to eat a *lump* of bread, which, from the handcuffs being so constructed as to enclose the hands in a leather case, was no easy matter. A couple of attendants were loitering about the door of the room, which opened into a small court, watching these poor creatures. The number of epileptics in this asylum seemed very considerable, and for such patients the beds were constructed like children's cribs, to prevent their falling out during a fit.

And now I come to the last city which it was my good fortune to visit, namely, Rome. The Roman asylum is a large institution, containing between 400 and 500 patients, the numbers being pretty equally divided between the male and female. The building is situated on the banks of the Tiber, on the same side as, and very near to, the church of St Peter's. The outside walls were painfully white. This extreme cleanliness, however, was quite borne out by the condition of the interior, which was clean and well kept. The baths were by far the most complete and varied I have seen in any similar institution. Though badly situated, with the river on one side and the public street on the other, large gardens on the hill above have of late years been acquired, access to which is obtained by a covered way over the street. The view from these gardens is very fine. Before you is spread out the vast city, with the Alban hills, etc., in the distance, while closely adjoining is the noble pile of St Peter's, with its handsome piazza, and fountains in constant play. In the gardens there is a small house for the better class of patients, which, however, at the time of my visit was untenanted. Before the acquisition of those gardens the inmates must have been very much confined, and also, I should suppose, very unhealthy, especially in the summer season.

Sisters of Charity attend upon the female patients, and to their care and attention, I believe, is greatly due the cleanly condition of the inmates, many of whom seemed busily engaged sewing and making clothing of all kinds under the kindly superintendence of those Sisters. The wards were, for the most part, large, clean, and airy. The doctor in the Roman asylum seemed to look upon the notion of keeping several hundred patients in one house without ever having recourse to restraint as the most preposterous thing he had ever heard of; in fact, regarded it seemingly as an utter impossibility. There was not the same amount of restraint, however, had recourse to in this asylum that was to be found in many others. I think that, in point of cleanliness and freedom, etc., it ranked next in order to that of San Servulo, though certainly not equal to it.

With Rome concludes my short and very imperfect notice of a few Italian asylums. Let us hope that, in the great advance Italy is now making in liberty and social reform, the condition and well-being of that unfortunate class of the community will not be forgotten. Nothing in all my visits struck me more forcibly than the condition of the two asylums in Venice, seeing them, as I did, on the same day : the female one, under the direction of Government, wretched and neglected ; the one for males, conducted as a labour of love by a body of monks, who did everything in their power to ameliorate and cheer the condition of their less fortunate fellow-creatures.

---

ARTICLE VI.—*Note of Experiments confirmatory of those of Kühne on the Non-existence of Free Ammonia in Blood.* By ARTHUR GAMGEE, M.D., Assistant to the Professor of Medical Jurisprudence in the University of Edinburgh.

(*Read before the Physiological Sub-Section of the British Association, Sept.* 1865.)

FEW questions have excited greater discussion amongst physiologists than that of the coagulation of the blood, and few researches have been received with greater interest by the scientific world than was that of Dr Benjamin Richardson on this subject. Based upon a large number of most elaborate experiments, Dr Richardson's researches led him to the conclusion :—

1*st*, That the blood, whilst circulating in the living body, contains free ammonia.

2*d*, That when the blood leaves the animal body ammonia escapes, and coagulation takes place ; the escape of ammonia and the phenomenon of coagulation being considered to stand in the relation of cause and effect.

I need scarcely refer to the well-known and beautiful researches of Professor Lister, which, in a conclusive manner, proved that, however plausible Dr Richardson's hypothesis might appear, it was contro-

verted by the most convincing facts,—researches which again re-
moved the coagulation of the blood from the class of phenomena
which can be satisfactorily explained by chemical and physical
laws to the position which it had previously occupied amongst the
most characteristic vital phenomena.   Without investigating, in a
special manner, the question of the presence or absence of ammonia
in blood, Lister showed that coagulation could not depend upon the
escape of a volatile constituent.   Without impeaching the accuracy
of Dr Richardson's observations, he demonstrated that the induction
which he had been led to base upon them was incorrect.

Since the period of the publication of these researches a further
step has been made in the inquiry, for Dr Kühne, of Berlin, assisted
by Dr Strauch, published, during the year 1864, in No. 30 of the
"Centralblatt für die Medicinische Wissenschaften," the account
of certain experiments which led him to the conclusion that the
blood does not contain free ammonia.   Before alluding to the
method employed by Kühne in these researches, I must remind the
Section of those which Dr Richardson had made use of.   By expos-
ing slips of glass, moistened with hydrochloric acid, to the vapour of
blood, Dr Richardson uniformly obtained, on evaporation, charac-
teristic microscopic crystals of chloride of ammonium.   On passing
atmospheric air through blood, and conducting the expelled gases
into hydrochloric acid, and then precipitating with bichloride of
platinum, he was enabled to separate very appreciable quantities of
ammonio-chloride of platinum.   Thus, in three separate experi-
ments, Dr Richardson was able to separate, from one quart of ox's
blood, 3 grs., 2·14 grs., and 4·7 grains, respectively, of the
ammonio-chloride of platinum.

In the experiments which Kühne performed, and which were
merely undertaken as preliminary to certain researches on uræmia,
he adopted a different method, one which I believe to be at once
perfectly free from fallacy and of extraordinary delicacy, a method
which was not available to Dr Richardson at the time of his re-
searches, as the reagent employed was then unknown.

A stream of perfectly pure hydrogen gas was passed into a flask
containing fragments of glass.   The flask was furnished with a cork
which, besides being perforated by the glass-tube through which
the hydrogen was introduced into the apparatus, was furnished with
an exit tube, to which was connected a $u$ tube containing Nessler's
reagent.   A third tube, passing through the cork and reaching to
the bottom of the flask, was provided for the subsequent introduc-
tion of the blood to be analyzed.   I may remind the Section that
the reagent known by the name of its inventor, Nessler, is by far
the most delicate test which we possess for ammonia.   It is pre-
pared by saturating a solution of iodide of potassium with iodide of
mercury, and afterwards rendering the solution very strongly
alkaline by the addition of caustic potash.   On allowing the fluid
to stand, a trifling precipitate subsides, and a perfectly clear fluid

is obtained, which, on the addition of free ammonia, or of an ammoniacal salt, yields a reddish-brown precipitate.

Having determined, in this way, that the hydrogen passing through the apparatus was absolutely free from ammonia, a certain amount of blood was allowed to flow into the flask, through which a stream of hydrogen was being passed without intermission, and which was agitated in order to separate the fibrin. Kühne and Strauch found that, operating in this way, the Nessler reagent contained in the *a* tube remained perfectly clear, proving the absence of any free ammonia. It was only when the blood was heated to a temperature of about 158° that any evidence was obtained of the evolution of ammonia, the ammonia then evolved being obviously the product of decomposition. As the experiments to which I have alluded were in direct opposition to those of so distinguished an experimenter as Dr Richardson, and as the quantities of blood which Kühne and Strauch subjected to analysis were comparatively small (not exceeding 6 or 8 ounces), I determined on repeating the experiments in the most rigid manner possible, and upon a larger scale than had previously been attempted.

The apparatus and arrangements which I made use of were the following :—Large gasometers were filled with hydrogen gas, which was prepared by acting upon very pure zinc by almost absolutely pure diluted sulphuric acid. The gas was conducted through washbottles, of which the first contained solution of corrosive sublimate; the second, solution of caustic potash; and the third, strong sulphuric acid. The last of these wash-bottles was connected with a flask varying in capacity from about half-a-gallon to that of many gallons, the flask which I made use of in my last experiments being one of the large carboys used for storing sulphuric acid. The flask was furnished with a cork, through which there passed three tubes, of which one reached to the bottom of the flask, and served to conduct hydrogen into the apparatus; a second, also leading to the bottom of the flask and of a comparatively large diameter, served to admit blood into it; and a third, which did not pass into the flask, was joined by a gutta-percha tube to a Will and Varrentrap nitrogen bulb, containing perfectly clear Nessler's reagent. A certain amount of water and pieces of broken glass were placed in the flask. I commenced my experiments by allowing hydrogen to bubble rapidly through the apparatus for at least half an hour. When the reagent was found to remain perfectly clear, the experiment was gone on with in the following manner.

The carotid artery of the animal (whose blood was to be examined) having been laid bare, an opening was made in it, a ligature having been previously placed on the cardiac side. A glass-tube was then introduced into the artery, and this was connected by means of tubing with the glass-tube which has already been alluded to as intended to allow the entrance of blood into the

apparatus. On removing the ligature, which had been placed on the cardiac side of the tube introduced into the artery, the blood flowed rapidly into the flask, through which a brisk stream of hydrogen was being passed. Assistants were at the same time employed in agitating the flask, so as to separate the fibrin. This part of the operation was attended with no little practical difficulty, as it was no easy task to shake the bottle without disturbing the other parts of the apparatus; and unless the fibrin were separated, besides introducing the fallacy that the whole blood would not be exposed to the influence of the hydrogen gas, the fluid in the flask frothed in such a manner as to prevent the continuance of the experiment.

*Exp.* 1.—*29th Dec.*—The arrangement above described made use of. Right carotid of horse exposed. About 24 ounces of blood allowed to flow into the flask, the purity of the hydrogen gas having been tested by allowing it to bubble for an hour through the apparatus. After the introduction of the blood, the gas was allowed to bubble through the fluid for an hour. At the end of this time, the Nessler's reagent remained perfectly clear.

*Exp.* 2.—*31st Dec.*—Apparatus arranged as in former experiment.

At 12, a very brisk current of hydrogen was passed through the apparatus, and tested by Nessler's reagent until fifty minutes past twelve.

At 12.50, about one pound of blood received directly from the right carotid of a healthy young shepherd's dog.

At 1, Nessler's reagent perfectly clear.

At 1.5, reagent still clear.

At 1.25, ditto, ditto.

At 1.45, reagent still perfectly clear.

*Exp.* 3.—On the 17th of January, I arranged an apparatus as I have already described. The flask used was of the capacity of $2\frac{1}{2}$ gallons; 20 ounces of distilled water placed in it.

At 11.45, hydrogen gas allowed to bubble through the apparatus.

At 12.15, the Nessler's reagent remained perfectly clear. A tube having already been inserted into the right carotid of a horse, a stream of blood was directed into the apparatus. The flask was constantly agitated, and a brisk stream of gas kept up.

At 12.35, Nessler's reagent remains perfectly clear.

At 1.10, reagent perfectly clear.

At 1.35, ditto, ditto.

At 1.40, two drachms of a very weak solution of ammonia (of which a like quantity was afterwards analyzed, and found to contain 0·555 grain of ammonia) were introduced into the apparatus. Almost instantly the most characteristic reaction was obtained. The Nessler's reagent contained in the first bulb of the Will and Varrentrap apparatus became reddish-brown, and turbid; and, after a few minutes, a characteristic precipitate subsided.

The experiment was now stopped. The fluid in the flask was accurately measured, and found to be exactly 160 ounces, of which 20 consisted of water, which had been introduced before the blood.

The experiment, besides showing that 140 ounces of blood did not evolve perceptible traces of ammonia, demonstrated how admirable was the method employed for its detection, it being capable of detecting with certainty and ease 0·555 grain of ammonia when mixed with one gallon of a fluid, consisting of 140 ounces of blood and 20 ounces of water. In Dr Richardson's experiments, the smallest quantity of ammonio-chloride of platinum obtained from one quart of blood was 2·14 grains, which represents 0·16 grain of ammonia. The amount of blood analyzed by me ought, at this rate, to have contained 0·56 grain of ammonia. That this quantity of ammonia would have been discovered with extraordinary facility was proved by the great ease with which I was able to detect a smaller quantity of ammonia purposely added.

Dr Richardson did not, however, consider the amount of ammonio-chloride of platinum which he obtained to represent accurately the amount of ammonia contained in blood, for, at page 276 of his work, he remarks, that as the amount of gaseous exhalation is greatest when the blood is first drawn, and as the blood was caught in an open vessel, and transferred to another vessel, much of the vapour was obviously lost ; and in his speculations as to the amount of ammonia probably present in blood, Dr Richardson remarked (page 332), that one part of alkali to 8000 of blood, containing 2·2 per thousand of fibrin, would be sufficient for retaining the normal fluidity of circulating blood. The proportion may be less, but need not be more. According to this estimate, the amount of ammonia which must have been required to keep the fibrin fluid in the quantity of blood analyzed by me must have been about 8·4 grains. The method which I, however, employed in the analysis would, I am sure, have detected the 100th part of this quantity of ammonia.

*Exp.* 4.—Apparatus arranged as in last experiment, a large glass carboy being substituted for the flask. Between two and three gallons of blood introduced directly from the right carotid of a healthy cow. The nitrogen bulbs, instead of containing Nessler's reagent, contained pure HCl, of sp. gr. 1·11.

After allowing the hydrogen to bubble through this quantity of blood for twenty-five minutes, the bulb was detached, the acid emptied into a capsule, a solution of bichloride of platinum (purified by repeated solution in a mixture of alcohol and ether) was added, and the mixed fluid evaporated to dryness in the water-bath. The residue was entirely soluble in alcohol and ether.

Dr Richardson, in his interesting work, remarked, that it was a question (page 325) whether the ammonia evolved in the blood halitus of all animals is ammonia ($NH_3$), or ammonia in which nitrogen is combined with a basic radical, in lieu of a simpler element. " There are," remarked Dr Richardson, " many ammonia

bodies of the latter class, such as methyl-ammonia, ethyl-ammonia, biethyl ammonia, all having properties so closely allied to the simple ammonia that they may be considered practically identical with the simpler form." It might, therefore, be asked whether Kühne and my experiments, which prove (to my mind) the absence of ordinary ammonia in the blood vapours, are equally conclusive of the absence of the compound ammonias; and this question, I think, I may answer in the affirmative. I have carefully examined the reaction of several of the compound ammonias with Nessler's reagent, and I find that, just as the physical characters of their platinum compound are almost identical with those of ordinary ammonia, so the reaction with Nessler's reagent is extremely similar. I have found that extremely dilute solutions of ethylamine, triethylamine, trimethylamine, and phenylamine (the only compound ammonias at my disposal), are precipitated by Nessler's reagent, the colour resembling closely that of the precipitate obtained with ordinary ammonia. The precipitate yielded by ethylamine and triethylamine has, however, more of a canary-yellow colour than of a reddish or brownish yellow colour,—that of the ordinary ammonia precipitate. One small drop of a not very strong aqueous solution of ethylamine, added to 6 ounces of water, gave, with Nessler's reagent, a distinct yellow haziness.

Further researches will, I believe, confirm and extend the results of the experiments which I have performed, and satisfactorily prove that Nessler's reagent is almost as delicate a test for the compound ammonias as for simple ammonia.

---

ARTICLE VII.—*A New Uterine Support.* By WM. MAIN, M.D., Lasswade.

(*Read before the Obstetrical Society, June 26, 1865.*)

NOTWITHSTANDING the many instruments which have been invented of late years for the cure or relief of displacements of the uterus, most medical men, I presume, will agree with me in saying that at present the treatment of many of these unhappy cases is very far from satisfactory. Perhaps the most generally useful and convenient appliance yet suggested is Dr Simpson's loop pessary of gutta-percha. Nevertheless, I have met with several cases in which that instrument afforded little or no relief, though fitted with all the care I could bestow upon it. With the view of adding to our resources in these troublesome affections, I beg to bring to the notice of this society the instrument which I now show.

It consists of a circle of gutta-percha, enclosing a copper wire of No. 12 thickness, fitted to receive the os and vaginal portion of the cervix uteri. Connected with this circle is a stem of the same
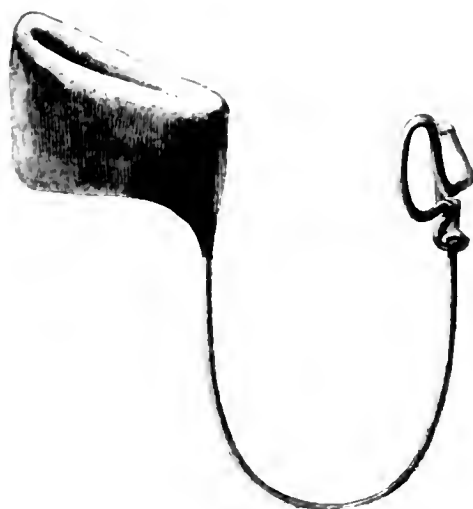
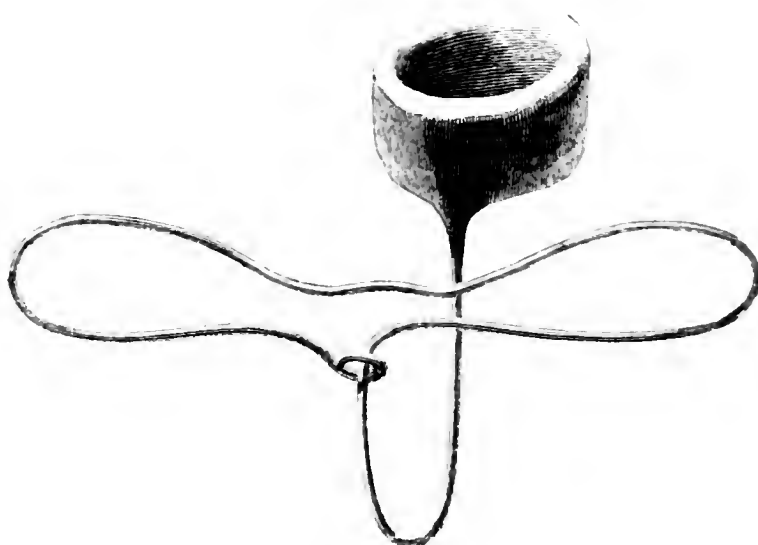Fig. I.— Side View of Uterine Support for Retroversion



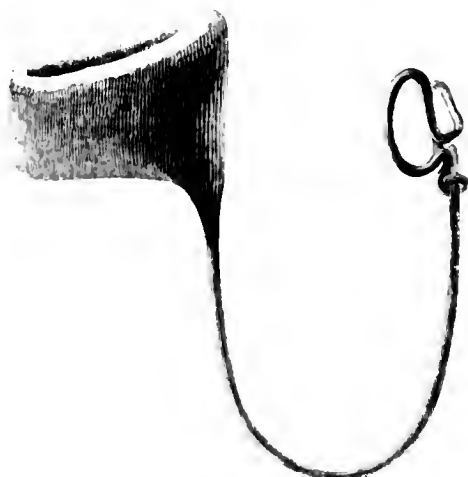Fig. II.— Front View of Uterine Support for Retroversion



Fig. III. —Side View of Uterine Support for Cases of Anteversion.

copper wire, a continuation of that enclosed in the gutta-percha, which terminates in a sliding loop. The instrument is very simple, is easily made, and does not cost more than two or three pence. Its application is also very easy. A bandage round the loins, with thigh straps, having first been adjusted, a sound is introduced into the uterus, and its position rectified. The gutta-percha circle is then passed over the sound, which acts as a guide, and is pressed gently upwards till it receives the os and cervix. The stem is then bent to fit the vagina, and, being sustained there by the fingers of the left hand, the remaining portion is carried forwards to the upper edge of the mons veneris. The running limb of the terminal loop is now brought to this point, and secured to the main stem by a piece of tape. It now only remains to bend the loop into two semi-circular horns, one to run along the lower part of the abdomen on either side, immediately above the groins, and fasten these horns to the abdominal bandage by tapes or stout pins. The instrument is now secure, and, when properly applied, it has never in my hands given rise to pain or irritation; but, on the contrary, has invariably afforded relief to all the annoying uneasiness of a displaced uterus, and permits of any movements, such as walking, sitting, or stooping, with perfect freedom.

I have had an opportunity of trying the instrument in a few cases of prolapsus, retroversion and anteversion of the uterus, and have found it equally serviceable in all of these, a slight change in the form of the gutta-percha circle being all that is necessary to suit the different kinds of displacement. In cases of simple prolapsus, the upper margin should be horizontal. Again, in those of anteversion, the anterior part of the circle should have a raised cushion of gutta-percha, so as more effectually to support the uterus in its proper place; while in cases of retroversion, the cushion or bolster should be on the posterior part of the circle, with a similar object in view.

The instrument may be made large or small, deep or shallow, to suit the circumstances of each particular case; and should the patient be large or fat, a wire of greater thickness than No. 12 may be substituted with advantage.

I may also mention that I have found the copper wire frame, without the gutta-percha circle, very useful when using Dr Simpson's galvanic pessaries. By taking merely the loop and stem, and on the end of the latter fixing the galvanic pessary with a little heated gutta-percha, it can be retained perfectly in its position in the uterus, while, at the same time, that organ can be kept at any desired angle by bending the stem; the loop end of the wire being fixed to the bandage, as before suggested.

As an illustration of the usefulness of these instruments, I append a short account of two cases. First, a case of anteversion.

E. T., æt. 24, a servant girl, had been incapacitated from work for some years in consequence of dysmenorrhœa, with stricture and anteversion of the uterus. Intra-uterine galvanic pessaries were

introduced; but by no possible means could they be retained, the uterus expelling them as soon as the support of the finger was withdrawn. The gutta-percha platform support, india-rubber balls, and a sponge, were all tried in vain. They failed to keep the pessary within the uterus. Dr Simpson's wire pessary was now tried, and it gave complete relief; but after a few weeks it caused so much irritation that I was obliged to withdraw it, when I found the uterus had become very much enlarged. In a short time the uterus was discovered to have resumed its former anteverted position, and I then began to use the loop pessary; but it gave little or no relief. Some modifications of the instrument I now show were afterwards tried, with encouraging results; and she has now been wearing one of the present form for some months, with complete relief. A day or two ago, I passed a sound, nearly straight, up to the fundus of the uterus.

How far she may ultimately recover it is impossible to say. At present, menstruation is not quite regular, and she suffers from frequent disordered function of the liver, but can walk about with perfect ease and comfort.

The next case which I shall relate is one of retroversion.

M. C., a young woman, 25 years of age, had suffered severely from dyspepsia for seven years before coming under my care,—vomiting a large quantity of mucus, and a small portion of her food almost daily, for nearly all that period, according to her own and her mother's statement. Her menstruation was irregular, painful, and not free; and she suffered from pain and weakness in the back, along with other uneasy symptoms which usually accompany this condition. After careful inquiry, I came to the conclusion that the uterus was the source of her distress, and warned her friends that local treatment would most probably be required, but that, in the meantime, medicine could be tried. Bismuth, nitrate of silver, nux vomica, lime-water, &c., were tried, with temporary good effects, but at every menstrual period all her symptoms were greatly aggravated. On examination, per vaginam, the uterus was found retroverted, and the passage somewhat constricted. The introduction of a galvanic pessary greatly relieved her dyspeptic symptoms within twenty-four hours, though a little local irritation was experienced. Under the use of the pessary for a short time, and without medicine of any kind, menstruation became quite regular and natural, and the functions of the stomach were completely regained. I now thought we could possibly dispense with the pessaries altogether, and withdrew the one she wore; but within twenty-four hours vomiting returned as bad as before, accompanied by all her other distressing symptoms. The gutta-percha loop pessary was now tried, but with only very partial relief; then some modifications of the present instruments, which were found to give relief so long as they retained their proper position. On becoming displaced or withdrawn, however, the vomiting almost immediately returned.

She has now been wearing a pessary similar to the one I have shown you for about four months, with perfect comfort, and with relief to all her symptoms. She can walk any ordinary distance, work in the garden, stoop, &c., with ease, and has grown comparatively stout and ruddy on good food, air, iron, and cod-liver oil; and my only anxiety now is to find whether she will ultimately be able to do without the pessary. At present, I fear, she cannot; for about a month ago I withdrew it, in the hope that she would remain well; but I was disappointed to find that the vomiting returned in two or three days, and continued to recur as usual till the instrument was replaced. This is a point, however, which I shall again test erelong, and shall be happy to report the result to the Society.

======

ARTICLE VIII.—*Cases treated in the Barnhill Hospital and Asylum.* By D. MACGREGOR, L.R.C.P.E. & L.R.C.S.E., House-Surgeon and Medical Superintendent.

### I.—*Excision of Hip-Joint. Recovery; with Useful Limb.*

EXCISION of the hip-joint has been so rarely successful in Scotland, that the following case may not be uninteresting to the readers of the Edinburgh Medical Journal.

J. M., æt. 17, of a strumous habit, two years ago felt a pain in his left hip, which he likened to a "rheumatic pain." He thought lightly of it, till, in the course of a month, the pain increased so much that he was unable to walk to his work, and he sought admission to the Royal Infirmary. After a week's rest in bed, he felt so thoroughly recovered that he was dismissed to resume his work. In a week or two afterwards, however, the pain returned with increased severity. He was again obliged to leave off work and seek parish relief; he was accordingly admitted to this hospital. In spite of rest, the long splint, and local applications, with good diet and cod-liver oil, etc., suppuration took place. The matter pointed on the outer aspect of the hip, opposite the root of the great trochanter. Free vent was given to the pus, and poultices applied, with a continuance of rest to the affected joint—the limbs being now tied to a pillow placed longitudinally between them. The discharge having continued for many months, while the patient was supported by wine, porter, and good diet, it gradually dried up, the skin cicatrized, and the patient began to improve.

The case thus went on well for many weeks, while the limb was kept at rest, with a view to anchylosis. Reasonable time having been allowed for this, and the patient not only feeling the joint free of pain but strong and well, he was allowed to get up and walk about a little on crutches.

This continued for a couple of weeks only, when the pain returned, and suppuration again commenced, the discharge being so con-

stant and profuse that the patient was rapidly giving way under it. Hectic fever soon set in, and the patient was fast sinking, when, to give him a chance of life, excision of the joint was resorted to.[1]

Chloroform having been administered in the usual manner, a longitudinal incision, about six inches in length, slightly curved outwards, was made in a line with the posterior edge of the great trochanter. A gush of blood at once filled the gap, which was the result of general oozing, owing to the increased vascularity of the surrounding parts due to long-standing disease, and not to any important blood-vessel having been wounded. Compression, by means of sponges, for a minute or two, completely arrested the flow, and the operation was continued. The capsular ligament was found only partially destroyed, particularly opposite the articulation, and the ligamentum teres was not at all affected. This rendered the turning out of the head of the femur rather a difficult task, and, to allow more room for the purpose, I made an incision outward at right angles to the first. This enabled me to get round the head of the bone, cut the ligamentum teres, and turn the bone out. It was sawn through the root of the great trochanter. In doing this the saw passed through a considerable abscess in the cancellated texture of the bone, the contents of which were of a dark sanguineo-purulent-looking character. The bony texture at each cut extremity gave way readily before the finger; in fact, the finger scooped out a large cavity in the interior of the detached portion of bone, while a smaller one was gouged out of the shaft, so as, if possible, to remove the whole of the disease. I was delighted to find the acetabulum healthy. The cartilage was white and glistening, and I did not touch it, further than to make sure that it was not diseased. Not a single artery required ligature. The parts were placed in apposition, and both incisions brought together by suture, with the exception of the lower part of the transverse one, which I left open to give egress to any discharge that might supervene. On examining the removed portion of bone, the whole of the neck was found to be carious. It had a worm-eaten appearance, and crumbled under the finger-nail. This condition was, no doubt, the cause of the abscess found in the bone, and the obstinate discharge, which so nearly proved fatal to the patient. The cartilage on the articular surface of the femur was healthy.

After the application of a long splint, which was continued opposite the wound by means of a curved rod of iron fixed to it, so as to allow of dressing the wound without its removal, the patient was placed in bed. With the exception of a slight discharge, which continued a few weeks, he went on without a bad symptom. The longitudinal incision healed by the first intention, and the transverse on the cessation of the discharge. The long splint was thus kept applied for three months, when it was removed. The patient was

[1] In the performance of the operation I was ably assisted by my late assistant, Dr Pole, now of Lochgoilhead, and by G. Wilkie, Esq.

kept in bed, however, another fortnight, at the end of which period he could move the limb about, free of any pain or uneasiness, except a little stiffness, which was to be expected. Fourteen weeks after the operation, he was allowed to get out of bed, and, with the aid of crutches, walk about. He laid more and more weight upon the limb, which is very little shortened, till now—five months after the operation—he walks out with only a stick to support him. With a high-heeled boot, he will by-and-by have a most useful limb. The hip-joint is, of course, anchylosed; but not only has the patient still the use of a good limb, but the operation has most undoubtedly saved his life.

## II.—*Arrest of Hæmorrhage by Acupressure.*

It is in one case more particularly that I found this method of arresting hæmorrhage very useful, when a ligature could not be applied. The case was an amputation below the knee in an adult male, necessited by phagedenic sloughing of the whole of the soft parts of the leg below the middle, after typhus fever. The tissues were so soft and friable that no hold could be got of the posterior tibial. It either gave way under the forceps, or after the ligature was applied. A common darning-needle was passed over it, at once arresting the bleeding. The needle was removed on the third day; no secondary hæmorrhage took place, and the stump was completely healed up within a fortnight,—one of the best stumps I have ever seen. In such a case as this, there can be no doubt of the value of acupressure; and my opinion is that, with a little more experience of its use, it will supersede the ligature in every case where it can be employed.

## III.—*Treatment of Acute Rheumatism by Blistering.*

In every case of acute rheumatism coming under my care for some time back, I have invariably ordered blisters to all the joints chiefly affected, according to the plan recommended by Dr Davies, and with the best results. I have now tried it in a good many cases, and have not found it fail in a single instance. One case, that of a man who had a severe attack a couple of years ago, laying him up for more than six weeks with most agonizing pain in his joints, and the usual constitutional disturbance consequent on a bad case, was lately admitted with an attack similar in all respects to the first. Blisters were at once applied to the joints principally affected in the upper and lower extremities, only a purgative being given internally. The following day he was free of pain, except what was caused by the blistered surfaces. No bad symptom followed, and in five or six days he was dismissed well. Here, therefore, was an attack of acute rheumatism, similar in every way to a former one which incapacitated the patient for six weeks, cured in so many days. The heart was slightly affected by the first attack, but its condition was certainly not aggravated by the second.

From my experience in the cases I have treated in this way, I think that blistering is the most speedy and effectual cure yet adopted for this most painful malady. I find the blistering fluid the most simple and easily applied preparation of cantharides. It can be painted over the surface, however uneven, while there is often a difficulty in keeping the ordinary emplastrum cantharidis in contact with some joints—as the knees.

---

ARTICLE IX.—*Successful Case of Ovariotomy.* Under the care of Dr Scott. Communicated by Dr Turner, House-Surgeon, Dumfries Infirmary.

*History.*—Mary Black, æt. 45, mill-worker, married, was admitted to the Dumfries Infirmary on 2d September 1865. The patient stated that she had been married about twelve years, but had no family, and that she had enjoyed her usual health up to January last, when her menstruation suddenly ceased, and she began to feel a sense of general debility, with loss of appetite, and an incapacity for work. Afterwards she commenced to suffer from frequent attacks of severe pain in the left lumbar region. About three months before her admission to the hospital, she, for the first time, became sensible of an increase in the bulk of her abdomen. The abdominal pain at last becoming very severe, she applied for admission to the infirmary.

*Symptoms on Admission.*—On examination, there was found to be considerable enlargement of the abdomen, the bulging being most prominent on the left side. There was complete dulness on percussion, over the left side, extending from the iliac fossa to the hypochondriac region and slightly across the middle line towards the right side of the abdomen. The edges of the tumour could be defined by careful palpation, but no fluctuation could be detected. It was hard, firm, and immovable.

On 1st October, fluctuation was observed in the right aspect of the tumour, and about 6 ounces of characteristic glairy fluid were evacuated by the trocar. There being now no doubt of the nature of the case, and the patient beginning to present a very cachectic look, immediate extirpation of the tumour was decided upon.

Accordingly, upon 6th October, ovariotomy was performed in the usual manner by Dr Scott. On the peritoneum being opened, it was found that the adhesions formed by the tumour were trifling and easily separated. The tumour was multilocular, the cysts containing a thick, viscid, glairy fluid, of the appearance and consistence of cold solution of starch. It was somewhat irregular in form, and equal in size to the patient's head. The length of time occupied in the performance of the operation did not exceed twenty minutes.

The wound healed by the first intention, and the patient made a

rapid recovery, without a bad symptom. On 22d October (16 days after the operation), she was permitted to rise from bed ; and on 27th October (21 days after the operation), she was able to leave the hospital.

*Remarks.*—The diagnosis of this case was for some time rendered somewhat difficult by the density, position, and apparent solidity of the tumour, and the absence of fluctuation. The frequent severe attacks of inflammatory pain induced the belief that serious adhesions would be found, but happily this turned out not to be the case.

# Part Second.

## REVIEWS.

*Some of the Causes and Effects of Valvular Disease of the Heart : being the Croonian Lectures of the Royal College of Physicians for 1865.* By THOMAS B. PEACOCK, M.D., F.R.C.P., Physician to St Thomas's Hospital, etc. London : Churchhills : 1865.

Dr PEACOCK has long devoted special attention to Cardiac Pathology ; and, in adopting it as the subject of the Croonian lectures, he has availed himself of the opportunity of bringing together the results of his numerous observations, the details of which are scattered through the periodical literature of the day. The result is a work which we consider of great value, and calculated to direct attention to various points which have been hitherto, to a considerable extent, overlooked.

The importance of inflammatory affections in producing valvular disease has long been recognised ; but, in Dr Peacock's opinion, too exclusive attention has been directed to them.

The causes of valvular disease he arranges as follows :—

1. Malformation of valves, arterial and auriculo-ventricular.
2. Injuries of valves, immediate and gradual.
3. Alterations in capacity of orifices and cavities.
4. Inflammatory affections, chiefly rheumatic, acute, and chronic.

It is of the first three causes that Dr Peacock treats, the fourth having already been fully elucidated.

1. *Malformation as a cause of disease.*—On this point we are principally indebted to Dr Peacock's own researches ; at least, he was the first who, in a paper in this Journal, published in 1853, directed attention to the subject. At that time, his remarks were confined to the aortic valves ; but he has since extended his views, and regards malformation of the auriculo-ventricular valves as occasionally the source of disease. Malformation of the arterial valves may be

either in excess or defect. The exact mode of development of the aortic valves we do not know; but, in Dr Peacock's opinion, each segment is originally composed of two parts, more or less divided, which subsequently become blended together. Where the number of valves is in excess, this is due to the presence of supernumerary valves, which are only partially separated from some of the other segments, so as apparently to indicate an imperfect blending of the curtains together. This form of imperfect development does not seem to be of any particular consequence, as it does not appear to interfere with the performance of the function of the valve. Malformation by defect, Dr Peacock believes to originate thus : the segments of the valves were originally correctly formed ; but, during fœtal life, the angles and contiguous sides of two or more of the valves become adherent, and the band of union of membrane which indicates the line of union subsequently becomes atrophied, and more or less completely disappears. This adhesion and blending of the valves appear to be due to intra-uterine disease of a similar character to that which produces adhesion and thickening in after-life,—namely, endocarditis. This form of malformation is important, as, in a considerable number of cases, it lays the foundation of serious disease. Where there are only two semilunar valves, and where, as is usually the case, one of the curtains is considerably larger than the other, the larger one, not being adequately supported in its middle, has a tendency to become stretched, and to fall below the level of the other segment, so as incompletely to close the orifice during the diastole of the ventricle. The regurgitant current thus established, will have a tendency to turn back the edge of the valve so as to aggravate the evil. Another important point is, that such malformed valves appear to be peculiarly liable to the occurrence of acute and chronic inflammation, leading to thickening and induration, or to partial destruction of the edges of the defective curtains. In the class of cases where the whole of the curtains are blended together, more or less obstruction to the flow of blood from the ventricle is necessarily occasioned. At the same time, the opening which exists is usually incapable of being closed, so that regurgitation from the artery into the ventricle also occurs. As life advances this condition becomes aggravated, the valves become thickened and indurated, so as to become rigid and unyielding, and to reduce the opening to a very small size.

Our only objection to Dr Peacock's explanation of these cases is, that it does not explain why a deficient number of segments is much more common at the aortic than at the pulmonary orifice. Supernumerary valves are present far more frequently at the pulmonary than at the aortic orifice ; but adhesions in the great majority of cases are met with at the latter. Were the adhesions leading to deficiency in the number of segments due to intra-uterine endocarditis, we should expect them to be more frequent at the pulmonary than at the aortic orifice ; for it is well known that,

during fœtal life, disease is more frequent in the right than in the left side. While, therefore, we admit that Dr Peacock's explanation may be correct, we cannot look upon the problem as completely solved.

The auriculo-ventricular valves are also, though less frequently, the seat of such malformations, the flaps of the valves being blended together at the centre so as to form a kind of membranous septum, separating the auricle and ventricle, and perforated by a larger or smaller aperture, generally of a triangular shape, in the centre. In such cases, one set of auriculo-ventricular valves is rarely the exclusive seat of disease; the other set, as well as the aortic valves, being generally simultaneously affected.

From Dr Peacock's observations, it appears that malformation is a far more common cause of subsequent disease than is usually supposed. He says :—" On referring to the notes of valvular disease which have been under my own care, and in which I have had the opportunity of examining the heart after death, I find that of twenty-six cases of aortic valvular disease, nine probably originated in malformation of the valves; and of seventeen cases of combined aortic and mitral valvular disease, two probably so originated; or, in other words, of forty-three cases in which the aortic valves were diseased, either alone or in conjunction with the mitral valves, in eleven, or 25·5 per cent., there was malformation of the valves, which probably laid the foundation of the subsequent disease; a proportion which is much larger than would à priori have been expected."

In these cases, however, the disease does not generally show itself in early life. For, while in the cases of aortic disease assigned to malformation, the age of all the patients averaged 42·3 years, the mean age of the patients in whom aortic disease originated in other ways was only slightly greater, or 47·4 years. In Dr Peacock's opinion, where the symptoms and signs of uncomplicated aortic disease manifest themselves in persons who have never had rheumatic fever or other serious illness, or have never sustained a severe accident, or followed for a long time a laborious avocation, we shall generally be right in assigning its probable production to malformation of the valves. Our diagnosis will be still further strengthened if the patient be young, or below middle age; if the patient have been habitually delicate, or has shown any symptom of cardiac defect; or if the symptoms have occurred suddenly, when the patient's strength was prostrated by some general indisposition, rather than when he had suffered from any cause obviously affecting the heart. The author's statements in this chapter are illustrated by a number of interesting cases.

The second lecture considers injuries as a cause of valvular disease. The valves of the heart may be directly injured by external accidents, but, in general, they give way under violent muscular exertion. Such cases are, however, rare; Dr Peacock being only

acquainted with seventeen. In the majority of cases, the aortic valves suffer, exposed, as they are, to the pressure of the column of blood in the arteries. " The other valves are, however, also occasionally ruptured. Of the seventeen cases, the four quoted being included in the enumeration, the aortic valves were injured in five cases, and probably also in five others; the columns of the mitral valve were ruptured in three cases, and probably in one other; and the columns of the tricuspid valve were torn in three cases." In the great majority of cases, the patients were males, about middle life, and were making a violent exertion at the time the rupture took place. The symptoms of such lesions are generally very characteristic. Severe pain is usually experienced in the region of the heart, extending to the back and to the shoulders and arms. When the aortic valves are injured, syncope is the most marked symptom; while in injuries of the mitral valve, a sense of oppression at the chest and of suffocation, are usually experienced. In some cases, seen shortly after the injuries, the physical signs indicating the defect were perceived. The severe primary symptoms commonly subside to a considerable degree; but the patients are seldom free from signs of cardiac disturbance, and usually die in no long time.

The third cause of valvular disease, considered by Dr Peacock, consists in alterations in the capacity of the orifices and cavities of the heart. The effect of dilatation and contraction of the aortic orifice is first treated of. The aortic orifice, as shown by Dr Chevers, is not a mere opening, but is a cylindrical canal, bounded below by the fibrous zone, into which the convexities of the semi-lunar valves are inserted, and, above, by the angles of attachment of the segments. It is thus about six lines in depth, and below is connected with the muscular substance of the ventricle; above, with the origin of the aorta. " If the ventricular portion of the orifice be dilated, either alone or in conjunction with dilatation of the ventricle, or relatively from contraction of the arterial portion, the sacs or sinuses of the valves become expanded, from the pressure of the column of blood in the aorta falling more directly upon them; if, on the contrary, the outlet of the passage be dilated or the angles of the valves become stretched, the curtains drop below their proper level, the sacs are rendered shallow, and the segments have their spaces of contact diminished. In the former case, the tendency is to breaking down the curtains at their most dependent parts; in the latter, to retroversion of their edges; in either case, the apparatus becomes incompetent, and allows of regurgitation from the aorta into the ventricle." The auriculo-ventricular valves may be rendered incompetent (in addition, of course, to changes in the valves themselves), either by the orifice becoming unduly enlarged, or by complete adaptation of the segments being prevented by alterations in the size of the ventricle. Changes in the size of the orifice are of very common occurrence, but, as a rule, do not lead to

incompetence, because the curtains of the valves are capable of
great extension, so as to be able to occlude the orifices, even when
their capacity greatly exceeds the natural size. Incompetency of
the valves from non-adjustment is, according to Dr Peacock, of
more frequent occurrence and of greater importance. This cause of
incompetency was first pointed out in the case of the tricuspid valve
by Mr William King, and in that of the mitral by Professor
Gairdner. In these cases the ventricle is considerably dilated, and
usually altered in shape, being broader at the apex than is natural.
The valves and tendinous columns are often much stretched, and the
fleshy columns reduced in size, and sometimes almost entirely absent,
being blended with the enlarged ventricular walls. The mechanism
of the valves is thus well described by Dr Peacock :—" There is,
perhaps, in the animal frame no more beautiful example of the adap-
tation of structure to the function to be accomplished, than is afforded
by the auriculo-ventricular valves. The insertion of the cords into
the fleshy columns, instead of directly into the muscular walls, is
apparently not to give greater power of resistance to the pressure of
the blood during the systole, but to furnish a means of shortening
the attachments of the curtains, when, with the contraction of the
ventricle, the walls are more closely approximated. Were it not
for this arrangement, the free fold of the mitral, for instance, would
fall back towards the auricle during the systole, and the two curtains
not being properly adjusted, the blood would flow into the auricular
cavity. By the action of the muscular columns, however, the cords
are drawn upon as the parietes of the ventricle approximate, and
the curtains are kept in apposition and tightly stretched across the
aperture, so as effectually to close it. It is, I think, clear, that
when the ventricle becomes enlarged, unless the cords and columns
undergo a proportionate alteration so that their just relations are
maintained, the perfect adaptation of the curtains will be interfered
with, and the free fold of the mitral will be held down or allowed
to fall back, and the aperture be so kept more or less open."

The third lecture treats on the changes produced in the heart by
valvular disease ; of the symptoms and results of different forms of
valvular disease ; and of the general principles of treatment. To the
first of these points we have not space to allude, but would refer our
readers to Dr Peacock's tables and remarks as giving a very clear
view of the subject. With regard to the prognosis in cases of
valvular disease, Dr Peacock considers, and in this we quite agree
with him, that duration of life is generally greater in cases of mitral
than of aortic disease. Probably the chief reason of this is, that in
cases of aortic disease death results from failure of the heart's action,
and may take place suddenly and when least expected, even when
there is but little serious defect. Whereas, in cases of mitral
disease death is generally due to pulmonary engagement or some
other complication : the symptoms come on much more gradually,
and there is more time for treatment to take effect. There is

peculiar danger in those suffering from aortic disease being exposed to any sudden excitement or taking any sudden exertion; such persons are living in a state of unstable equilibrium; when everything is going on smoothly they may appear to be in good or even robust health, but a slight additional weight to one end causes the beam to be irrecoverably and fatally depressed.

Dr Peacock concludes with some short but judicious remarks on the treatment of aortic and mitral valvular diseases. We shall quote his observations on the use of digitalis: a subject on which there is still much difference of opinion :—

"It is well known that digitalis possesses special action upon the heart, lessening the frequency of its pulsations; and it has hence been supposed that it is particularly applicable as a remedy in cases of cardiac disease. It has, however, I conceive, been employed too generally and too indiscriminately in their treatment. In many cases of disease, and, indeed, usually in cases of obstruction at the aortic orifice or in the course of the aorta, and always in cases of incompetency, the heart acts violently because it has a serious obstacle to overcome; and to reduce the power of its contraction would be equivalent to adding to the obstruction. In such cases, therefore, a remedy which, like digitalis, impairs the power of the heart, cannot but be injurious. It has, I am aware, been contended that digitalis not only lessens the frequency, but increases the power of the heart's pulsations; that, indeed, it exercises a tonic influence over the muscular structure. I have, however, not seen any decided proof of the correctness of this opinion; it has several times occurred to me to observe the symptoms of cardiac incompetency greatly aggravated by the use of the remedy, and equally remarkably lessened by its discontinuance. The sedative influence of digitalis appears to be only exercised upon the heart and arteries when it is employed for a short time; if long continued, it loses that power, and produces a depressing effect upon the general system, under which the pulse becomes both feebler and quicker. I have noticed that when patients have taken digitalis, even in small doses, for a long time, the discontinuance of its use and the exhibition of stimulus, has been attended by a great improvement in the general condition, and a proportionate diminution in the frequency of the pulse. I have at present in St Thomas's a man labouring under aneurism of the common carotid artery, who some time ago was placed under digitalis combined with a nutritious diet; under this treatment his strength declined and his pulse became feebler and quicker. The digitalis was discontinued and some wine given, and with this change the pulse fell and continued quiet, the local disease making little progress for many months. In another patient, also at present in the hospital, a boy labouring under mitral valvular disease, the sequence of acute rheumatism, the pulse fell under the use of digatalis, combined with quinine and iron and wine. To test whether this was due to the remedy or to the tonic medicine and diet, I decreased the digitalis, but found that the pulse still fell the reduction being evidently due to the increasing strength and vigour of the patient.

"In cases of mitral valvular disease, I believe, however, that digitalis is eminently useful: not by any influence which it exerts over the heart itself, but from its powerful diuretic action, by which it tends to lessen the amount of the blood, to relieve congestion, and promote the absorption of any fluid which may have been effused, and so indirectly to assist the action of the heart. Dr Withering, when speaking of the use of digitalis in dropsy, says, that it is not in cases where 'the patient is strong, the skin warm, and the pulse firm and hard, and the anasarcous limbs tense and resisting,' that the remedy operates beneficially: on the contrary, 'when the pulse is feeble and intermitting, the countenance pale, the lips livid, the skin cool, the belly soft and fluctuating, and the limbs pit freely on pressure,' the diuretic action is satisfactorily established, and with the

greatest advantage to the patient. Or, in other words, digitalis is effectual in those cases of dropsy which, in a large proportion of instances, we now know to be dependent on or connected with mitral valvular defect. It must, however, be recollected, that in these cases also the action of the remedy requires to be carefully watched and the strength of the patient to be upheld, so as to guard against too depressing an influence being exercised upon the heart."

It is needless to say that we entertain a very high opinion of Dr Peacock's work, and consider it a valuable addition to cardiac pathology.

*The Malformations, Diseases, and Injuries of the Fingers and Toes, and their Surgical Treatment.* By THOMAS ANNANDALE, F.R.C.S. Edin., Assistant-Surgeon to the Royal Infirmary. The Jacksonian Prize for the Year 1864. Edinburgh: Edmonston and Douglas: 1865.

DISEASES of the fingers and toes might, at first, be considered a subject of comparatively little interest; but, from the necessity of the integrity of these organs for the purposes of prehension and progression, their diseases and injuries have an importance quite disproportionate to their size. The volume before us is an exhaustive treatise on the subject, and will form a valuable addition to surgical literature.

The first chapter treats of congenital affections of the digits in relation to their hypertrophy, congenital deficiencies, supernumerary fingers and toes, congenital union, contractions, and tumours. In the second chapter we have a good account of the inflammatory diseases of the digits, while the third and fourth treat of the tumours and injuries to which they are liable. In the fifth chapter non-congenital contractions and dislocations of the digits are considered; the sixth and seventh treating of the joints and bones of the digits, and amputation of the fingers and toes.

The work entering very largely into details, we cannot follow the author through its different portions; it may be sufficient to say that it is in the highest degree creditable to Mr Annandale, who exhibits great powers of observation, while the treatment he recommends is invariably based upon sound surgical pathology. Great additional value is given to the work by the very numerous and excellent lithographic drawings by which it is illustrated.

# Part Third.

## PERISCOPE.

### SURGERY.

#### ON A NEW MODE OF ARRESTING HÆMORRHAGE BY TEMPORARY COMPRESSION
#### BY FREDERICK CHURCHILL, ESQ.

THE very rapid success which has attended the introduction of acupressure as a hæmostatic agent in surgical operations, and its speedy adoption by many of our most distinguished surgeons, is sufficient proof that the ligature, though employed with such success for a number of years, is not the best mode of arresting arterial hæmorrhage. Still the very fact that the advocates of acupressure have hitherto failed to produce a single valid argument to prove the advantages of acupressure over those of the ligature, except as a temporary compressor of the bleeding point, is sufficient to induce us to give further consideration to the claims of the latter. With this view I began to consider how we might combine the advantages of each, avoiding all the disadvantages, or, in other words, to secure the artery against the risks of secondary hæmorrhage by a ligature removable at will. The result of my investigation is the knot I am about to describe. Take two ligatures of the ordinary length, and tie upon them a reef-knot. This may be either prepared beforehand, and kept slack, so as to be slipped over the head of the forceps when the artery is seized, or, with the help of an assistant, it may be easily tied at the time of the operation.
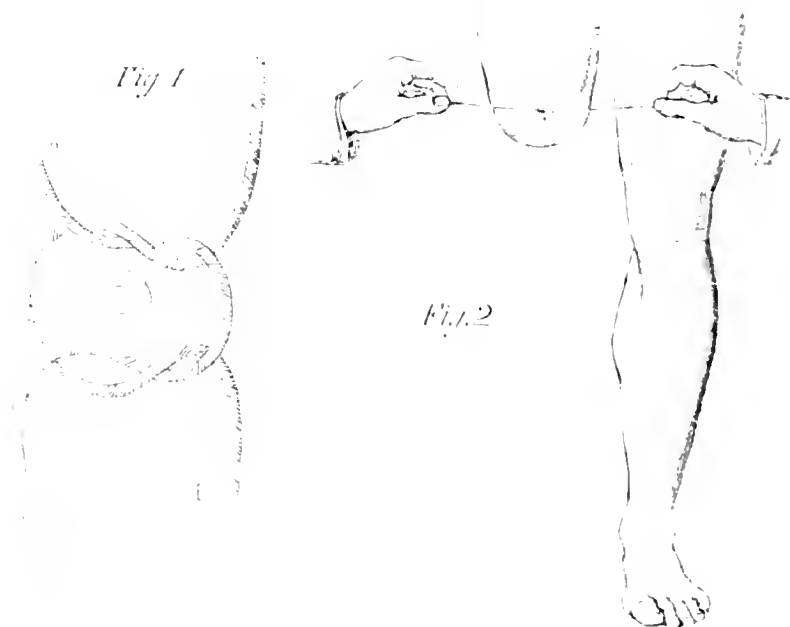


NOTE.—In figure 1, the engraver has represented the ends of the knot much shorter than they would be in actual practice.

The form of knot may be better understood by referring to Fig. 1. In the centre of it is a transverse section of the gutta-percha tube described below. When the knot is made fast, cut close either A and B or C and D, the remaining ends being arranged on each side of the flap (vide Fig. 2). Where several ligatures are employed, it is advisable to tie the corresponding ends over the face of the stump. When the artery is occluded, which, I believe, will prove

to be in a much shorter time than with acupressure, a gentle and equable pull of the corresponding ends in a straight line will untie the knot. The fact that the middle coats when divided will retract, renders it certain that the risks of secondary hæmorrhage are less than when the needle is used, and, consequently, an earlier removal of the ligature can be effected with safety.

Should the objection be raised that " in sponging the flap you may untie the knot,"—I have found by experiment that it holds as firm as the old ligature, except when counter-traction is made upon the corresponding cords, which never would be the case when sponging; or that " in removing the knot you disturb the artery,"—I maintain, on the contrary, that the removal of the knot *may* tend to occlude the artery more effectually, by twisting it upon itself, and cause the formation of an external clot within the sheath to support the internal coagulum. In order to test the resisting power of the knot to the current of blood in the artery, I made a number of experiments which prove that the single reef is quite sufficient to hold any artery. For instance, I took a piece of thick elastic tubing about three inches long, to one end of which I fastened my knot, and into the other I inserted the nozzle of a very powerful syringe, the piston-rod being an inch in diameter. I then handed it to a sceptical friend, who injected water into the tube, but with all his force he failed to dilate the knot. This he acknowledged quite convinced him. Still, in order to combat, if possible, all the objections that may be raised, I experimented with the double reef—*i. e.*, a reef-knot on either side of the artery, which has, of course, double the strength of the old ligature; and I removed this also with comparative ease, though not quite so readily as the single reef.

I am therefore sanguine that, should my future experiments on living animals confirm those I have already made, my plan will become generally adopted. With these few remarks, I beg to leave the suggestion in the hands of the profession, who, I have no doubt, will give it an unbiassed reception, if it prove worthy of recognition at all.—*The Lancet.* [We understand that Professor Pirrie has employed this method in a case of amputation, and that (to quote his own words) " it has succeeded admirably."—*Ed. Ed. Med. Journ.*]

### ON HERNIA IN CHILDREN.   BY M. GUERSANT.

HERNIA is met with almost as frequently in children (oftener in boys than in girls) as at other ages, and especially during the early months of life. Some facts lead M. Guersant to believe that it is sometimes hereditary; but however this may be, its production at this early period is favoured by the less tense and less closed condition of the apertures, and by the exaggerated character of the efforts made by infants, whether in uttering their cries or passing their evacuations.

The cries of the child, the neglect of the application of an abdominal bandage, or the infant's feeble or emaciated condition, may prove the cause of umbilical hernia soon after birth. It is easily recognised, and when only small, the aperture may become narrower, and the tumour promptly diminish in size, especially when the child becomes stronger and stouter. When the hernia increases in size, and the bandages that are usually resorted to slip and prove inefficient, M. Guersant rejects all spring trusses and those having a fixed pad in their centre, and prefers the employment of adhesive plaster. A pad is formed of a hemisphere of yellow wax or vulcanized caoutchouc, and is fixed by a band of diachylon plaster long enough to make a turn and a half round the body, and about four centimetres in breadth. It is an excellent bandage when carefully applied, adhering to the skin, and well supporting the hernia. The yellow wax should be softened by heat so as to form a ball, which is divided into two parts, varying in size according to the dimensions of the ring, which it must always exceed, so as not to pass within it. This ball, covered with fine linen, should be so applied as to have its convexity against the ring, and the flat part in contact with the plaster. The parts which will have to come into contact with the diachylon should first be powdered with rice or starch, for the prevention of erythema. The plaster, covered with a

bandage, should be left on for three or four days, when it should be renewed, another ball of wax and plaster being in readiness, and the finger applied so as to prevent the hernia escaping until this can be put on. By the careful use of this bandage, the hernia is generally cured in six weeks, although two months or even more may be required. M. Guersant has never had recourse to the ligature or other means of radical cure; nor has he ever met with strangulated umbilical hernia, although he has seen it so swollen as to require poultices before it could be reduced.

Inguinal hernia is of common occurrence, especially in boys, being often double. It usually contains the small or large intestine, with or without omentum and in the case of a child seven or eight years of age, M. Guersant met with the ileo-cæcal appendix. In three female children he has seen hernia of the ovary; this, mistaken in the first case for an encysted tumour, was removed, the child dying of peritonitis. Inguinal hernia in children, left to itself, may acquire a large size, but it may sometimes exist without causing any notable accidents. Generally, however, there are colicky pains, especially when the hernia is not reduced, or is only imperfectly retained. The choking *engouement* of hernia, characterized by the presence of matters in the intestine, and especially seen in feeble aged persons in whom the bowel is not supported, is also met with in weak and delicate children. The tumour is increased in size and weight, and has a soft and pasty feel. The skin retains its normal colour, and the abdomen is distended without being painful; nausea, or even vomiting, being sometimes present. Local emollients and some enemata generally suffice for relief, which takes place by evacuations; but the taxis, or even a gentle aperient, may be indicated. Strangulation is more rarely met with in the child than in the adult, but M. Guersant has seen several cases, even in infants only a few weeks old. In general, however, he has found the hernia yield to the taxis. When this has to be applied, especially in the younger children, the patient should be placed upon an inclined plane, so as to bring the head and trunk lower, and the pelvis higher. The application is then made in the same way as in the adult, and for a short time without chloroform. When it proves of no avail, as is often the case, owing to the resistance which the efforts of the child make to the reduction, a cataplasm should be applied over the tumour, and a bath administered. Chloroform is then resorted to during the use of the taxis, and almost always reduction is accomplished. M. Guersant has only had recourse to an operation in three cases, and in only one of these was it successful.

However, occurrences of this kind are very rare in children, for when treated in good time their hernias can generally be cured by the constant and careful application of appropriate bandages, and that especially when the child, from having been thin, gains flesh. When treatment is commenced during the early months of life a cure may be effected in four or five months, while, when the child is a year or more of age, a twelvemonth will scarcely suffice. For older children the bandage will have to be continued for some years, and it must at first only be discontinued during the night before leaving it off entirely. For young infants, Galante's caoutchouc bandages with air pads are to be preferred; but for children from six to twelve months old, spring trusses, very lightly made, and covered with jean and oiled silk, are required. It is indispensable, in the case of young children, to have two bandages at least, as, often getting wet, they require to be changed daily.

Whenever the testis is found to have passed the external orifice of the inguinal canal, and yet remains near the ring, endeavours must be made to prevent its re-entering the canal. When it is found that it cannot be kept external to the ring, without exerting too much compression, by a hernial pad constructed for the purpose, which at the same time prevents the hernia from descending, Marjolin's advice should be followed—to return both testis and hernia, and maintain both reduced, rather than expose the child to the double danger of compression of the testis and strangulation of the intestine.—*British and Foreign Medico-Chirurgical Review.*

## ON STRANGULATION OF THE TESTIS WITHIN THE INGUINAL CANAL.
### BY M. VELPEAU.

An interesting case of this kind was presented at M. Velpeau's clinic. A man, twenty-seven years of age, and of robust build, though small in stature, was admitted with a tumour in the right groin, having all the appearances of a strangulated hernia. It had only existed during two hours, and gave rise to the most intense suffering, accompanied by frequent vomiting of brownish matters having a fæcal odour. The patient related that, from birth, the right testis had not descended beyond the root of the penis, the slightest pressure sufficing to cause it to pass back into the abdomen. While standing at his work as a cabinet-maker, he was seized with a violent pain in the abdomen, while a swelling appeared in the groin, which gradually extended to the scrotum of the same side. The patient was placed in a bath for three quarters of an hour, with some relief to the pain. In the groin was a very hard and slightly movable tumour of about the size of an orange, and very sensitive to pressure, the scrotum of that side being distended with fluid. Under the application of ice, the pain and tumour had in a few hours almost entirely disappeared. Next day, two tumours in the scrotum, the size of a walnut, were recognisable: the one consisting of the testis, a good deal atrophied, surrounded by a small quantity of fluid, and the other in the inguinal canal, composed only of fluid. By pressure, the testis could be forced back into the canal, followed by the fluid which surrounded it. The pad of a truss was applied against the external ring, so as to prevent its gaining admission to the canal. For a similar occurrence, two years since, the patient was near undergoing an operation for hernia.

Cases of this kind M. Velpeau observed, are very rare, and easily misunderstood. Curling, indeed, mentions several cases of strangulation of the testis, but they are generally complicated with hernia, and are developed in quite another manner—viz., from within outwards, the gland contained within the abdomen entering the canal by the internal ring. In other cases the symptoms are due to an orchitis arising either in the abdomen or within the canal. In the present case, however, the testis was habitually placed without the external ring, at the root of the penis; and what renders it so remarkable is, that there was such an entire absence of orchitis that the unenlarged gland could next day be handled without any pain. What took place was this: the testis having entered the canal from without, became compressed, whence the severe pain and a serous exhalation into the canal, the undilatable rings resisting the exit of the gland more and more. The liquid continuing to increase, filtered below into the tunica vaginalis constituting a second tumour in the scrotum. Compression of the gland gave rise to immense suffering and vomiting, and all the symptoms of strangulated hernia; indeed, to symptoms still more intense, for the subjects of hernia are far from complaining of such severe suffering. The prognosis in such a case is a serious one, if, as often happens, the patient does not take proper precautions. The symptoms may all reappear, and even a real strangulation may compel recourse to an operation; while independently of this, inflammation, the formation of pus, and death itself may occur. But by means of a good truss, and care, all such accidents may be avoided. In the application of this we have not the same difficulty as in cryptorchids, where, in order to prevent a hernia, we are obliged to prevent the possible descent of the testis, as here we have to fix the pad below the ring. It is true that the fibres of the cremaster, very powerful in some subjects, may, during their contraction, compress the gland against the pad, and give rise to some slight signs of strangulation; but this is usually of little consequence.—*British and Foreign Medico-Chirurgical Review.*

## THE AMAUROSIS AND DEAFNESS OF SMOKERS AND DRINKERS.
### BY MM. SICHEL AND TRIQUET.

M. Sichel, in continuation of a former communication published in 1863, observes that among the forms of cerebral amaurosis there are two which, although little known, are not of infrequent occurrence, and are difficult of cure. One of these, produced by the abuse of alcoholic drinks, he described as long ago as 1857, under the designation of "amaurosis symptomatic of delirium tremens;" and the other, produced by the abuse of smoking, was first described by Mackenzie. Incredulous as to this last, when first announced, M. Sichel, in the course of twenty-eight years' practice, has frequently met with it, and he believes that there are few persons who can smoke for any long period more than five drachms of tobacco daily, without their vision, and often their memory, becoming affected. In both these forms of amaurosis there is well-nigh absence of all well marked cerebral congestion, and there is a vagueness in their sthenic or asthenic characters, which may cause hesitation and perplexity on the part of the surgeon, if unaware of the cause in operation. The ophthalmoscopic appearances, as in most old cerebral amauroses are negative or ill-marked. The optic papillæ, sometimes very white, especially in one of their halves, sometimes slightly injected, have their contours ill-circumscribed or in part effaced. The retina is but little injected, the central vessels being sometimes normal and sometimes enlarged, the central veins being especially so when the affection has reached its last stage. All the characters observed are, in fact, in common with those of other cerebral amauroses. As in many of these, too, the memory is often enfeebled; and in the amaurosis from alcohol there are frequently trembling of the hands in the morning, and, at a later period, morning vomiting. Both of these varieties are very slow in their progress towards cure, and very refractory to treatment. Usually observed separately, they may be seen together, and in such cases it is not easy to decide whether the tobacco or the alcohol plays the chief part. The treatment of these cases usually occupies a long time, and an essential point, of course, is the discontinuance of the practice that has given rise to the amblyopia or amaurosis. In the few cases in which there is any marked congestion present, this must be met by antiphlogistics; but when this is not very positive, bleeding must only be resorted to with the greatest care. As in all forms of passive or old cerebro-ocular congestion, liberal depletion, even by leeching or cupping, and still more even moderate bleeding, soon completes the loss of vision, and this is only slowly and incompletely restored. On the other hand, external and internal stimulants, such as liniments, flying blisters, camphor, strychnine, etc., resorted to before a moderate antiphlogistic and derivative treatment has been put into force, only aggravate the disease. When there is but little congestion, mild aperients are very useful, such as equal parts of cream of tartar and magnesia, alternating with pills of gum ammoniac, sulphate of potass, and aloes. In drinkers these means will not be borne, and minute doses of rhubarb and magnesia may be substituted. Cold water should be applied to the forehead and eyes, while the lower extremities are irritated by sinapisms, dry cupping, etc. At a later period are indicated stimulant liniments to the circumorbital region, flying blisters first to the nape, or behind the ears, and then to the temples; and in very obstinate cases, the various internal stimuli, as camphor, arnica, strychine, etc., may be tried.

M. Triquet states that in smokers and drinkers an insidious and obstinate form of otitis frequently becomes developed. There is a kind of numbness or torpor of the ear, with a sense of cold, but rarely any pain. There is no cerumen in the meatus, the membrane and ossicula are in a normal state, and there is little or no vascularity. There is, however, extreme dryness with very minute granulations of the pharynx, nasal fossæ, tubes, and middle ear. Frequently both ears are affected, but one has always commenced being so before, and is more deaf than the other. The deafness, without being very troublesome at first, rapidly increases. Noises in the ear almost always exist at an early

period, and it is of importance to notice that they assume a hissing sound. The affection exhibits itself in three periods,—1, that of excitement, in which there is intolerance of noise, and a hissing noise in the ear ; 2, that of depression, in which the hissing sound disappears, or only remains as a distant and feeble echo ; and 3, that of a paralytic condition of the auditory nerve, in which the sense of hearing is more or less completely, and often permanently lost.    In this period there are also often trembling of the tongue, embarrassment of speech, and disturbance of vision.    The prognosis is very unfavourable, for those patients alone are susceptible of cure who will consent to leave off the bad habit which has produced the affection.    For treatment, in the early stages cupping of the mastoid processes and drastic purgatives, and then alteratives, as calomel, sulphur, and small doses of arsenic, are indicated.    Locally stimulating fumigations, and weak injections of strychnine or veratrine have proved useful ; electricity has always done harm.—*British and Foreign Medico-Chirurgical Review.*

## ON OVARIOTOMY IN RELATION TO DISEASE OF BOTH OVARIES.    BY PROFESSOR VON SCANZONI.

PROFESSOR von Scanzoni, while admitting the great progress which has been made of late years in establishing ovariotomy as one of the great recognised surgical operations, calls attention to its inferiority as a means of radically curing a fatal disease, when compared with other important operations, as the Cæsarean section, lithotomy, the larger amputations, and the ligature of great vessels.    When the immediate danger of these has been triumphed over, the patient has a reasonable expectation of being cured of his disease ; but thus much cannot be said of ovariotomy, owing to the probability of the second ovary being diseased.    This point has been insufficiently inquired into, and the object of this paper is to call attention to it.    At present, great difficulty exists in drawing any conclusion upon the matter, owing to the few accounts which we have of the after-history of those who have been operated upon. Thus, in Dutoit's statistical work, embracing 324 cases of successful ovariotomy, in only 34 instances is the state of the patient declared two years after the operation.    No conclusion can be drawn from such insignificant numbers, and Professor von Scanzoni has endeavoured to throw light upon the matter by causing the register of the Würzburg Pathological Institute to be searched. In the course of fourteen years the autopsies of 99 cases of ovarian disease were recorded, and in 48 of these the disease was found existing on one side, in 51 on both sides ; and von Scanzoni is convinced that this statement does not represent all the cases of disease of the ovary brought to the institute, nor a sufficiently high percentage of double disease, this not being noted when slight and incipient.    At all events, in one half of the cases recorded the ovary was diseased on both sides ; and this point, with the probability of relapse it implies, has been too little regarded by surgeons.    These 99 cases are divisible into two groups, accordingly as the subjects had reached their fiftieth year or not ; 52 of the number were below and 44 above that age, this point not being indicated in three cases.    In the 52 cases both ovaries were diseased 31 times, or 59·6 per cent. ; and but one 21 times, or 40·5 per cent.    In the 44 cases, in 17 both sides, and 27 one side, were affected, or 38·6 to 67 per cent.    A conclusion to be drawn from these figures is that, as double ovarian disease is of so much more frequent occurrence prior to the menopausis, the danger of relapse is much greater before than after that period—a fact to be borne in mind in considering the indications and contra-indications of the operation. It may be said that if, during the operation, both ovaries are found diseased, they may be at once extirpated, and all danger of relapse obviated.    In Dutoit's statistics this double operation is said to have been executed in 25 cases, with 11 recoveries and 14 deaths ; and it is remarkable that this double operation should have been performed in so small a proportion of cases, seeing, from the statistics already adduced, that disease must have existed much

oftener in both sides. Either the diseased condition of the ovary must have been overlooked, or fears were entertained of aggravating the danger of the operation, or hopes were entertained that the disease would not undergo a dangerous development. However, the double extirpation seems to have been followed by bad results in the few cases in which it was undertaken, for there were only 44 per cent. recoveries to 56 per cent. deaths; while the entire number of ovariotomies recorded, 468, furnish 262 recoveries and 206 deaths, or 56 per cent. to 44—the figures being exactly reversed.

Professor von Scanzoni appeals to all those who may have the opportunity, and especially to the English surgeons, for their aid in a thorough examination into the point which he has raised in this paper.—*British and Foreign Medico-Chirurgical Review.*

#### IMPACTION OF A CALCULUS IN THE ANTERIOR WALL OF THE URETHRA, GIVING RISE TO SYMPTOMS OF STONE IN THE BLADDER. UNDER THE CARE OF MR PAGET.

A BOY, 4 years old, was sent up from the country, with the statement that he had been suffering for many months from all the usual symptoms of stone in the bladder; that he had been repeatedly and carefully sounded, but that no stone could be found.

Upon his admission the bladder was searched by means of an ordinary sound, which passed readily without meeting with any obstruction; nothing, however, was found. The child was now kept in bed, that his symptoms might be closely watched. At the end of a week it was clear that, whatever the case might eventually prove to be, everything pointed strongly to the presence of a stone either in the bladder or in some part of the urethra. He was, therefore, again sounded, and still with a negative result. After the lapse of a few days more, as the symptoms remained urgent, Mr Paget again sounded the boy, selecting, on this occasion, an instrument more sharply curved than those which had before been used. This was arrested in the membranous portion of the urethra, where it struck calculous matter apparently embedded in the anterior wall of the canal. The nature of the case now became clear. A stone had passed from the bladder into the narrow membranous portion of the urethra, where it had lodged, and subsequently so embedded itself (either in a pouch in the anterior wall, or in the depression of an ulcer which had been caused by its presence) that it left the canal clear, and lay concealed, and not to be detected except by a sound of a particular curve. This was, upon subsequent occasions, shown to be the explanation of the difficulty which had occurred; for it was found that the stone could be readily struck with this same sound, although others of a different curve passed it and went easily into the bladder. The stone was removed through an incision in the perineum, as if for median lithotomy. The child made a good recovery.—*Medical Times and Gazette.*

#### IMPACTION OF A STONE OF LARGE SIZE IN THE FOSSA NAVICULARIS, PRODUCING SEVERE SYMPTOMS OF ONLY FOUR DAYS' DURATION. UNDER THE CARE OF MR PAGET.

A patient, married, and about 35 years old, recently applied to Mr Paget on account of pain and swelling of the glans penis, and difficulty of micturition. His symptoms were of four days' duration only, and till within that time he had been conscious of no trouble of any kind in his urethra, or of any symptoms of stone either in the kidneys or the bladder. He was an active person, in the habit of taking strong exercise at cricket and in other ways. Upon examination, it was found that there was a calculus, apparently of considerable size, lying immediately behind the meatus urinarius. This was at once removed. It consisted of a uric acid nucleus thickly coated with phosphates; it was almond-shaped, and measured ten lines in its long, and four in its transverse diameter. When told to what his symptoms had been due, and asked whether he could form an idea as to the time the stone had been lodged in the urethra, the patient persisted in his statement that he had suffered no

inconvenience of any kind until within the previous four days. He added that five years before, he fell astride upon a gate over which he was climbing, and bruised the scrotum and body of the penis so severely that he was obliged to lie in bed for a few days, but he had no reason to connect that accident with his present trouble. The case seemed an extraordinary one, whatever explanation of it was attempted. It seemed very strange that so large a body could have been long lodged in the urethra without the patient's knowledge, or without producing urgent symptoms; yet probably this had really occurred, for the patient had felt no recent symptoms, either of the presence of a stone in the bladder, or of its passage down the urethra; on section, the deposit of phosphates was found to be much the thickest on that side of the stone which had been most exposed to the urine; the stone lay deeply embedded in a hollow in the urethral wall, which could hardly have been formed in four days; and it was smeared over with very fetid, cheesy, follicular secretion which had evidently been long retained. The incision of the glans, which it was necessary to make for the removal of the stone, quickly healed, and the patient had no farther trouble.

Mr Syme has reported a case very similar to this, the brief particulars being that a boy, 18 years old, was struck on the pelvis by a heavy piece of iron machinery. Extravasation of urine and extensive sloughing of the perineum followed, with the separation of a piece of bone at the end of a month. The patient came under Mr Syme's treatment eight months after the accident, suffering from symptoms of obstruction in the urethra and a perineal fistula, through which the greater portion of the urine was discharged. It was discovered that there was a tight stricture in the bulbous portion of the urethra, and immediately behind it a large cavity, surrounded with walls of cartilaginous hardness, and containing two portions of exfoliated bone, which together measured two inches in length and one in breadth. These were removed through a free incision made upon a staff on the left side of the perineum. The patient did well.—*Medical Times and Gazette.*

## Part Fourth.

## MEDICAL NEWS.

### MEDICO-CHIRURGICAL SOCIETY OF EDINBURGH.

#### SESSION XLV.—MEETING I.

*8th November* 1865.—Dr WARBURTON BEGBIE, *Vice-President,* in the Chair.

#### I. EXCISION OF OS CALCIS.

*Mr Annandale* brought before the Society a man from whom he had removed the entire os calcis, taking care to save the periosteum. The patient had been admitted into one of Dr Gillespie's wards in the end of July, with a sinus at the back of the heel, leading to a carious cavity in the calcaneum. Soon after, the part was attacked with hospital gangrene, which destroyed the tissues at the back of the heel. In Dr Gillespie's absence from town, the patient came under the care of Mr Annandale, who, finding that the condition of the soft parts prevented the performance of amputation at the ankle-joint, and thinking it a good case for saving the foot, excised the whole of the os calcis, at the same time saving the periosteum. The result, it would be seen, was, that the parts were now quite solid and sound. The incisions employed were those recom-

mended in Holmes' System of Surgery. Mr Annandale used Langenbeck's instrument to scrape off the periosteum, which was thrown back, along with the flap so as fully to expose the bone. It would be seen that the chief disease was situated in the posterior part of the calcaneum, where there was a carious cavity. The rest of the bone was somewhat enlarged.

### II. VASCULAR RECURRENT TUMOUR.

*Mr Annandale* showed a new kind of tumour, or one at least of which he had found no description, of which he had seen three well-marked cases under Mr Syme's care, and which he would propose to call *vascular recurrent*. The first case was that of a gentleman, from whom such a growth was removed eight years ago, a second time after two years, and again three years afterwards. Since the last operation, it had not returned. It had always appeared in the cheek, between the skin and mucous membrane ; it was somewhat elastic and compressible, and, on section, had a bluish-grey colour. In the substance of the tumour were numerous venous sinuses, varying from the size of a pin's head to that of a pea. On microscopic examination, the tumour presented all the characters of the recurrent fibrous tumour,—elongated and oval cells, with nuclei. In one of the cases (the second), some of the sinuses contained phlebolites. In two of the cases, the tumour recurred three times ; in the last, it had only been removed once.

### III. REMOVAL OF PORTION OF THE TONGUE BY THE ECRASEUR.

*Dr P. H. Watson* showed a specimen of epithelial cancer which he had recently removed from the tongue by means of the ecraseur. The disease had previously been removed by a practitioner in the country, but it had recurred. On carefully investigating the case, Dr Watson found the disease to be very limited, and the rest of the organ was healthy ; accordingly, he only removed the tumour and the portion of the organ from which it grew. The patient was put under chloroform, and the operation was very easily effected without any loss of blood. No doubt it would have been equally easy to have removed the growth by means of scissors, but Dr Watson had been anxious to test the instrument, with a view to determining whether the whole tongue might not be removed by means of two ecraseurs. He was now quite satisfied that this might be done. The ecraseurs would cut quite as accurately as the knife, and additional security might be given by the use of stout needles to regulate the direction in which the parts were cut.

### IV. PHOSPHATIC CALCULUS.

*Dr P. H. Watson* showed a phosphatic calculus which he had removed from a patient in Chalmers' Hospital,—a man between fifty and sixty years of age. The patient had suffered from symptoms of calculus for two years. There was much irritation of the bladder ; the urine contained much ropy mucus. The patient was kept at rest, and treated with *triticum repens* and muriate of ammonia till the irritation subsided. On introducing the lithotrite sound, in order to determine the dimensions of the calculus, Dr Watson caught the stone by the short axis, when it seemed so small that he determined to crush it. On seizing it with this view with the lithotrite, he got hold of the stone in its long axis, and then, from the irritability of the bladder and size of the stone, he thought it better to perform lithotomy. The stone was easily removed ; the only peculiarity was the great depth of the perineum : it was so deep that, on introducing his finger (which, he might state, was four and a-half inches long), Dr Watson could only reach the prostate, being quite unable to introduce it into the bladder. Three days after the operation, the whole of the urine was discharged by the natural channel.

### V. FOREIGN BODIES SWALLOWED BY AN EPILEPTIC.

*Dr Beilby* exhibited three marbles which had been swallowed and passed by a boy thirteen years of age. The boy was subject to epilepsy, and had a strong

propensity to swallow foreign bodies.  Dr Beilby had often had to treat him on this account, and might mention that he had found emetics quite useless. On this occasion, he had left the case to nature, causing the evacuations to be examined.  Five marbles had been swallowed, but only three had been recovered.

### VI. ON THE RETENTIVE POWER OF THE ABDOMEN.

*Dr Matthews Duncan* read a paper on this subject, which will be found at page 516 of this number of the Journal.

### VII. ACUPRESSURE IN TEN CASES OF AMPUTATION.

*Dr M'Kinlay* read a paper on acupressure, stating the results of his employment of that hæmostatic means in ten amputations.  In none of his cases was there the least degree of secondary hæmorrhage, nor any tendency to pyæmia.  All the wounds healed kindly and rapidly.  In one case of amputation in a woman very far advanced in life, the resulting wound healed entirely by the first intention.  He was induced to try accupressure in the first instance in consequence of meeting with a case where, after amputation below the knee, he attempted, in vain, all means of ligaturing a retracted interosseous artery, but at once secured this end by employing acupressure with a darning needle.  He now had recourse to acupressure instead of the ligature in all the surgical wounds he met with besides amputation ; and after much experience with this new method of arresting hæmorrhage, he had been forced to the conclusion, that it was simpler, safer, and better than deligation.  In his surgical pocket case he carried now the means of using acupressure instead of the old means of applying ligatures.

*Dr Gillespie* remarked that the subject of acupressure had been pretty well argued within these walls.  His own opinion was that the advantages of acupressure had been considerably overrated, or at least that the ligature had been decried, in order that the other method might be exalted.  One of the chief reasons on account of which it was recommended to give up the ligature was the alleged less chance of pyæmia when acupressure was employed.  So far as his own experience had gone, and so far as he knew, pyæmia was just as likely after the one as after the other.  There were also cases in which it was very difficult or impossible to apply the needles, as where flaps after amputation were very thin, or where the vessel was very near the end of the stump.  Dr Gillespie's opinion was, that both plans were useful ; in some cases, the ligature was best ; in a good many, acupressure was the most desirable, especially in the case of the smaller arteries.  Dr Gillespie thought that there was some risk in applying acupressure to the femoral artery in the case of amputations of the thigh ; he had once, under these circumstances, left a needle in five days, and there had been secondary hæmorrhage.

*Dr P. H. Watson* considered the Society indebted to Dr M'Kinlay, both for the cases recorded and the opinions expressed.  The mind of the profession as to the merits or demerits of acupressure must be made up, not from a consideration of abstract statements and arguments, but from the accumulation of practical results.  At one time, Dr Watson had thought that acupressure did not possess any special advantages, and that it was open to certain theoretical objections.  What first induced him to try it was because friends of his own had employed it, and found it perfectly successful.  He believed, therefore, that the accumulation of experience from different quarters, such as that afforded the Society by Dr M'Kinlay, was the certain means of creating a general feeling of confidence throughout the profession in this country in the employment of this hæmostatic agent.  Dr Watson's own experience had early convinced him that acupressure possessed certain advantages, and the only question with him then came to be, whether it should be employed in all cases, or only in some.  Dr Gillespie, and others, said that though acupressure might be suitable in certain cases, it was not so advantageous in others.  It might be so, but Dr Watson had not found anything in its employment in recent wounds to make

him doubt its easy adaptation to any case. Some thought that acupressure was not adapted for such large vessels as the femoral artery. His friend, Mr Brown, of Carlisle, had lately told him, as the result of his extensive experience, that, of all vessels, the femoral artery was the one to which acupressure could be most easily and successfully applied. In judging of the success or non-success of acupressure, considerable account should be taken of the persons who employed it, and of the method adopted by them. It was hard, for example, that acupressure should be blamed for disastrous consequences, which were entirely due to carelessness in its application. It was also certain that many who had professed to try acupressure, had performed it, not according to Dr Simpson's directions, but according to their individual ideas. He had heard Dr Simpson's method held up to abuse on account of failures of a procedure which was supposed to be acupressure, but which had nothing in common with it but that a needle was employed; and the wonder to his mind was, not that such measures failed in success, but that they were attended with anything but failure. Since Dr Watson had begun to employ acupressure, so well satisfied was he with its effects and the general facility of its application, that he had only resorted to the ligature in two cases, and in both the bleeding came from an already suppurating surface. The *first* was a case where Mr Spence had tied the carotid; profuse and repeated arterial hæmorrhage came on at the time of separation of the ligature. Dr Watson saw the case in Mr Spence's absence from town, and on opening the wound he found the artery bleeding from the proximal end. Pressure was applied with the finger to the bleeding point, and the innominata was laid bare, but was too much enlarged to make it safe to tie it. Dr Watson then cleared the root of the carotid, and laying hold of it with dressing forceps, the bleeding was thus controlled until the open end of the vessel was laid hold of with artery forceps and tied. Acupressure could not have been employed here, as there were no textures into which the needle could be passed so as to compress the carotid from without towards the spine, and as the vessel was too much enlarged at its root to permit the aneurism needle to pass readily round it, still less could the acupressure needle safely be passed behind its channel. In the *second* case, there was secondary hæmorrhage from an ulcerated opening in the posterior tibial, and the bleeding vessel lay exposed upon the surface. The wound was narrow, a ligature was passed above and below the bleeding point, and the vessel tied on either aspect of the opening. In these cases, Dr Watson considered it quite immaterial, so far as any principle was concerned, whether the ligature or the needle was employed, as there was no chance of healing by the first intention. Now, it was not so much as a means of arresting bleeding, but as a measure calculated to favour primary union, that acupressure had been introduced by Professor Simpson. The important question, therefore, and one which experience could alone decide was, what results in that respect did acupressure show? There could be no doubt, theoretically, that there was a far greater chance of wounds healing by the first intention when acupressure, rather than the ligature, was employed; while, practically, his experience showed that in cases where we could scarcely expect union by the first intention to take place, it had occurred under the use of acupressure. In a case, for example, of excision of the knee-joint, complete primary union was obtained. In one of the first cases where he had employed acupressure—one of amputation of the forearm—if union by the first intention was not absolutely obtained, there was as close an approach to it as possible. Dr Watson must remark, in passing, that certain wounds of the face, which are generally referred to as a standard when allusion is made to healing by the first intention, do not really unite in so absolute a manner as is sometimes supposed; for though the external aspect of the incision heals, there is often suppuration on the inner surface which escapes observation. In a case of excision of the upper jaw which he had performed, and in which smart hæmorrhage came on, and the wound had to be laid open, he found that, though the external aspect of the cheek incision was closed, the internal was not. In attempting to obtain primary union of clean cut surfaces, it was not, however,

sufficient to employ acupressure and wire sutures, and to rely on these measures alone for the desired result, nor to suppose that these metallic agents were a failure because primary union was not obtained. A very important point to attend to in endeavouring to produce primary union after amputation, was to keep the deep parts of the flap in accurate apposition. With the view to attain this, he had, in some cases, passed long needles through and through the deep parts of the flap, securing the deep wire sutures thus introduced by pads of cork, so that the raw surfaces might be retained in contact throughout the whole depth, and vessels too small for the ligature or acupressure might be stopped by pressure. Dr Watson had also employed this method most successfully in the wound resulting from extirpation of the testicle, while in cases where this was unsuitable, as in some "single long flap" amputations, the employment of a plaster of Paris bandage enveloping the whole stump, leaving open only the line of union of the margins of the cut surfaces, had seemed to him to afford no less satisfactory effects in the way of maintaining apposition of the entire wound, and complete repose of the elements, muscular and osseous, of the stump. Of these procedures, intended as adjuvants to acupressure, his experience was as yet too limited to enable him to speak dogmatically. But before sitting down he would desire to express his firm conviction that acupressure should be adopted; and he would recommend those who at present spoke against it, on merely theoretical grounds, to make themselves practically acquainted with it.

*Professor Simpson* observed that Dr Gillespie had complained that the ligature had been decried by the supporters of acupressure. The only way in which he had decried the ligature consisted in citing the decided and deliberate opinions of the most celebrated surgeons and pathologists to the effect, that it inevitably strangulated and sloughed, as a general law, the part of the artery to which it was applied, and that no union could possibly take place along the course of its track outwards. An eminent operator, when lately discoursing on the progress of surgery at Leamington, is declared to have stated that the ligature occasions no irritation nor gangrene, though he daily kills internal hæmorrhoids, etc., by it; and though, in the language of Mr Syme, it, when applied to an artery, "at once deprives the part embraced within its noose of vitality." A late perplexed writer upon, or rather against, acupressure, in the British and Foreign Medico-Chirurgical Review, charges Dr Simpson with "unpardonable" misrepresentation in stating that arterial ligatures left minute morsels of dead flesh within the cavities or upon the raw sides of large wounds; and yet the very same author, in the strangest spirit of contradiction to this most unwarrantable observation and language of his, admits it, in the very same page, as "unquestionable" that "the small part of the artery below the situation of the ligature must die in almost every case, and be cast off as a slough, and that in every case the ligature itself must be loosened from the tissues embraced in it by a process of ulceration and suppuration." Among his published cases of acupressure, Dr Watson had cited an instance in which, after resection of the knee by him, the prodigious flesh and bone wound thus left had united entirely by the first intention. Dr Watson himself had alluded to the case to-night, and it was certainly, at the present time, one of the most successful in the records of surgery. The Medico-Chirurgical Reviewer had seen Dr Watson's paper and comments on it, and yet remarks :—"Who ever saw, or who ever expects to see, the wound of a resection unite by the first intention." "No one," he observes, "would venture to expect primary union in such a case (as resection of the knee), nor would recommend any treatment as likely to effect that end." This end, however, was attained in this operation of Dr Watson's by acupressure, and other means ; and yet it appeared, to the truly surgical mind of the Reviewer, more logical to disbelieve the authenticity of Dr Watson's report of a case treated publicly by him, and seen and watched by many, than to believe in a result so much beyond all his own foregone expectations and prejudices. The whole is surely a great compliment to Edinburgh surgery,—that it has attained a result which a London surgeon had

deemed impossible. In wounds actually suppurating, as in one of the cases alluded to by Dr Watson in his speech, or in any wounds which must inevitably heal by the second intention, it was of course a matter of little, or indeed of no moment whether the ligature or acupressure was employed; but in any large wounds intended to be healed by the first intention, acupressure was most undoubtedly to be preferred; and late experience had abundantly shown that it could be applied, under one form or another, in all such circumstances. He had published several cases of amputation of the limbs where the wound had united completely by the first intention. Entire union of that kind in amputation-wounds was impossible before the days of acupressure, could not be obtained by the ligature, and consequently cannot be found in the anterior records of surgery. His esteemed friend, Professor Pirrie, of Aberdeen, had long held that no wound should be said to be healed by the first intention where one single drop of pus was visible. Yet, in the interesting series of cases which he had published of acupressure, Professor Pirrie points out that three of the thirteen cases he describes united by primary union. And he and his brother surgeons of the Aberdeen Infirmary now used, he believed, acupressure as the general rule, and the ligature as the exception. Dr Kerr, of Aberdeen, still employed the ligature, and told Dr Simpson, when he met him a few weeks ago in consultation on a case in the North, that in one instance lately of amputation of the thigh in his practice secondary hæmorrhage had come on after the separation of the ligature, and he was considering the propriety of deligating the trunk of the femoral artery, when it was suggested to him to close it by acupressure, which he did successfully, and without any use whatever of the knife. In this, as in various other cases, acupressure can be practically applied with much greater facility and safety than the ligature. In cases, for example, of wounds of the wrist and palm of the hand, it was necessary when the ligature was used to make a dissection—and often a difficult dissection—down upon the bleeding vessels. But now, in many such cases, acupressure had been applied with perfect facility and success, and without any dissection or cutting whatever. Dr Gillespie doubted if pyæmia did not occur as frequently after the needle as after the ligature. Dr Simpson had no doubt that betimes, when sufficient statistics had been collected, the result in this respect would be found greatly in favour of the needle, which was removed in a few hours, and left nothing in the wound, whilst the ligature left the very elements of pyæmia there, in the form of dead sloughs and infected purulent threads. In any contrast, however, between the ligature and acupressure, it must, as Dr Watson observed, be held in view that cases were included under the name of acupressure which scarcely belonged to that category, as where the needles, instead of being removed within two or three days at most, were left in for five or ten days, till they necessarily cut through the obliterated vessel, with an inevitable amount of ulceration and suppuration. In one or two distant hospitals he had seen cases of this kind, where the needle was left so long in as to have all the bad effects of the ligature. He had heard of other cases where the tube of the bleeding vessel had been intentionally and with difficulty transfixed by the needle, and where bleeding had almost necessarily supervened when it was withdrawn a day or two subsequently. These cases, however, were truly cases of the acupuncture, and not of the acupressure of arteries. But even with acupressure more points must be attended to than the use of the needle, to give the patient a just chance of that most desirable of all objects, namely, the complete union of wounds by the first intention. The flaps must be shaped so as to allow the sides of wounds to lie easily together, and these sides, and even sometimes the floors of the wounds, must be kept together by sufficiently deep and broad metallic stitches, so that the whole cut surfaces were everywhere in accurate contact, instead of being brought together—as they had all too often seen—by a few cutaneous and most superficial stitches only. And, again, all touching, moving, and dressing of flesh wounds must, he believed, be as sedulously avoided as the handling of bone wounds. He had seen gentlemen who would have stood utterly

aghast at any one moving and twisting a uniting fractured bone, have no hesitation in touching, handling, and pressing together the sides of a flesh wound, which they yet affected to expect to unite by primary union. The cementing material in the one wound is as friable and lacerable as in the other. In these, and in other respects, much advance and improvement would, he believed, be made within the next ten or twenty years; and there were few departments of practice that required more real reform than the management of large wounds intended to be united by the first intention. Dr Watson had already worked most successfully and ingeniously in this direction, and he, no doubt, would be followed betimes by others.

---

# PROCEEDINGS OF THE EDINBURGH OBSTETRICAL SOCIETY.

## SESSION XXIV.—MEETING XIII.

### 14th June 1865.-- Dr Pattison, Vice-President, in the Chair.

#### I. ON A CASE OF MALFORMATION OF THE FEMALE ORGANS OF GENERATION.

*Dr Sanderson*, Musselburgh, stated that he had lately had an opportunity of examining a woman who came to him to see whether he could do anything for her under distressing circumstances. Her husband had left her on account of something being wrong. On examination per vaginam, that passage was found to terminate in a cul-de-sac, about 2½ to 3 inches in length. No uterus could be found either on examination by the vaginal passage or per anum. She was a stout person, with mammæ well developed, and in every other respect well formed.

*Dr Bell* remarked on the medico-legal bearing of such cases in reference to divorce. He understood that, if the passages were sufficient to afford intercourse, a divorce could not be obtained, notwithstanding such malformation as to prevent the possibility of child-bearing. A divorce, also, could only be obtained after three years' marriage in such cases.

*Dr Keiller* had seen several cases like the one related by Dr Sanderson; one, indeed, was very similar in regard to the malformation. In one case, the woman was generally well developed; had large mammæ; was anxious to be married. The cause of her applying to him, indeed, being that she had never menstruated, and her anxiety to know whether she ought to marry. He examined her, and found exactly the same condition as in Dr Sanderson's case. He believed Dr Bell to be correct in his statements regarding the medico-legal question, but the concealment of the knowledge of a sexual deformity was sufficient ground for an immediate divorce.

#### II. ON A UTERINE SUPPORT.

*Dr Keiller* exhibited a uterine support which he had got from Dr Ritchie, of Glasgow, and which Dr Ritchie requested him to show, and to state to the Society that, if not of exactly the same form, it was in principle similar to the pessary recently exhibited and recommended by Dr Charles Bell. Like the instrument of Dr Bell, it consisted of a pelvic portion or circular strap, the front of which was so arranged that it firmly embraced the pubis, and received a pliable stem, on the extremity of which was fixed a globular pessary, which could, by means of the socket in the bandage, or pelvic part of the instrument, and the pliable nature of the connecting stem, be readily enough made to fit, in length or direction, any case. Dr Ritchie, from having years ago published a notice of his instrument in the Medical Journal, naturally enough claimed precedence in this matter, although there could be no doubt Dr Bell's pessary was quite original, like that of Dr Ritchie's. Dr Keiller pointed out their individual characteristics, and approved of the principle of both, which, to his mind, combined simplicity with efficiency. The distinctive character of Dr Bell's instrument was its comparative *fixedness*, which was

of great consequence, and which was mainly managed by the firm attachment given to the pessary-stem through the well-fitting and more firmly-fixed pelvic or supporting strap.

Dr Bell remarked that the instrument now exhibited by Dr Keiller as that invented by Dr Ritchie, was not similar to the one he (Dr Bell) had constructed and recommended, and he begged to say that he had not before seen Dr Ritchie's pessary, nor was he at all aware that a notice of it had been published. To enable the members of the Society to see the difference between his instrument and the one now exhibited, he would again show the one he had invented at the next meeting, when the members could judge for themselves.

---

SESSION XXIV.—MEETING XIV.

26th June 1865.—Dr PATTISON, Vice-President, in the Chair.

### I. ON UTERINE SUPPORT.

Dr Charles Bell again exhibited his new uterine support, and pointed out the difference between it and that claimed as the invention of Dr Ritchie, of Glasgow.

Dr Keiller observed that he agreed with Dr Bell in regard to the more solid and otherwise more firm construction of the circular or pelvic portion of his instrument, as compared with that of Dr Ritchie, but, at the same time, thought there could be little doubt as to the general mechanism and principle being somewhat the same in both. From what had been stated, and from what the Society otherwise knew, the novelty and credit of the invention were both due to Dr Bell and Dr Ritchie, and experience would soon test their practical value.

### II. CASE OF OVARIOTOMY.

Dr Moir showed an ovarian cyst, which he had that day removed from a patient. The operation had been well borne, and the woman, when Dr Moir saw her that evening, was in a very favourable way. He merely wished to show the cyst while fresh, the history of the case, of course, being yet incomplete. He would merely state that the means adopted to secure the pedicle was by tying it with wire, and returning it into the cavity of the pelvis.

Dr Keiller remarked that this method of securing the pedicle had been adopted in a case at which he assisted. After the operation was concluded, and the last suture was being put in the abdominal wall, a great gush of blood welled up from the pelvis. The sutures were at once undone, and the pedicle again raised, when it was found the bleeding was not from that source, but from some part in the pelvis. With some difficulty the hæmorrhage was checked, but the patient died the following day. No post-mortem examination was allowed, but he suspected that the twisted end of the wire securing the pedicle had torn a vein in the pelvis. He mentioned this case to show the care requisite in this method, which had certainly many advantages over the clamp, and which he believed was now adopted by several operators.

### III. A NEW UTERINE SUPPORT.

Dr Main, Lasswade, gave an account of this instrument, which will be found at page 545 of this number of the Journal.

Dr Charles Bell considered the instrument very ingenious, but he thought there was a want of resistance in the simple wire; this objection, however, might only be theoretical, and the support might be found to answer well in practice.

Dr Bryce, Dalkeith, stated that he had tried this support in two cases. The one was for retroversion in an elderly female; the uterus was completely turned upon itself, so much so that what proved to be the fundus was suspected by a medical man who had seen the case to be a tumour. The cervix was very much thickened. After rectifying the position, one of Dr Main's pessaries was fitted

on, the cervix resting within the ring at the upper part.   This support kept the uterus in perfect position ; but, after it had been worn for some time, the vagina became so irritable that it had to be withdrawn.   The second case was that of a young unmarried female, who had anteversion of the uterus, which greatly impeded micturition, and prevented her following her usual duties.   He (Dr Bryce) had introduced a galvanic pessary, but it would not remain in.   Dr A. R. Simpson had kindly seen the patient with him ; and, after some difficulty, on account of the great degree of flexion, again introduced the intra-uterine pessary, and placed a gutta-percha shelf-pessary below, to support the other. This, however, also failed to keep the pessary in its place.   He (Dr Bryce) afterwards employed Dr Main's wire-support, fitted on to the galvanic pessary, and by this means the uterus was kept in perfect position.

*Dr Keiller* remarked that, when in the Royal Infirmary, he had worked for a long time at pessaries, and, with the view of deciding as to their comparative advantages, had used all forms of them.   He had constructed and tried numerous shapes and sizes of gutta-percha pessaries for supporting the various forms and degrees of uterine and vaginal prolapsus without the external aid to the internal support, as lately suggested by Dr Bell and Dr Ritchie, and again, in another form, by Dr Main.   The great objection to pessaries in general was their bulk ; they did not allow of the contraction of the passages, which was so much to be desired. Now, Dr Main's instrument had not this objection, and was an instrument likely to be of service.   But he doubted whether the cervix would be found to keep its position in the gutta-percha ring.   He feared it would be apt to become displaced ; and, in the case of a divided os, as when a woman had born children, the ring might pass between the lips.   Were this to happen, as he had seen where a ring pessary was used, the two portions of the cervix might become greatly separated and elongated.   He mentioned this as a result which he had seen happen, by the edge of the pessary getting between the uterine lips.   One important point to be attended to in prolapsus was to support the anterior wall of the vagina, which, in the form of cystocele, was of very common occurrence.   He would repeat that most pessaries tended to dilate the passages, but Dr Main's pessary, in this respect, seemed correct in principle, and was likely to be found of much service.

*Dr Main* and *Dr Bryce* stated, with reference to Dr Keiller's remarks, that, in the cases where they had used the support, they had found the cervix to retain its position, and had never found it become displaced.

*Dr Stephenson* remarked, that the pessary now before the Society had, to his mind, two great recommendations,—great simplicity and cheapness.   Those who devoted their time to the treatment of uterine diseases among the poorer classes know the great difficulty often experienced of supplying proper support, especially where external aid was required to an internal instrument.   The expense of Dr Bell's and Dr Ritchie's pessaries placed them out of the question, but here was one which could easily be made for a few pence.   To country practitioners, also, it would be of great benefit, for they could readily make it for themselves.   The wire could easily be silver-plated, which would add to the elegance of the instrument.

---

SESSION XXIV.—MEETING XV.

12*th July* 1865.—Dr BELL in the Chair.

I. CASE OF MISSED LABOUR.

*Professor Simpson* exhibited a fœtus which had been retained *in utero* for more than twelve months.   He had seen the patient, with Dr Finlay, of Newhaven, six days before her death.   She had expected to be confined in the end of January or beginning of February,—the last menstruation having taken place nine months previous to that period,—and had engaged a midwife to attend her.   The anticipated confinement, however, did not take place.   On the 13th of April, Dr Finlay was sent for in consequence of the discharge of a fleshy and

putrid substance from the vagina. Mixed up with the mass, which Dr Finlay believed to be a placenta, there was something like an umbilical cord. For eight or ten days previous, a bloody and watery discharge had been escaping from the vagina. The expulsion of the mass was unaccompanied by pain, nor, indeed, did the patient experience pain throughout the course of her illness. On the 8th of May, Dr Finlay saw her again, and brought away from the vagina a fœtal tarsal bone. Professor Simpson visited the poor woman that evening with Dr Finlay, and passed a sound several inches into the uterus, its top coming in contact with bony matter, and fixing the diagnosis. The os uteri was almost quite closed; an intolerably offensive grumous discharge was coming away from the vagina, and the pulse was very rapid and weak. He recommended that ergot of rye should be given, and that if it failed to excite uterine contractions, the os uteri should be dilated with sponge-tents, and the fœtus extracted. Sickness and vomiting supervened in a day or two, and the patient sank, and died on the 14th of May. She was in her 26th year, and had born two children at the proper time,—the first labour having been a natural one, and the second complicated with placenta prævia. On post-mortem examination, the fœtus was found lying with its nates downwards, and its head doubled in on the chest and abdomen, and was squeezed into such a firm mass that at first it seemed impossible to distinguish one part from another. It was converted into a substance resembling adipocere, and had a highly offensive odour. The uterus was firmly adherent to the abdominal walls in front and at the sides, to the small intestines behind, and to the colon above. An opening, rather larger than a half-crown piece, connected the transverse colon with the uterine cavity. The walls of the uterus were almost as thin as parchment, and consisted of areolar tissue, mixed up with unstriped muscular fibre, in a state of fatty degeneration. Professor Simpson stated that cases of missed labour— as they had been termed by Dr Oldham—formed one of the rarest of all the forms of morbid parturition in the human female, for not more than perhaps a dozen were to be found in all the records of obstetric medicine. Dr M'Clintock had published a few interesting cases of this class in the Dublin Quarterly Journal for last year. Though missed labour occurred so rarely in the human subject, it was not very uncommon in the sheep and cow.

*Dr Keiller* had seen a case of the kind referred to by Dr Simpson. The patient was some time ago sent into one of Dr Keiller's wards in the Royal Infirmary, as one labouring under malignant disease of the uterus. There was very fœtid discharge, and, in consequence of this and other symptoms, as if indicative of cancerous degeneration, the patient stated that she had been frequently "burned with caustic" for its removal. Dr Keiller discovered, on examination, the cause of the fetor, which was the slow putrefactive process going on during the breaking up and expulsion of a long-retained dead fœtus. Dr Keiller was led to the properly diagnosing of this case by finding small fœtal bones discharged from, and lying in, the passages. A number of these bones were collected, and were now in Dr Keiller's museum.

*Dr Young* had a case under his charge many years ago, where several bones had been passed; but he had kept no notes of the case.

### II. RECENT CASES OF MONSTROSITIES IN SHEEP.

The *Secretary* read a letter from Dr Grierson, of Thornhill, on this subject.

*Dr Keiller* exhibited a specimen of a double-headed sheep, and also a preparation of an abortion of a human monster, about the third month, united by the sides, with separate heads.

### III. REMARKS ON PHLEGMASIA DOLENS.

The *Secretary* read a paper communicated by Dr Shepherd, of Eccles.

### IV. CASE OF CRANIOTOMY.

*Dr Cairns* read the following notes of the case:—

I first saw the patient to whom this case refers in the beginning of January of the present year. At that time she complained of nausea, sickness, and

general debility.   She stated, on inquiry, that she was 24 years of age—was married, had never had a child, had aborted four times between the fifth and sixth months of pregnancy, and had last menstruated before seeing me in the month of October.   I found her of slender habit—nervous-sanguine diathesis. The thoracic region was very narrow, but the pelvis was well formed.   I could detect no lesion in the lungs, but there was a well-marked bruit with the first sound of the heart, loudest at the base, and communicated to the vessels of the neck.   This bruit still continues.

The characteristics of pregnancy were sufficiently marked to warrant me in telling her what I thought her condition was.   To allay the irritability of the stomach, I prescribed a few bismuth powders, which had the desired effect.   I then put her on chlorate of potash, and enjoined good nourishing diet, with daily gentle out-door exercise.   I have made it a point to see her nearly every alternate day since she first called upon me seven months ago.   About the seventh month of pregnancy I made a stethoscopic examination, and found the fœtal heart in what seemed to me a good condition, and urged her to submit to the induction of premature labour.   She would not submit, owing, as I have learned since, to the advice of some of her friends.   I made frequent examinations after that date, but found the pulsations become gradually weaker.   Matters went on in this state till the patient was within three weeks of her full period. I was summoned on Tuesday night last week to attend her in alleged labour, but on calling, and waiting with her for about two hours, no pains occurred.   On calling again on the following day, the pains were observable, but very irregular, the shortest interval between them being about half-an-hour, and without producing the least effect upon the os.   I administered 30 drops of sol. mur. morph., and called again on Thursday about four P.M., when the pains were recurring every quarter of an hour, and the os was dilated to the extent of a half-crown piece.   I then was enabled to detect a hydrocephalic condition of head, which presented.   Having a small trocar and canula in my pocket, I perforated the head, but no fluid escaped; and after waiting for half-an-hour without seeing any progress made, I hastened off for Professor Simpson, who, with his characteristic readiness and kindness on such occasions, accompanied me to render his assistance, and, having confirmed my diagnosis, at once performed the operation of craniotomy, and in a few minutes delivered the fœtus which I now place before you.   This is the sixth day since the patient's delivery, and she is now enjoying better health than she has done during the seven months I have known her.

### V. CASE OF TUBERCULAR PERITONITIS.

*Dr Keiller* exhibited a preparation from a case of tubercular peritonitis.   The patient, a young girl, had just died under his care in the Sick Children's Hospital.   She had previously been treated in the Royal Infirmary, and various views had been entertained regarding the nature of the case.   By some it was diagnosed as a case of fæcal accumulation, by others as mesenteric glandular disease; but the nodules distinguishable through the abdominal wall during life were found to be tubercular thickenings of, and nodosities in, the parietal or more superficial portion of the peritoneum.

### VI. POLYPUS OF UTERUS.

*Dr Keiller* exhibited a polypus arising from the cervex uteri of a patient who had died after the usual symptoms of pyæmia, although no operative measures had been adopted.   The case was in this respect interesting, for had the fatal symptoms not manifested themselves before the intended surgical removal of the polypoid tumour, any operative procedure, followed by such a result, would in all probability have been looked upon as the direct cause of death.   The os uteri was occupied by a partially extruded tumour, the pedicle of which being narrow, and slightly attached to the inner surface of the anterior lip, could have been readily detached by torsion, or other comparatively simple means.

## VII. CASE OF EMBOLISM AND FLOATING LIVER. (?)

*Dr Keiller* related the particulars of a case of fatal embolism which he had lately been called to see in the country. The lady was advanced in years, and her death was somewhat sudden. On post-mortem examination, a large clot was found firmly impacted in the pulmonary artery, which sufficiently accounted for the mode of death, and the immediately preceding symptoms. There was seen, at the dissection of this case, what he (Dr Keiller) looked upon as a most interesting and important pathological condition of the liver, which at first sight seemed to consist of a distinct and movable tumour. The hepatic mass was so elongated and pendulous, and, in position and touch, so like what during life might readily, and in all probability if detected would, have been put down as a *movable or floating kidney*, that he (Dr K.) would report the facts of the case more fully on a future occasion.

## VIII. CASE OF AMAUROSIS.

*Dr Murray* stated that the patient whose history he lately laid before the Society, he was glad to say, was greatly improved. She could now see to thread a fine needle.

---

## THE MACKINTOSH CASE.—TESTIMONIAL TO DRS SMITH AND LOWE.

Our readers will recollect that a series of actions was brought by Mr Mackintosh of Holme against those whom he charged with having been accessory to his illegal detention in a lunatic asylum. The last of these actions was brought against Drs Smith and Lowe, the proprietors of Saughtonhall Asylum. There were peculiar hardships in the case of this action, for not only had Mr Mackintosh been received as a patient in virtue of a regular warrant, but the action was not raised for a number of years after the alleged illegal detention had taken place. In the subsequent proceedings the defendants were successful; but even though they gained their cause, a very considerable expense was incurred. The medical profession of Edinburgh, desirous of testifying to Drs Smith and Lowe the sympathy they entertained towards them, collected a sum of money to aid in defraying their expenses, which, a short time ago, was handed to Dr Smith by Dr Burt, President of the Royal College of Physicians. The subjoined correspondence will explain itself.

Physicians' Hall, Edinburgh, 17th November 1865.

My dear Sir,—You cannot doubt that you and Dr Lowe have had the widespread sympathy of the profession in your long and vexatious litigation with Mr Mackintosh of Holme, and that the termination of the case in your favour has given universal satisfaction.

A few of your brethren have entertained a strong desire to lessen in some degree the pecuniary loss you have sustained, and with that view have contributed to the best of their ability, and it gives me great pleasure to be the medium of conveying to you the amount subscribed (£190, 10s.), with their best wishes for your future happiness, and the prosperity of the admirable institution over which you and Dr Lowe so ably and so skilfully preside.—Believe me ever yours faithfully,                                                 John G. M. Burt.

Dr John Smith.

Saughtonhall, 20th November 1865.

Sir,—We shall esteem it a favour if you will insert the enclosed copy of letter in the next number of the Edinburgh Medical Journal, and will at the same time allow us to express to our professional brethren the gratitude we feel for their handsome testimonial. Were we merely to acknowledge their very liberal subscription of the sum of £190, we should do so with feelings of the utmost gratitude; but even this sum, large as it is, sinks into insignificance in comparison with the pleasure we must ever feel at this general and unequivocal testimony on the part of those whose good opinion we highly prize. We accept it

as an acknowledgment of their sense of the injustice by which we were made the defendants in a prosecution, which was not only most uncalled for in the first instance, but which was allowed to sleep for twelve years before it was submitted to a public trial.   At the trial we had the satisfaction of being supported by the Scottish Courts of Justice, and eventually by the unanimous decision of the House of Lords.

Satisfactory as their decisions were, after a long and protracted trial, attended, we need hardly say, with great anxiety and expense, we have now to acknowledge a still higher satisfaction in this expression of the opinion of our professional brethren; and we beg most cordially to express the feelings of gratitude we must ever feel towards them for this unlooked for act of liberality and kindness.—We beg to remain, Sir, your very obedient servants,

*The Editor of the Edinburgh Medical Journal.*          JOHN SMITH.
                                                        WM. H. LOWE.

---

# REMARKS ON THE MEDICAL EVIDENCE IN CASES OF DEATH BY SUFFOCATION,

*With reference to the case of Mary Baird or Downie, reported in the Edinburgh Medical Journal for November* 1865.

THE reporter of Downie's trial remarks as follows :—" The following case of infanticide presents several interesting features from a medico-legal point of view, chiefly as illustrating the difficulty of arriving at anything like precise conclusions as to the cause of death of this nature from post-mortem examination of the body of the child."

However apposite these remarks might have been ten years ago, I think that since 1856 they are no longer true or appropriate; for such proofs do certainly exist, and may be found, provided that suffocation proper has been the cause of death, and provided that the body has not passed into a too advanced state of decomposition, or has not, from other causes, had its anatomical features destroyed.

This I now proceed to show.   In 1855, Dr Ambroise Tardieu, of Paris, a gentleman of the greatest experience and skill in medical " expertise," published " A Memoir upon Death by Suffocation,"[1] from which I proceed to make excerpts narratively, without, in all cases, quoting literally.

He relates that, in 1847, Bayard, in his " Manual of Legal Medicine," notes in a few lines :—" The presence of disseminated punctuated ecchymoses, under the pulmonary pleura, in infants who have succumbed under a more or less complete occlusion of the air-passages; but in associating this character with the " ensemble " of signs of death by suffocation, he does not indicate the *constancy* of its appearance.

Tardieu defines " suffocation " to comprehend all those cases—apart from submersion, hanging, or strangulation—in which a mechanical obstruction is violently opposed to the entrance of air into the respiratory organs.

This obstruction may be caused by compression of the thoracic and abdominal parietes, *direct occlusion of the mouth and nostrils*, the introduction of foreign bodies into the air-passages, forcible sojourn within a space too confined, and burying in the earth or other pulverulent medium; and, he adds, " if any of these causes act without sufficient energy or persistence upon a living being, death by ' suffocation' will result."

In all such cases, of which Dr Tardieu has seen examples in criminal affairs as well as in his own proper experiments, he has found " common lesions "— " essential and fundamental characteristics "—*internally*—over and above the " secondary signs which have resulted from the different circumstances in which suffocation has been produced."

---

[1] Memoire sur la Mort par Suffocation, par le Docteur Ambroise Tardieu, professeur agrégé à la faculté de médecine à Paris, médecin de l'hopital la Riboisiere.   Chez J.-B. Baillière, 19 Rue Hautefeuille, à Paris.   1855.

Amongst the " lesions" which have been met with in individual children and adults, and also in animals which perished suffocated, the most important, and the " *only constant* lesions," are (contrary to received opinion, not traces of external violence, but) " lesions which have their seat in the internal organs, and notably upon the lungs, the heart, and upon the skull."

*First,*—as to the lungs. According to the experience of Tardieu, we do not very frequently find that aspect to which we have in a general manner been accustomed to attribute asphyxia.

They are, for the most part, not remarkably voluminous—of a roseate colour—sometimes even very pale—sometimes exhibiting a little engorgement towards their base and posterior borders.

" But whatsoever may be their colour and degree of sanguineous engorgement, one finds, in cases of death from suffocation, upon their surfaces small stains (*táches*), of a very deep, almost black-red colour, the dimensions of which vary from the size of a pin's head to that of a lentil, in the lungs of a new-born infant, and in the case of an adult, although somewhat larger, preserving the same relative proportions.

Their number is as variable as their size—sometimes five or six, sometimes thirty, and sometimes they are so numerous as to give the lung the appearance of granite. More rarely they unite amongst themselves, and form marbled patches. In all cases they are very exactly defined, and their colour is very precise and distinct from that of the neighbouring parts—trenching more or less decidedly upon the general tints of the lung. These stains are formed by small sanguineous effusions under the pleura, and their situations are as various as their number, being most frequently, nevertheless, found at the root of the lungs.

One fact most remarkable, and altogether exceptional, but which, notwithstanding, is not the less worthy of attention, is that, in so far as these appearances relate to new-born infants, they are only to be found in lungs which have functioned in a complete manner, and upon which docimastic experiments have put beyond a doubt the penetration of air into the respiratory organs.

It is also deserving of notice that these sanguineous extravasations are the most marked when death by suffocation has been most rapidly induced; and, on the contrary, when the passage of air into the respiratory organs has been less decidedly interrupted, the lungs are generally more engorged, and the stains relatively paler, but still very abundant and distinct, although less decidedly so in reference to the violet colour of the lungs.

*Second,*—as to the Heart. Ecchymoses are developed under the pericardium, principally about the origin of the large vessels. These are altogether similar to those which exist under the pleura (*pulmonalis*). Whilst this sign is not quite so constant as in the lungs, it nevertheless seldom fails to be exhibited in cases in which the pulmonary lesions are fully developed.

*Third,*—as to the Head. Here are not only to be found that "asphyxial" sanguine engorgement which is directly allied to that of the lungs in cases of strangulation or hanging; but in death by suffocation the prominent fact is the formation, under the integumental coverings of the skull, of " punctuated ecchymotic stains,"—" very limited sanguineous extravasations disseminated upon the cranial vault," not in the thickness of the hairy scalp, but in the periosteal cellular tissue. This is still the same lesion, of the same symmetry and the same nature, as that which exists upon the surfaces of the lungs and the heart.

Dr Tardieu illustrates these facts by many cases and experiments. He has observed them in many new-born infants; in an infant of three months; in a girl of 11 years, and in repeated experiments upon adult animals in which death was caused by suffocation produced by various forcible means.

I will not prolong these important extracts from Dr Tardieu's interesting memoirs, but refer you and your readers to the original, which is well worthy of the careful perusal and notice of medical jurists in this country, but will conclude by reciting the only case which has enabled me personally to verify the importance of the signs detailed by Tardieu as probative of death by suffocation.

About four years ago, a married female, who had been deserted by her husband, was delivered, in the Walsall Union Workhouse, of a stout, well-made, and healthy child. Previous to her confinement, in expressing herself resentfully towards her husband to some other inmate, she made a remark to the effect that she wished she could be rid of his child as well as of himself; and that as she had been left by him, she would not be burdened with his child. After her delivery the child was properly attended to by the nurse; fed, and put to bed to the mother. It was placed under the bed-clothes, apparently sufficiently supplied with air, but had a *calico sheet* loosely over its face. It seemed comfortable, and in a short time both it and its mother seemed asleep. In three or four hours afterwards, the mother awakened and found the child lying at her back, in close contact with her person, the sheet damp, and fallen closely around its face; and it was quite dead. An alarm was made, and the general opinion of the inmates was that the infant had been "overlaid by the mother," whilst those who were privy to her statement above detailed, considered that she had actively compassed the child's death.

My own theory as to the cause of death was, that the child had been suffocated by an occlusion of the air-passages, and thus deprived of sufficient air to support life. And in this way. The mother had perspired rather freely, and the calico sheet, which had loosely covered the child's head, had been, by the moisture thus generated, and by that of the child's breath, rendered at the same time both limp and more impervious to air: thus it had fallen close upon the child's head and face, and confining the exhalations from the mother's surface and the child's lungs, and excluding access of fresh air, it had soon acted as an "occlusion to the air-passages" of sufficient intensity to produce death by gradual suffocation. An inquest was held upon the body, and, in a post-mortem examination performed some thirty-six hours after death, the following facts were noticed:—Body well developed, of full size; countenance pale, tranquil, slightly tinged, of a leaden hue; expression of face happy and almost smiling; no external mark of violence; the mouth, and nostrils, and trachea free of any obstruction; no marks of adhesive matter about the mouth or nostrils. Upon opening the chest, the lungs were found engorged, of a reddish-brown colour. Particularly on the back parts on both lungs, but more numerous on the right, there were many ecchymosed punctuated dark-brownish red stains, in size from one-eighth of an inch in diameter down to a pin puncture. On the heart, six or seven similar stains, but none so large as the largest on the lungs. The hairy scalp was not removed, as time pressed, and these appearances were, in my opinion, taken with the history of the case, sufficient to justify the opinion I gave that death was caused by accidental suffocation.

Although having no necessary connexion with Downie's case, I cannot but point to this cause of death as probably explaining the cause of many of the deaths attributed to "overlaying infants," which *must be* but seldom, unless from the insensibility caused by intoxication or narcotics.

<div align="right">JNO. BURTON, M.D. Edin.</div>

---

## NOTE ON THE RINDERPEST.

### By Dr HJALTELIN, Reykjavik, in Iceland.

" I AM sorry to hear of the cattle disease in your country. I know very well that the ravages of that dreadful epizootic may be frightful, and of the most serious consequences. During the fifteenth and sixteenth centuries it raged in our country; and its ravages seem to have been very great, according to the statements found in our annals. No doubt, this disease then arose from bad treatment of the animals, and so it is also most likely in your native country. Nevertheless, it may also be propagated by contagion; for certainly there is not a single epidemic amongst men, or epizootic amongst animals, which may not become exceedingly contagious, although it has taken its origin

from bad treatment,—viz., unhealthy food, confinement, bad water, with a certain, still little known, atmospheric influence.

"But, at all events, I think that the contagion may be effectually counteracted by strong disinfectant compounds or remedies; and it is really a wonder how slow physicians and veterinary surgeons are in adopting this principle.

"Quarantine may often be used with considerable success, but it is, I think, very difficult to enforce it to its full extent where many cattle are already affected."

## UNIVERSITY OF EDINBURGH—NUMBER OF STUDENTS.

THE Winter Session of the University of Edinburgh commenced on Wednesday, the 1st November.

The number of students matriculated at this date (23d November) is 1391. They are distributed as follows among the different Faculties:—Arts, 645; Medicine, 411; Law, 278; Divinity, 57. Last year at the corresponding date the total number of matriculated students was 1359, distributed as follows:— Arts, 627; Medicine, 426; Law, 244; Divinity, 62.

## UNIVERSITY OF EDINBURGH—ELECTION OF LORD RECTOR.

ON Saturday, the 11th November, the election of Lord Rector took place. For some days previously an active canvass had been going on between the supporters of the Right Honourable Benjamin Disraeli, and Thomas Carlyle, Esq. At the close of the poll, Mr Carlyle was found to have been elected by a large majority; the numbers being—

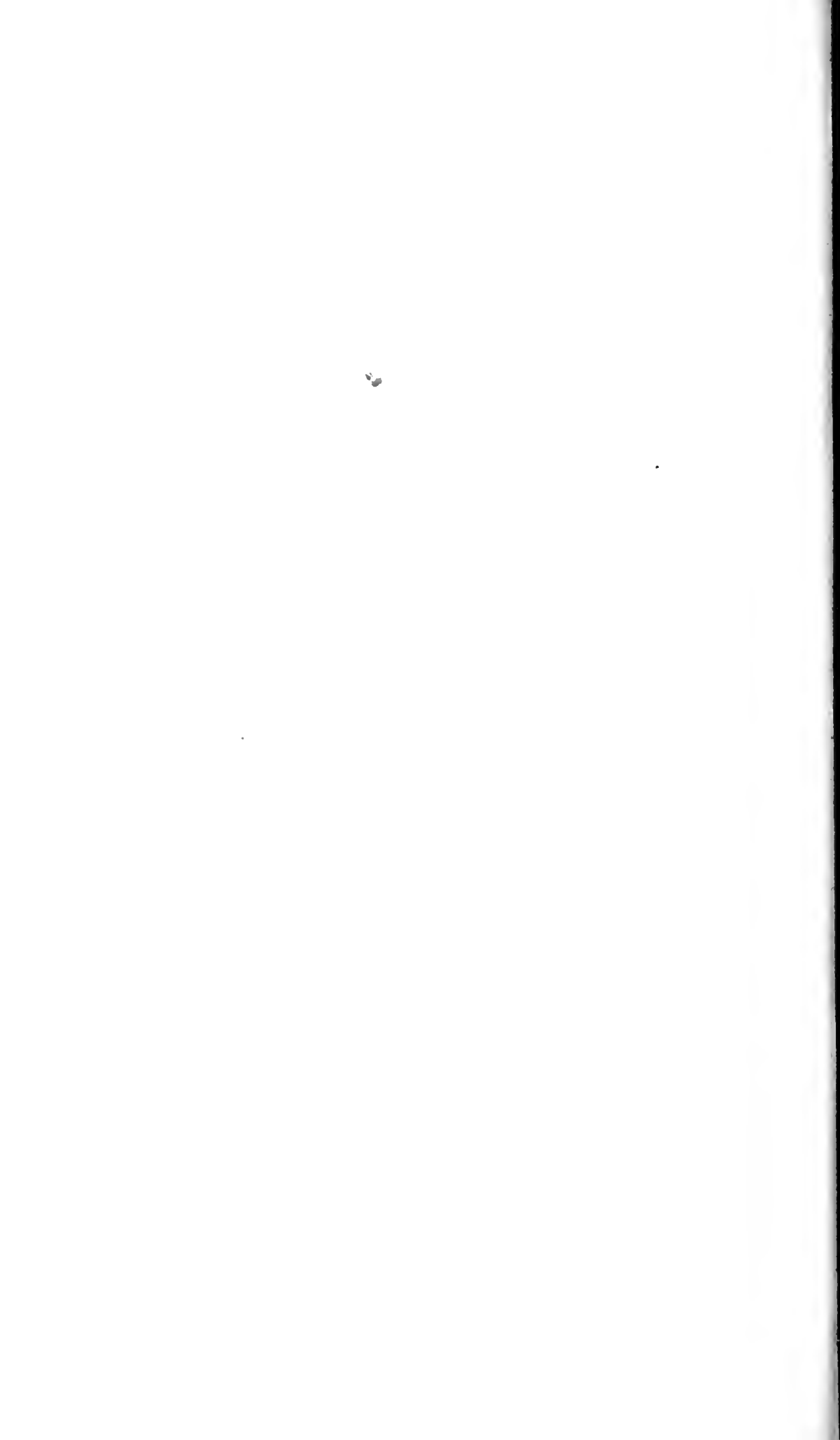| | | | | | | |
|---|---|---|---|---|---|---|
| Carlyle, | . | . | . | . | . | 657 |
| Disraeli, | . | . | . | . | . | 310 |
| | | | | Majority, | | 347 |

## MEDICAL SCHOOL, SURGEONS' HALL.

THE Medical School, Surgeons' Hall, was publicly opened on Tuesday, the 31st of October. Dr Burt, President of the Royal College of Physicians, and Dr Dunsmure, President of the Royal College of Surgeons, were present and took part in the proceedings. The opening address was delivered by Dr Littlejohn.

## PUBLICATIONS RECEIVED.

Anderson,—Handbook for Yellow Fever: describing its Pathology and Treatment. By Thomas Anderson, M.D., etc. London, 1865.

Bullock and Vacher,—Fresenius's Quantitative Chemical Analysis. Edited by J. L. Bullock, F.C.S., and Arthur Vacher, F.C.S. London, 1865.

Earle,—Flooding after Delivery. By Lumley Earle, M.D., etc. London, 1865.

Farre,—Pareira's Manual of Materia Medica and Therapeutics. Edited by Frederick John Farre, M.D., etc. London, 1865.

Gallavardin,—Les Paralysies Phosphoriques. Par le Dr Gallavardin. Paris, 1865.

Grove,—Epidemic Cholera and Diarrhœa; their Prevention and Treatment by Sulphur. By John Grove, M.D., etc. Third Edition. London, 1865.

Guy's Hospital Reports. Edited by Samuel Wilks, M.D. Third Series, Vol. XI. London, 1865.

Howe,—Theoretical Inquiry into the Physical Cause of Epidemic Diseases. By Alexander H. Howe, M.D. London, 1865.

Moore,—The Antecedents of Cancer. By C. H. Moore, F.R.C.S. London, 1865.

Roberts, — Practical Treatise on Urinary and Renal Diseases. By William Roberts, M.D., etc. London, 1865.

Swan,—The Means employed for Correcting the Inverted Image on the Retina of the Eye. By Joseph Swan. London, 1865.

Tucker,—Nature and Treatment of Cholera and Fever, etc. By James Tucker, M.D. Dublin, 1865.